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THE  
BRITISH AND FOREIGN  
MEDICAL REVIEW,  
FOR JULY, 1846.

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PART FIRST.

Analytical and Critical Reviews.

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ART. I.

*Die Hauptformen der Selenstörungen in ihren Beziehungen zur Heilkunde.* Von Dr. MAXIMILIAN JACOBI.

*The principal Forms of Insanity in relation to Treatment.* By Dr. MAXIMILIAN JACOBI.—Leipzig, 1844. 8vo, pp. 822.

OF the continental writers of the present day on the subject of insanity, few, if any, have acquired a higher reputation than Dr. Maximilian Jacobi. Although perhaps his opportunities for observation have not quite equalled in extent those afforded by the larger lunatic hospitals in England and France, yet so much accurate discrimination has been exercised in the still numerous cases which have fallen under his notice, and so much practical knowledge has been displayed even in some of his earlier works, that he may, without doubt, be looked upon as one of the highest authorities in all questions relating to disorders of the mind.

The lunatic asylum of which he is the director is at Siegburg, situated about five miles from the town and university of Bonn, on the eastern bank of the Rhine. It is devoted to the accommodation of insane patients from the Prussian Rhenish provinces. Jacobi's first work was entitled, 'Collections for the Treatment of Disorders of the Mind' (*Sammlungen für die Heilkunde der Gemüthskrankheiten*), and consists of two volumes. The first, published in 1822, contains an account of "The Retreat" near York, a translation of Mr. Samuel Tuke's work; and the second division, a translation of various articles respecting insanity, contributed by Esquirol to the 'Dictionnaire des Sciences Médicales,' never collected in the French language until published shortly before his death by the author, in his last great work, entitled 'Traité des Maladies Mentales,' &c. The subject of the second volume is 'Psychical Phenomena, and their relation to the system in a healthy and diseased state.' Here we have

some account of his physiological theory concerning the relation of the brain and of the system in general to the mind, which, in the course of his other works, he more fully develops. In 1830 Jacobi published a work, called 'Observations concerning the Pathology and Treatment of Diseases connected with Insanity' (Beobachtungen über die Pathologie und Therapie der mit Irreseyn verbundenen Krankheiten), the most important and valuable part of which consists of a series of very interesting cases, related with great minuteness. In the former part of this book, after some introductory remarks, he brings forward a theory, in which, however, he is far from being peculiar, viz. that there is no disease of the mind existing as such, but that insanity exists solely as the consequence of disease, either functional or organic, in some part of the bodily system. The third section is taken up with critical remarks upon the works of Esquirol, Georget, and Guislain; and in the fourth we have some imperfect statistical account of the insane in the Prussian Rhenish provinces. The number of lunatics is computed to be 1 in 1000 inhabitants. The remaining two-thirds of the work are devoted to the description of twenty-seven cases of insanity of various forms, interspersed with the conversations and writings of the patients themselves, both in prose and poetry.

In 1838 the first volume of the 'Journal for the Diagnosis and Cure of disordered Conditions of the Mind' (Zeitschrift für die Beurtheilung und Heilung der krankhaften Seelenzustände) was edited by Jacobi and Nasse of Bonn, and published at Berlin, containing a series of essays upon subjects connected with insanity—cases, prophylactic treatment, a description of English asylums, &c., by the editors, and by Drs. Flemming, Jesse, and Zellen, who are directors of lunatic establishments in Germany. The English asylums, his visits to which are described in this Journal by Jacobi, are Hanwell, St. Luke's, New Bedlam, Maidstone, Oxford, Stafford, Lancaster, Liverpool, Wakefield, and the Retreat near York. The construction of these houses is a subject to which considerable attention has been paid by the author. Besides the translation of Mr. Tuke's description of the Retreat, inserted in his 'Sammlungen,' Jacobi afterwards wrote a separate work, laying down principles and directions for the erection of suitable buildings for a hospital for the insane, with a particular notice of the establishment at Siegburg, of which he is the director. This treatise has been translated from the original language and published with introductory remarks by Mr. Samuel Tuke. It is unquestionably the best work extant on the construction of lunatic asylums.

The present work of Dr. Jacobi will, when complete, consist of three volumes, only one of which is as yet published. It is divided into five sections:—1st, a series of cases; 2d, an account of the pathological phenomena which occur in diseased conditions connected with mania; 3d, the causes of mental disorders having the character of mania; 4th, the course of diseases connected with mania; and 5th, the treatment of mental diseases. In his introductory remarks we find the following classification of the various forms of insanity.

I. Alienation of the appetites or propensities, comprising a state of exaltation, being *mania*, and a state of depression, being *melancholia*.

II. Alienation of the intellectual powers, subdivided into *illusion* and *dementia*.

III. To these two classes we have a third added, which also comprehends two divisions, viz. *delirium* and *idiocy*, forming altogether six species of insanity, which he proposes to describe. This first volume is devoted to the consideration of mania alone. The second will contain melancholia and illusions: the third, the remaining forms of insanity.

In its general plan the volume before us bears considerable resemblance to the celebrated work of Louis on phthisis. The inferences and observations in the second part are founded upon a strict examination of several accurately recorded cases, which are given in detail in the first part.

The cases are separated into three groups, which are thus arranged:

1st. Cases of mania (*Tobsucht*) without illusion (*Wahnsinn*), with but little disturbance of the intellectual powers.

2d. Cases of mania with delirium or incoherence (*Bewirrtheit*) without illusion.

3d. Cases of mania with illusion, being subdivided into two sets—mania in one and illusion in the other class being the predominant feature.

All the cases, with the exception of some which are called "Outlines," are drawn up with that accuracy and attention to minute details in their histories which is the result of the admirable system of clinical instruction prevalent in most German medical schools.

Before we pass on to notice the second section of this volume, in which are contained the author's remarks and inferences from the fifty cases which he reports, it will be worth while to extract one from each group, to assist us in understanding the succeeding parts of the work. These—and the others are like them—will probably startle and provoke our readers, from their astounding lengthiness; but we prefer giving a few of these cases in all their fulness, to abridging many of them, as it is on the fidelity of his individual portraits that the author must be judged. Unquestionably many of the details are unnecessarily minute, and a careful revision of the MS. might have saved the reader from not a little unnecessary diffuseness and repetition. The cases, however, as they are here delineated, will afford valuable studies to the practitioner; and, we doubt not that their perusal is capable of supplying important hints to most of our clinical physicians that have to deal with insane patients.

In the division of cases exemplifying mania without illusion, the first is interesting, as it gives us an example of the disease in a low stage of its development, and serves to illustrate the accuracy with which the commencement of the disease is investigated. We give it in full:

"CASE.—J. R., a man of strong bodily constitution, had just passed his 37th year when he was brought, in September 1839, to the asylum. He was born of healthy parents, in good circumstances, who were remarkable for their intelligence and good dispositions, and no other case of insanity was known in the family. The patient also in his early life, although an unruly boy, had shown proof of excellent abilities, and was on that account intended by his parents for some literary occupation. After having been sent to school he soon became disinclined to this mode of life, and discovered a strong inclination for farming, was fond of horses and dogs, and delighted in hunting. As his father possessed considerable landed property, and his brothers had devoted themselves to other pursuits, it became easy for him to follow his inclination, and he had acquired so much skill that the whole of his paternal estate was left under his care, and improved yearly by his exertions. Until his twentieth year his health had been apparently good. In that year he



suffered an attack of insanity, which continued for six weeks, and this recurred in a similar way seven times, with intervals of two or three years. The progress of the attack was every time as follows: At first the patient became for a short period dull and depressed, and this state was followed by a continually increasing excitement, which in its highest degree only manifested itself in an excessive eagerness in following his ordinary pursuits. His love of horses and dogs, and of hunting, was very much increased, as was also his activity in business. He was in the greatest degree enterprising and full of projects, and restless in every thing which he took in hand, performing with his own hands the hardest agricultural labour from morning till night; showing, moreover, an immoderate degree of self-confidence with a tendency to bursts of anger, and an evident weakness of judgment at the same time, avoiding the society of his family, and conducting himself towards them in an unusually repulsive and contemptuous manner. At these periods he usually spent sleepless nights, without any decrease in his overstrained activity during the day. His appetite was in a like manner increased, and whilst he was generally (except during the attacks of violence) very moderate in the use of spirituous drinks, he now showed a great inclination for them, without, however, becoming intoxicated. During the attacks no decided disorder of the intellectual powers ever occurred, although transient illusions were remarked. Altogether, his insanity especially manifested itself in an exaltation of all the active powers of the mind, and, above all, in an impulse to vehement expressions of the will and propensities. When, after the lapse of four or five weeks, this condition had reached the highest pitch, then the excitement speedily subsided, and he returned to his usual physical state only after an interval of a few days of depression corresponding to that with which the attack ordinarily commenced. The patient himself, even during the continuance of his attacks, had a consciousness of some disordered condition, and was always depressed and anxious after they had passed off, and several times expressed a wish to be brought to this institution to undergo treatment; and at last gave it as his decided wish, as the attack before the last had been more violent and obstinate than those which had previously occurred, that upon the recurrence of the disease, even if at the time it should be against his will, his friends should bring him hither. This wish was acted upon. But as his friends were convinced that during the attack he would not submit to the measure, it was performed with the assistance of an officer of the police; at which he was very discontented, pretending that he would have gone willingly; nor could he for a long time forgive his brothers for their share in this proceeding; yet he evinced no particular dislike to the establishment, and was willing to follow the directions there laid down for his treatment. Moreover, upon his arrival the attack had already nearly reached its highest point, and the period of decline soon began when after the usual depression he regained his healthy state. The question now was, what plan was to be adopted in order to secure the patient from future attacks of a like nature?

"It was impossible to doubt that the tendency to these morbid phenomena which developed themselves during the attacks, as well as the attacks themselves, had their origin in a disordered exaltation or irregular excitement of the brain and whole nervous system. But from what source did this change arise, and how was it maintained? We found after pressing inquiries that this individual, so powerful and leading so active a life, had been from his boyhood up to his present adult age excessively addicted to masturbation. Being continually tormented by his conscience, and even full of anxious thoughts lest the attacks of disorder of his mind should be caused by this vice; always making new resolutions to renounce it, and after a short period of determination, during which he kept his resolutions, breaking through them; always driven to doubts from his weakness, and at the same time incapable of summoning strength for a continued renunciation; always feeling himself struggling to avoid this frightful precipice, and driven to it again, his mind suffered no less than his body, and the disordered condition of his brain was no doubt the result.

"The patient, who was now convalescent from his mental disorder, willingly acquiesced in a plan of treatment which was to emancipate him from an evil which always threatened him, and which was so much dreaded. A spare diet, avoiding soup and meats difficult of digestion, especially in the evening, a cool regimen, continued active employment in the open air, sleeping in a "camisole" near the attendant, general cold baths with cold douches on the perineum, the internal use of iodine, and lastly, means calculated to strengthen his moral power, and revive his courage to contend against the propensity which had overcome him, were the chief means adopted to subdue so destructive and deeply-rooted an evil. And these means, with the co-operation of the patient, did not disappoint our expectations. He preserved himself free from vice during several months. But as in this complete and continued state of psychical good health he could no longer be kept in the establishment, and as he himself wished to return to his business, where he could not conveniently be spared, he left Siegburg at the end of May, cheerful and with the most earnest intentions to behave himself continently. No further account has been received of him." (pp. 1 et seq.)

The case is not without interest, but it scarcely amounts to what in this country would be called mania. The propensity to drink showed itself during the attack, and was not habitual with the patient; whereas the other vicious habit is said to have been the cause of the disorder. In many cases when these depraved propensities exist, it is extremely difficult to ascertain whether we are to look upon them in the light of cause or effect.

To illustrate the second group, or those of mania with delirium or incoherence with illusion, we extract case 20 :

"J. M., a countryman, 52 years of age, well made, lively and excitable, was brought to this establishment in July. He had been engaged in the expeditions of the French into Spain and Russia from the year 1806, and had been addicted to great excess in drinking and debauchery; had been repeatedly infected with syphilis; and after narrowly escaping being drowned in the Beresina, lost the ends of his toes from the cold. At the end of the war he married, and lived quietly at home. He was, at the time of his arrival at the asylum, suffering from an illusion with a considerable degree of excitement, believing himself to be in the possession of great riches and of a high station, and busying himself with continually changing plans for employing his property, &c. Yet his ideas of wealth never became perfectly boundless (which we have frequently observed to be a symptom of incurable organic degeneration of the brain), and he recovered in a few weeks; so that at the beginning of September he was able to return to his home. In the following year, about the season of his first attack, after continued hot weather, the disorder of the mind recurred, having now the character of mania with illusion. In the first days of July he complained of violent beating and noise in the head, but until the 6th he showed no sign of insanity. On this day, after he had drunk an excessive quantity of brandy, and eaten about three pounds [!] of meat, the disorder of the mind showed itself with such violence that it was found necessary after two days to bring him back to the asylum.

"The patient was in good bodily health; the conjunctivæ were reddened; the look changeable, lively and gay, his eye bright, with a fixed, contracted pupil, his chest moderately expanded; a closer examination detected slight dilatation of the bronchial tubes, and hypertrophy of the left ventricle. The viscera of the abdomen showed no sign that should lead us to suspect any particular disease; his appetite was very good, tongue moist and almost clean, and the bowels acted freely. The powers of voluntary motion and the functions of the organs of sense were unimpaired. Common sensation was also unaffected, with the exception of a transient pain in the head. Sleep was almost entirely wanting. The pheno-

mena of mental disorder were the following: The patient continually talked of military affairs, at one time with loud shouts giving the word of command, swearing, and calling to generals and officers known to him in the service, &c. ; but during this violent maniacal excitement no single idea was continually present; the pictures in his mind appeared rather to rise as involuntary reminiscences than as the effect of an illusion. He was moreover dirty, indecent, shameless, and often very violent, his violence being directed rather against inanimate objects than his neighbours, towards whom he at the same time behaved himself in a friendly and even jocose manner; as, for instance, once when he managed to free himself from his strait-waistcoat, he quickly put it upon an idiotic patient who was near him, and led him to the attendant. There was a continual change from paroxysm to remission, neither of which often extended beyond ten or twelve hours, the former prevailing generally throughout the night. In the progress of the disease for the first two months the paroxysms continued to increase in length and intensity, and then became somewhat shorter and less severe, although he had occasional attacks of the most violent nature. The pulse was very changeable in frequency and quality from the commencement, the difference not being caused by more intense maniacal excitement or remission, nor did it correspond with those different states. In point of frequency it was seldom under 80 beats, often being 92-96, but only occasionally reaching 100, and this generally during the period of decided remission, there being at the same time very little force in the artery. The tension and impulse were much less in the radials than in the carotids, which generally beat very strongly, whilst at the same time all the veins of the neck were turgid and pulsating, owing without doubt to the existing disease of the heart. For two days only, during the exhibition of the infusion of digitalis, the pulse sank to 66, and in one day, when the patient was unusually quiet, to 60: but on the next day, under the continued use of the same medicine, and whilst the patient remained in the same tranquil state, it rose to 90, and in the following day, under great maniacal excitement, it fell again to 68, 60. The secretion of saliva was very copious, the bowels were daily moved, his tongue was clean and moist, and his appetite was always very good, except during a few days, in which it appeared to be lessened by the use of digitalis.

"The patient was treated upon the principle that his brain and nervous system, from the first very irritable, after having undergone, together with his whole system, destructive influences of various kinds—such as the effects of climate, repeated attacks of syphilis, and the means of their cure—were now suffering from the consequences of a repeated and continued over-excitement, with its still persistent high degree of irritability, and also from irritation of the nervous ganglia of the abdomen, which were in a similarly disordered state and from irregularities in the circulation of the blood consequent upon disease of the heart. This treatment (as was the case almost without exception in all the soldiers who had taken part in the French, Russian, and Spanish campaigns, and in whom insanity had appeared in this form) was entirely ineffectual. Towards the end of September a general affection of the system appeared, followed by œdema, ascites, and hydrothorax. All means which were adopted proved of no service in averting the continual progress of the disease, and he died upon the 2d of January—that is, just half a year after the second attack of insanity.

"At the commencement of the more severe bodily diseases all the signs of mania disappeared, and the patient became more sensible; so that during the last weeks of his life he might be considered as tolerably free from insanity, although his intellect was somewhat weakened. It was observed that he was sometimes delirious in the night, but this had no evident connexion with his insane condition." (pp. 172 et seq.)

A minute account is given of the morbid appearances on dissection. We will notice only those of the head:

"The scalp was oedematous and full of blood upon the left side; skull cap of moderate thickness and density, saturated with blood upon the left side. The dura mater firmly adherent to the skull, thickened at the falx major, and adherent to the arachnoid and pia mater, in which some serum was infiltrated. The substance of the brain was in the upper part of the hemispheres of moderate consistency, very much softened at the fornix, the third ventricle, and the posterior cornua of the lateral ventricles. Some fluid was found in the left lateral ventricle. The commissura mollis did not exist. The corpora quadrigemina appeared somewhat roughened. Traces of softening were found in the fourth ventricle." (*Ibid.*)

Cases of mania with illusion are divided into two classes: those in which the maniacal symptoms predominate, and those in which illusion is the most prominent character. To illustrate the first let us extract the 44th case, or rather a part of it; for it is so very long and minute that we fear no English reader would peruse it in full:

"CASE.—J. H. R., a young agriculturist, was brought to the asylum, labouring under mania, three times in the course of seven years, and recovered completely each time. The first attack took place in August, 1828, when he was 23 years of age. He was a well-made young man, whose external appearance seemed to promise continued good health. In his childhood he had shown himself at least of moderate intellect, and, as far as could be discovered by inquiry, no case of insanity had occurred in his family. Three years ago, however, he had been ill for three weeks with a nervous fever, attended by obstinate delirium; and it appeared that from that time a diseased excitement of the brain existed: his temper during the same time had been very irritable. After his recovery from the fever he had been considered to be in perfect health, and had since devoted himself continually to his employments with great activity. In June, 1828, in the middle of the day, as he was returning home in company with many other young men, who with him had been subjects of the conscription, he suddenly became violently excited, loaded his companions with unfounded reproaches, and behaved himself altogether like a man who had lost his reason. During the five following days he remained in the same condition, some hours, however, passing in which he was perfectly sensible and quiet. On the sixth day he lay in bed, said that he was ill, and had continued illusion. His face was red and swollen, and he had great thirst and disinclination to take food. The disease during the following days became more serious, with continued fever, muttering, delirium, and a dark brown coat upon the tongue. An experienced physician was called in, and upon the administration of the remedies he so quickly improved that upon the fourteenth day he could leave his bed, and appeared quite sensible, complaining only of lassitude and want of appetite. After the lapse of eight days his face became again flushed, his conversation was irrational, and violent attacks of mania followed, returning after a certain remission, but preceded always by a considerable flushing of the face. He became quite ungovernable, so that it was necessary to keep him constantly under restraint, and in this state he was brought to the asylum on the 28th of August. He continued some days in the same condition, his nights being quiet. The pulse was 80-85, his face flushed, temperature of the body heightened, especially in the palms of the hands, his appetite good, and digestion unimpaired. After the first part of September the paroxysms became gradually weaker; he was quiet and obedient, and began to employ himself, but he was still mad to a certain extent. In the second half of the month he became again much more violent and troublesome, so that he was obliged to be strictly guarded; the pulse showing no great tension, and amounting in frequency scarcely to 75, afterwards to 66, whilst the temperature of the skin was natural, the face being at the same time redder than usual. These exacerbations soon passed off again, and in October the state of his mind became quite natural, and remained so; and after the

lapse of a few months of trial he left the asylum at the end of March, 1829, perfectly well. No critical secretion was perceived upon his convalescence, but he was decidedly stouter and more healthy in appearance than before the commencement of the disease.

"R. remained well until the end of February, 1833. At this time he began to suffer from cold feet, heavy sleep, and constipation. On the evening of the 22d he was found kneeling in prayer in the open street; and upon returning home he complained of a noise in the head, and expressed a fear that he was again about to be ill. Five days afterwards he had another attack of mania, in which the face was very red; he cried out violently, passed his evacuations involuntarily, and talked much of God and the saints. After the abstraction of some blood he became quiet, and continued so for the five following days with alternations of mania, and from that time till the middle of March he was tranquil, when he returned to his occupations; but upon the 16th the disease returned with such violence that he was hurried back to Siegburg upon the 19th.

"On his arrival his skin was dry and of a yellowish gray colour, his head hot, and extremities cold; he said that he was free from headache. Digestion was natural, the function of the lungs was properly performed. The first eight days were spent in the most violent state of mental excitement, without his taking any fluid whatever; he slept during the night generally undisturbed. Occasionally illusion manifested itself, and he declared that he was St. John, and that he would be so styled; this, however, was not very permanent. The disordered condition, as in the former attack, was considered to be a greatly increased irritability of the brain, combined with weakness, and accompanied by an excited state of the vascular system, by which the irritation of the brain was produced. The following observations were made respecting the further course of treatment of this attack:

"On the 20th of March the patient was flushed, his head hot, he was covered with perspiration, his feet cold, pulse full, 100, 105 in a minute, that in the carotids not being unusually strong; he was turbulent and noisy, but upon the application of cold to the scalp, and mustard baths to the feet, he became tranquil after a few hours; the temperature of the scalp was lowered, the pulse was slower, small, and weak, the respiration quiet and gentle, and he passed a quiet night. On the 23d he was again excited and greatly disturbed, he spat in every direction around him, and licked up the saliva of the other patients. He was confined on the chair, cupped upon the back of the neck, cold applications were made to the head, and he was given dilute sulphuric acid with water. His condition changed continually from moderate excitement to tranquillity until the beginning of May, when the flushed and heated state of his head accompanied another attack of violent mania, and continued in a greater or less degree throughout the summer. Digitalis was now given in the form of powder, from  $\frac{1}{2}$  to  $1\frac{1}{2}$  gr. four times daily for two months, and at the same time shower-baths were used. The pulse was very changable during the use of this remedy, 95, 100, 140, 110, 120, 78, but at last, on the 6th of July, it sunk to 43 beats, and on this day the patient was particularly violent and noisy. When most frequent the pulse was altered in quality, but not depressed; when least frequent, it was smaller and softer. Digitalis was now omitted, and the treatment was confined entirely to an unstimulating diet, and a corresponding moral regimen. The psychical condition remained unaltered, so that the maniacal excitement continued through August, the pulse differing at different times, not exceeding 70 beats, sometimes being as low as 48. In September the pulse was more steadily low, and the patient, whose illusion with the mania had remained unaltered, although the former showed no influence upon the latter, began to be more quiet and manageable; his convalescence then advanced so rapidly for a few weeks that at the end of the month he could be considered well, and no unnatural condition of any organ could be discovered, except the slowness of the pulse, which was 55, and which by very slow degrees returned to the natural state, which in this individual was 75-78. He was retained during the winter in the asylum,



to preserve him if possible from a relapse, and from the use of intoxicating liquors, which were so dangerous to him, and in the beginning of March he was dismissed.

“Twelve months afterwards he was taken ill in a similar way. On the 15th of March (until which day he had remained in perfect health) he complained that “his skin had adhered to his body,” and a few days afterwards he considered himself a rich gentleman, and his brothers his servants. He became then suddenly maniacal, and tore and destroyed every thing he could lay hold of. He was confined, became quiet, and slept during the night, showing no trace of mania for fourteen days, although he still considered himself a rich man, and would not trouble himself by doing any work. On the 30th of March he had another attack of mania, which compelled his friends to bring him again to the asylum, where he arrived on the 2d of April. Upon his arrival he displayed great maniacal excitement, cried, spoke abusively, tearing his clothes, menacing us with blows, &c. He again had the illusion that he was St. John. After a few days the excitement gradually abated, returning again speedily; and this change continued with short intervals for six months, whilst the illusion was always present in the same way, although it exerted no evident influence over the paroxysms of mania. The attacks during this illness were slighter than in the earlier ones, so that they only showed themselves in excitement, and not as real outbreaks of fury. The temperature of the head and the colour of the face were much heightened, and the patient appeared always more confused after he had warmed himself with exercise. Digestion was unimpaired, his tongue clean, appetite good, and the bowels were in a natural state. As our former view of the cause of the disease might be looked upon as well founded, the treatment in this attack remained on the whole the same. At first tartar emetic was given with nitre, and from the end of June to the beginning of September digitalis, in the form of acetum digitalis, in increasing doses afterwards in conjunction with aqua amygdal. amar. At one time twenty leeches were applied to the arms, and afterwards twelve cupping-glasses to the back of the neck, and for a long time daily for several hours cold applications to the head. The frequency of the pulse varied this time also under the use of digitalis, but not to so great an extent, as it never exceeded 100, and only once sank below 70. We not unfrequently observed great tension of the arteries with fulness, but without any real strength; it preserved in the carotids its natural relation to that in the radials.

“The maniacal condition, which had continued in the above-mentioned form during the whole of the summer, began to yield gradually to this treatment at the end of August, and disappeared entirely in September.

“From this time R. appeared to be in a perfect state of physical and mental good health; but he wished to remain longer in the asylum for fear of another relapse, and therefore applied to be made attendant—a request which was readily complied with, as, during his convalescence, he had shown himself to be very sensible, attentive, and careful. He remained in this situation without a return of his illness for nearly three years, and then went away to marry and keep house for himself.” (p. 273.)

The last case (48) which we extract will illustrate the second subdivision of the third order, viz., mania connected with illusion, where the illusion predominates:

“CASE.—N. J., 40 years of age, had served as grenadier under Napoleon for ten years, and had received many wounds. He afterwards had some small civil appointment, and married a woman much younger than himself, with whom he lived peaceably, and had eight children. But now his income was insufficient for his increasing family, and he was tempted by want to appropriate a small sum that had been intrusted to him; and not being able to replace it, became melancholy, and suffered from extreme anxiety and restlessness. Not long afterwards,

in the month of May, he began occasionally to act in a foolish manner, to talk unconnectedly, and to scream out in the night. Instead of the sleeplessness which had lasted to the present time, he suffered from exceeding drowsiness, a feeling of depression, and at the same time an enormous voraciousness. This condition having lasted till the middle of the summer, a continually increasing excitement was observed, and at last, about the middle of September, his wife was obliged to call in medical assistance. The patient now manifested an unusual degree of impetuosity, rashness, and hastiness in all his movements. He himself complained of extraordinary disquiet, oppression of the chest, faintness, headache, and unrefreshing sleep, much disturbed by dreams, he said he had no peace or repose at home, and even his old wounds in his head and arms began to smart. The pulse was at the same time full and hard, his countenance unusually red and swollen, with spasmodic twitching of the eyelids, the conjunctivæ traversed by enlarged vessels, a wild look, voracious appetite, bowels confined, tongue but little coated, skin dry, and its temperature natural. According to the account given by his wife, he had slept but little during the last six weeks, was quarrelsome, and did not as usual amuse himself with his children. After two days he became much worse; and being quite sleepless he leaped out of bed in the night, seized his wife by the hair, dragged her about the room, and attempted to strangle her. At the same time his conversation was indecent, he abused his children, was dirty in his person, suffering from the highest degree of excitement of the genital system, with the greatest shamelessness, whilst his mind was filled with illusions respecting the conduct of his wife, and the persecutions to which he was subjected.

"After venesection (the blood exhibiting the inflammatory coat), the patient became for a moment quiet, and said that the oppression on his chest was lessened. On the next day, however, the morbid excitement of the mental faculties was much increased; he was more shameless and filthy, shouted, abused, and tried to injure his friends, and at the same time he was under an illusion that all men wished him ill, and tried to rob him of his bread, that he was unable to support his family, &c.

"He was brought to the asylum a fortnight afterwards, in the beginning of October. He was a tall, powerful man, with a scar, three or four inches in length, over the occipital bone, resulting by a sabre cut received at the battle of Austerlitz; the scalp was very moveable, its temperature not too high, the eye very unsteady, pulse moderate, the lungs and heart healthy, bowels regular, uninterrupted sleep during the night, and no present manifestation of mania.

"His mind was, notwithstanding, much disturbed by many illusions and discordant ideas, the influence of the disordered condition manifesting itself in his judgment, especially in the false interpretation of all the acts and words of those around him, to whom he imputed the most hostile intentions, the object of which he either was or feared to become. He was full of hatred and ill-will, mischievous and lying, and always endeavoured to present an appearance of justice and truth; he avoided all acts of violence, although sometimes a trace of furious excitement was perceived, and the maniacal element of the disease, which at first was very prominent, had now disappeared, and cowardly meanness appeared in its stead. The longer this state continued the stronger was the disorder of his imagination, and the more his behaviour was perverted. Thus he endeavoured, among other things, to remove the keys from the doors, secretly to destroy the walls and plaster, to throw away any tools or clothes which he could secrete, took the lining out of his breeches, in order to have an opportunity of complaining that they gave him no drawers, fastened his gloves beneath his coat, and cursed the attendant for exposing his hands to the cold during the winter frost, &c. He persevered in the idea of the injustice, oppression, and neglect which he daily had to undergo, and every prudent regulation which was acted upon on account of his violence merely confirmed his illusion. He seemed, however, to have occasional lucid intervals, in which he acknowledged that his conduct was



perverse, and promised not to repeat it. These intermissions seldom lasted to the next day, and were generally only the signs of a more excessive alienation. In the summer months he was somewhat better, although he still dissembled and was inclined to abuse and slander. The pulse (which was scarcely affected for two days consecutively by digitalis taken for a long time) was for the most part 80-85, frequently sharp, but without strength and fulness; the bowels were regular, appetite good, tongue clean, and sleep quiet.

"Upon the 24th of December, the patient suddenly manifested great excitement, with hastiness in his movements and incoherence in his ideas, and the following day he was perfectly maniacal. This condition, now appearing for the first time since his arrival at Siegburg, remained for eleven months stationary, accompanied with an ungovernable propensity to destruction, and ended only with the life of the patient; remission for a few days having been occasionally observed. He frequently spent sleepless nights in continual motion, screaming so violently that he was hoarse for several days afterwards; frequently, however, his nights were quiet, and his fury commenced by day. His unconnected raving bore the stamp of the most extreme impudence and indecency, and he passed his evacuations in his bed and clothes. He ground his teeth in a terrible manner, and tried to bite and tear everything that he could reach; he tore to pieces several strait-waist-coats during the day, rendering it necessary to put on a wire mask, and in the night, when his hands and arms were confined, he tore to pieces his bed and bed-linen with his feet. The pulse was changeable, 85, 90, 78, soft, and otherwise quite natural in the carotids and radials during the maniacal attacks. No increase of temperature was observed during the course of the disease.

"The plan of treatment which was pursued was quite ineffectual. Before the beginning of spring a general state of ill health, from the influence of the disordered condition of the nervous system upon the organs of physical life, came on and increased during the summer; œdema of the feet, abscesses in the extremities, cough, with copious expectoration of yellow mucus, diarrhœa, emaciation, and general weakness. He died on the 6th of November, twenty-five months after his reception into the asylum.

"On dissection a considerable quantity of serous fluid was found in the left side of the chest, but scarcely two ounces in the right, whilst about five ounces were found in the pericardium, which, after exposure to the air for a quarter of an hour, coagulated into a gelatinous mass. Considerable hypertrophy of the left ventricle, the aorta was dilated at its arch, and its internal coat, as well as that of the innominate artery, was softened with atheromatous deposits of the size of a pea in different parts. The abdomen contained some fluid; the liver was large, pale, and bloodless, and so was the spleen. In the cranium the sinuses and veins of the brain were full of black blood, the arachnoid opaque, without any thickening. The consistence of the brain was less than usual in all parts, especially in the more deeply-seated regions. The hemispheres were firmly united together above the corpus callosum for two-thirds of their length, so that it was requisite to divide them with a scalpel, in order to arrive at the great commissure. The two lateral and the third ventricles were filled with fluid." (p. 300.)

The preceding cases will suffice to display the style of Jacobi's observations, and they are worthy of note in another point of view. The frequent bleedings and the use of depressing means which were adopted in the majority of the cases, are part of a method of treatment which in this country finds at present but few advocates.

The second division of the work gives an account of the pathological phenomena which occur in diseased states connected with mania, and consists of conclusions drawn from a comparison of the fifty cases which are contained in the first division, and which are for this purpose arranged in four tables.

I. The first table gives the name, age, sex, constitution, and temperament of the patient, the number and character of the pulsations at the wrist and in the carotids, with the state of the functions of the heart, lungs, and skin during the attacks of mania, and during the remissions or the period of convalescence.

With regard to the frequency of the pulse no general inference can be drawn, although the number is accurately noted in every case; the chief peculiarity being the quickness with which variations occurred both in the number and quality of the pulsations in the carotids and radials. In certain attacks of mania the pulse mounted to 100-130, whereas in other attacks in the same individual it was even less frequent than during the remission; nor do the observations which were made in order to ascertain if any variation occurred at different times of the day give any positive result, beyond the variations caused by accidental circumstances, as, for instance, by the upright or horizontal position, &c. The cases in which the pulse of the carotid corresponded in quality to that of the wrist bore the relation of 7 to 1 over those in which the pulsation was stronger in the neck.

The action of the heart corresponds with the pulse, but by means of the stethoscope in several instances symptoms of hypertrophy were discovered during the remission, which during convalescence entirely disappeared. A similar remark is made respecting the state of the lungs, viz., that the physical signs of tubercular deposit, as mucous râle, dulness on percussion, and considerable resonance of the voice were frequently present during an attack of mania, but vanished on recovery taking place. Esquirol remarked the frequency of organic disease of the heart and lungs in cases of insanity, and Foville says that five out of six of the bodies of lunatics which he examined displayed organic disease of the heart, generally hypertrophy, but the observation of their transient nature is of great importance and peculiar to Jacobi. He accounts for these anomalies by imagining that the nutrition of the parts has been influenced and altered by the affection of the brain and nervous system. In half the cases there was no change in the temperature of the skin; but in the rest it was more frequently raised than lowered; and in the half of those in which no alteration existed in this respect, the colour of the face was notwithstanding evidently heightened.

The observations which were made in order to draw up these tables are additional to the general description of the cases, some specimens of which we have extracted, and furnish what may have been thought wanting in them, namely, a close account of the state of the different organs and their functions; and here Jacobi finds fault with other authors and observers, and remarks, that by scarcely any of them has any real attention been paid to these important particulars; that even Esquirol, in forty-five cases which are scattered throughout his great work, has given the state of the pulse only in one instance. But it is not fair to conclude from the fact that Esquirol did not publish in systematic tables like those of Jacobi the results of his experience, that his observations are not close inductions from facts actually noted down by him. Esquirol was through his life a most careful observer of facts, which he recorded with remarkable accuracy, and afterwards arranged and systematised with a rare sagacity and

insight into their relative importance. His general observations are the deductions which he himself made from a multitude of individual facts; Jacobi's method of laying before his readers in a tabular form a collection of phenomena, from which inferences are to be drawn, has a more imposing effect and taxes less the faith of his readers as to the accuracy of the inductions, but we doubt whether better and sounder opinions will be formed by many readers on the data submitted to them, than those which Esquirol offered in a tone of confidence which he felt that he had a right to assume.

It follows from the facts stated in these tables that the condition of the pulse either with respect to its frequency, quality, or the relation between the pulse at the wrists and in the neck, will afford any assistance to diagnosis in cases of mania. This negative result does not prove that it is unnecessary to examine the pulse in lunatics; it is highly important with regard to variations occurring at any particular time in any individual case, but it shows that no general rule can be laid down, and that descriptions of mania giving as a diagnostic mark a quick hard pulse, &c., are either taken from one single individual, or are merely the preconceived notions of the author.

Another important fact which Jacobi upholds in opposition to the opinion of many writers is, that a febrile state, or pyrexia, is not the usual concomitant of a maniacal attack; and as he gives us the numbers, the accuracy of which we have no reason to doubt, he is certainly more entitled to credit than those who only mention the opposite fact in a general way. Out of 222 cases seen in the establishment at Siegburg, only 22 or 23 presented symptoms of fever, many of which were hectic, and other forms of pyrexia, having no necessary connexion with the mania.

According to Dr. Burrows, who considers that an abnormal condition of the pulse is closely connected with the psychical state of maniacs, there is a remarkable difference observable in many cases between the number of beats in the radial artery and in the carotids, and even in this respect the two carotids not unfrequently differ from one another in the same individual. This observation has been confirmed, or rather repeated, by other writers. One case is mentioned by Burrows, where the pulse at the wrist was 90; that in the carotids 115, 120. Jacobi evidently treats this case with considerable suspicion, and takes some pains to contravene the opinion of Dr. Burrows, and adds, "that of 1200 patients who were repeatedly the subjects of observation by him and the assistant physicians of the establishment, *in no one instance* had the number of the pulse in the radial and carotid arteries, or in the two radials, differed to the amount of one single beat." It would certainly be very strange if facts should turn out so contrary to all our physiological notions respecting the circulation as to induce us to believe that the pulse is not produced by the contraction of the heart, or that any cause between the heart and the wrist can have the effect of lessening the number of pulsations to such an extent. It is perfectly intelligible how there can be a difference in the quality of the pulse, even between the two carotids, from a diseased condition of one hemisphere of the brain, as we find in inflammation of the hand that the radial artery beats much more fully and strongly than upon the opposite arm, with which, however, it corresponds in point of number.

Jacobi remarks, with regard to this difference in the quality of the pulse of the two sides, that when it is constantly present the case is almost always incurable.

Compression of the carotid arteries in cases of mania was first imagined and tried by Dr. Parry: it has been employed by Dr. Burrows and also by Jacobi at Siegburg. Burrows says, that in recent cases of mania the violence of the symptoms was lessened by the practice. Jacobi observes on this point:

“With respect to the result of this operation (compression of the carotids), which has been so frequently performed in our asylum, we must first of all remark that the occurrence of phenomena following it varied exceedingly in different individuals, and that in some they were scarcely to be detected. They were as follows: a feeling of burning heat which spread suddenly over the head and neck down to the chest, or, when the compression was merely upon one side, over the corresponding side of the head and neck, with a feeling of heat within the head in many cases. The face becomes darker, with frequently the production of a vivid colour, an extremely painful sensation of compression on the chest, a feeling of tension, weight, and pain in the head, giddiness, staggering, sleepiness; actual sudden sleep, with stertorous breathing; in many incipient syncope, with uncertainty in the use of the lower extremities, stumbling, and in some cases they suddenly fall down with entire insensibility, but speedily recover. One of these patients fell before me at the moment when both carotids were compressed, as if struck down by a blow, but came to himself in a few moments, and complained of a headache; in another the syncope was not so complete, continued for some time; and a third said, when he recovered from the state of insensibility, looking wildly around him, that he had suddenly fallen asleep, and could not collect himself upon waking, so as to remember where he was.” (p. 383.)

We here have a collection of symptoms in all probability arising from violent pressure, not only upon the carotid artery, but also upon the veins and nerves of the neck, and we are not surprised at finding that Jacobi considered the plan of compression of no practical value in treatment, although he adds that, when carefully employed, the appearance or absence of any of the above-mentioned results of the proceeding may afford considerable assistance in the diagnosis of the case.

II. The second table refers to the state—1st, of the upper part of the alimentary canal, teeth, gums, appetite, thirst, &c.; 2d, of the lower part of the digestive organs; 3d, the function of the kidneys and condition of the urine; 4th, of the skin; and lastly, of the functions of the entire apparatus of nutritive or organic life. The results of these observations are simply remarkable for the very little departure from the usual condition of health which they exhibit. The secretion of saliva was frequently increased and the appetite was often excessive; notwithstanding this latter fact, however, the nutrition was in the generality of cases in a very low state, but there was no constant disorder in the digestive system. On this point the experience of Dr. Jacobi is contrary to that of all English and French practical writers who have observed that the functions of secretion and the state of the mucous membrane of the alimentary canal in maniacs is very much disordered, so much as to cause a peculiar fetor which has been noticed by very many observers. In the third section of his ‘Observations,’ upon the pathology and cure of mental disorders, Jacobi endeavoured to disprove an observation of Esquirol, who remarks that there

is a smell peculiar to the insane, proceeding partly from the skin and partly from the breath, although in the present work he passes it over unnoticed. He accounts for the increased desire for food as well as for the abundant secretion of the salivary glands by the somewhat unsatisfactory expression of an alienated condition of the functions of the nervous system, and he devotes a considerable portion of this section to extracts from other writers, which he compares, or rather contrasts, with his own observations. He has looked through the writings of other authors, in which descriptions of cases have been given, and has found that out of 130 which have been described in various works, the state of the tongue is mentioned in only 13, that of the bowels in 25, and the condition of the salivary organs and secretion in only one, while no writer gives a description of mania without mentioning particularly the condition of the various organs. The tendency of these remarks would lead us to look upon such general accounts of the diseases, when they differ from his own, even though narrated by Esquirol himself, with a certain degree of suspicion.

The fulness of these tabular statements has certainly the advantage of bearing an appearance of authenticity and weight; but it must be evident that the observation of the practitioner may be diverted by the great number of particulars which he is called upon to notice from directing itself to symptoms which, to a mind not so engaged, would present themselves as essential and characteristic.

III. The third chapter and table is drawn up under four heads, and gives—1st, a statement of the age and temperament, the form of the skeleton, development of the muscles, colour of the hair and iris; 2d, the state of the organs of sense, and sleep; 3d, that of the brain and nervous system, exclusive of the higher psychical powers; and 4th, that of the organs of generation.

The muscular system was, in the generality of these cases, moderately developed, and the degree of bodily strength was in relation with it. Esquirol, Ideler, Pinel, Neumann, Rush, and, in fact, almost every writer upon the subject, describe the muscular power as excessive, enabling the patients to break their chains, &c., whereas Jacobi asserts from his own observations that no increase whatever in the strength takes place, and that a single attendant is generally able to overcome a patient during an attack of maniacal excitement, so as to put him under restraint.

The senses were slightly disordered in 12 out of 50 cases, in the generality of whom the abnormal condition consisted in an increased sensibility of the ear and eye, a dislike to loud sounds, and a slight degree of intolerance of light: common sensation was in an altered state in many cases; and in a large proportion sleep during the night was almost entirely absent.

Paralysis and other disorders of the brain and nervous system were not very frequent. In most cases the brain was the first part of the nervous system which was affected, while in some the disease was said to have commenced in the sympathetic—in others simultaneously in both sets of nerves.

Among the male patients but very few exhibited any signs of inordinate excitement of the generative organs, and among the females the only re-



markable fact was the frequency with which the catamenial discharge was found wanting, returning in some when the mania became confirmed—in others as a commencement of convalescence.

IV. The fourth table gives an account of the morbid psychical phenomena in maniacs; in the appetites, moral feeling, and exercise of the will; in intellect, imagination, association of ideas and judgment, as well as the state of the mental faculties during the remission.

After giving an account of the relative numbers in which the various phenomena presented themselves, a repetition of which would be of no interest to our readers, the author remarks :

“ From this table it is seen that a general violent psychical excitement, hastiness, and mental irritability, with tendency to break out into fury and rage, exists almost always in mania, united with an inclination to act violently towards others, a love of destruction, inattention to the rules of cleanliness, sometimes also with a complete alteration in the natural disposition of the individual; whilst, on the contrary, morbid excitement of the genital organs, rudeness of manners and lewdness, which are not natural to the patient, are only decidedly seen in the minority.”

He describes as the source of their behaviour an ungovernable change of the appetites, sometimes displaying itself but slightly, but generally breaking out into excess of malicious, insolent merriment, occasionally preluding a state of depression and anxiety. The exact subjects or material which furnishes them with ideas during the period of maniacal paroxysm are traced by Jacobi to three sources:—1st, objects which present themselves to the cognizance of the senses, as we so frequently see in the delirium of typhus; 2d, pictures of past events called up in the memory; and, 3d, single isolated words, phrases, or numbers which the patient remembers and repeats without cessation, and frequently with the most violent gestures. And here we must recollect that out of 228 cases of mania which the author notices, only 20 showed any illusion to such an extent as to become an element in their ideas during the period of excitement.

Dr. Jacobi considers that this form of madness differs from a merely excited state of the mind produced by illusions, and that during the attack itself certain causes (*momente*) act upon the mind, and influence the maniacal language and gestures, whereas most authors consider that a disordered and confused state of the ideas, or even an illusion, is the exciting cause of the paroxysms. Esquirol says that the multiplicity of the ideas, and the rapidity with which they are received, the faulty association, and the deception of the senses, and want of attention, lead astray the judgment of the patient, and by over-exciting and over-straining his passions drive him on to violent and dangerous extremes.

The third division of the work, ‘On the causes of mental disorders which have the character of mania,’ is subdivided into four chapters:—1st, General influences, circumstances of climate and weather, condition of the country, food, family, morals, political state of the people. 2d, Influence of sex, age, bodily constitution, temperament, intellectual abilities, religion, occupation. 3d, Hereditary and congenital causes. 4th, Acquired, remote, and exciting causes.

The statistical or numerical results presented to us in this part of the

volume are based on too few data to be admitted as general conclusions. We have access to data much more extensive, which we hope to lay before our readers on another occasion; we shall therefore, for the present, pass over the principal results of this portion of our author's work. We must, however, find room for a brief notice of his peculiar views respecting the nature and causes of insanity.

Most authors consider mental or moral causes as of more frequent operation than physical in producing diseases of the mind; but, according to Jacobi, those who think thus found their opinions not upon the accurate observation of facts, or, at any rate, they only observe those occurrences which are immediately antecedent to the maniacal attack. This remark of the author respecting the psychical causes of mania in some measure involves the consideration of a view peculiar to him. He thinks, for instance, that the brain is not (as most observers, from its structure and from pathological facts, are led to conclude) the only organ of all psychical actions, but that the bones, ligaments, muscles, in fact every part of the system, has an equal share in producing the phenomena which are usually referred to the mind! This is in a few words the substance of his theory, to which we alluded in the commencement of the present article, when speaking of his former works, and of which the following is an abridged account, taken partly from his '*Sammlungen*,' and partly from the work now under consideration.

After giving quotations from various authors who consider the brain as exclusively the organ of the mind, he says that if we will examine the facts which tend to elucidate the relation of psychical phenomena to the entire organism of the human body, we shall necessarily be led to conclude that besides the brain and the nerves, the whole system, as well as the individual organs, are in immediate relation to the original production, and to the various forms of these phenomena. In regard to the bony fabric, for example, he says that, from the improved manner of examining such subjects, the time will come when the physiologist will contemplate the os sacrum quite as fully as the os frontis, if he wishes to determine what had been the degree of mental endowment of the individual, and "when the form of the skull being given, it will be no proper trial of skill if he is called upon to describe the mental powers of a Philoctetes or an Ulysses." How often, he continues, do we see peculiarities in the skin and its appendages, hair of a certain colour or strength, and peculiar cutaneous secretion, temperature, &c., connected with some particular condition of the mental powers? while varieties in the state of the mental faculties are continually observed upon the recession or reappearance of cutaneous eruptions, &c. The vascular system, he says, is on all hands allowed to be closely allied to mental phenomena; a man with a large development of the arterial system is active, hopeful, sanguine, and enterprising; and a contrary condition of the vascular system is found in men disinclined to active exertion, and slow in judgment and resolution. The organs of voice have similar indications, and in like manner the stomach and alimentary canal; for instance, different kinds of food produce different effects. "Some men are incapable of deep thought in the morning before they have taken coffee; some require half, some an entire bottle of



wine to make them sociable." Similar facts are brought forward respecting the muscular and other systems.

Holding, as he does, such views as these, it is perfectly intelligible how Jacobi can admit that diseases of any part of the system do not act upon the mind and produce insanity through the medium of the brain. Being not less than the brain parts of the organism connecting the mind to the body, the imperfection of the instrument in any instance prevents the possibility of a natural or healthy action of the mental faculties. We may connect with this idea of Dr. Jacobi the fact of his leaving out of his list of causes one which is of frequent occurrence, and which is always enumerated in other works upon the subject, viz. injuries of the head. Although there is a certain amount of truth in his remark concerning the occasional occurrence of insanity in cases which do not manifest cerebral disease, and are dependent on the disorder of other organs, his view is utterly untenable as a true theory of insanity; and certainly he has been able to establish the truth of no such thing in any part of his writings.

We find nothing in the sections on treatment which would interest our readers. It is evident that the system of non-restraint, as carried out in this country with such admirable results, is imperfectly known and practised at Siegburg.

In conclusion, we must say that although the present volume contains much that is valuable, and merits a place in the library of every physician who treats insane patients, or studies insanity, it will add much less to the previous stock of our knowledge, and to the widely-spread reputation of its author, than we had anticipated before perusing it.

## ART. II.

*Physiologie Pathologique, ou Recherches cliniques, expérimentales, et microscopiques sur l'Inflammation, la Tuberculization, les Tumeurs, la formation du Cal, &c.* Par H. LEBERT, M. D. Accompagné d'un Atlas de vingt-deux planches gravées. Deux Tomes.—Paris, 1845.

*Pathological Physiology, or Clinical, Experimental, and Microscopical Researches on Inflammation, Tuberculization, Tumours, the formation of Callus, &c.* By H. LEBERT, M. D. With twenty-two engraved plates. 2 Vols.—Paris, 1845. 8vo, pp. 554 and 515.

THE volumes we are about to notice contain the first attempt yet made in the French language to present a general view of the minute nature and constitution (as discoverable with the microscope) of the processes and products of disease. The work is entitled to a welcome in France, where a very remarkable state of ignorance concerning the advances made of late years in this country and in Germany in the department in question prevails. The amount of favour with which it may claim to be received *here* must depend upon the amount of novelty in fact, inference, or doctrine it contains—upon the degree of critical acumen it exhibits in separating the mountains of dross from the sprinkling of gold scattered through the recent literature of pathology, and upon the fairness and con-

scientiousness with which the labours of the author's predecessors are employed and acknowledged. It is, at least, upon grounds such as these that our award shall be made.

The author divides his account of inflammation into two chapters. In the first appears an inquiry into the general phenomena of inflammations, those common (more or less invariably) to that state as it occurs in all the tissues and organs; in the second we are made acquainted with the peculiarities of the disease in certain textures.

The general history of inflammation commences with that of the state of the capillaries and blood in the parts actually implicated in the morbid process. As the greater part, indeed almost the entire, of the description here given not only contains no novelty, but bears upon facts which, though announced with considerable pomp and an ingenious tone of self-sufficient originality, are as familiar to all minute observers of inflammation as redness is to the surgeon among its clinical phenomena, we shall only notice a few statements scattered through these prolix pages. The author holds that in the first stage of inflammation the caliber of the capillaries is diminished, and the rapidity of the circulation increased; he should have known that this has been questioned altogether by some very sagacious microscopists, and that, at least, it is *certainly* not a constant occurrence. He estimates the possible amount of diminution at one third of the total width of the vessel. This attempt at metrical precision appears to us to render inaccuracy more inaccurate, rather than any thing else.

M. Lebert takes credit to himself for the announcement that the generation of new vessels in inflammation is "not exclusively confined to the organization of the products of exudation," but occurs amid the tissues themselves affected with the hyperæmic changes. A precious *discovery* this! which happens to have been taught by one of the very earliest minute observers of the inflammatory process, Kaltenbrunner.\* It is true that Kaltenbrunner supposed that extravasated blood channeled canals for itself in the tissues amid which it escaped, and that M. Lebert does not admit this mode of production; but the main *fact*, put forward as novel, is as old as the study of inflammation. M. Lebert introduces with considerable flourish of trumpets his explanation of the mode in which new vessels are formed: "He has specially engaged in this investigation, entering as it did into the plan of his pathological researches, and of his study of development in general." (p. 15.) Scarcely will it be believed that after this morsel of egotistical puffing this author should have neither investigated in any novel manner any old facts, nor traced out any new ones, and that he leaves the three notions, which have long contended for supremacy in the minds of pathologists, (namely, that new tubes are produced by the elongation or looping process from old vessels—that they are merely vessels which pre-existed, but which being too narrow to receive red blood, have, through the inflammatory process, acquired sufficient width to admit this—and, lastly, that they are actual new formations generated independently of the pre-existing circulation,)—that he leaves these three notions precisely as he found them, unproved and unsettled. Nay, more, he is either deplorably ignorant of researches made in this country, in Germany,

\* See *Cyclopædia of Medicine*, art. *Inflammation*, p. 713.

and in Italy, on the subject; or he conceals his acquaintance with them for motives which we do not pretend to understand.

There is nothing new here on the phenomena of hyperinosis; but it is as well to make it known to the readers of this Journal that the "great fact" of hyperinosis, increased proportion of fibrine, had been pointed out by Stannius and Denis in 1838. The fame of M. Andral, *filis*, has thrown into shade too profound the merits of the original observers of the facts which he has confirmed and generalized.

The "compound-inflammation corpuscles" of Gluge receive a description of considerable length and much pretension at the hands of M. Lebert. He adds nothing to our knowledge of their nature, and there is something ludicrous in the solemnity with which he declines to admit the error of Gluge (the error of, however, an accurate and truly original observer), that the corpuscle in question is formed of aggregated nuclei of stagnating red corpuscles. M. Lebert believes the constituent granules rather of fatty nature than composed of fibrine or albumen. This is announced in the guise of an original opinion. It is well known, however, that such was the notion held and taught by Valentin and Ascherson. The German observers gave ingenious and most plausible, if not absolutely convincing, reasons for the views they advocated. M. Lebert contents himself with a magisterial statement of his opinion; he goes back instead of advancing. And he finishes off this lame and yet conceited description by referring to his figures of these corpuscles—figures which are, beyond all comparison, the very worst representations of the real bodies we have ever chanced to see.

The few next pages contain some similar announcements of the discoveries of others, (without any reference to the original observers,) which we conceive it is well to notice. Speaking of the fibrinous matter of common plastic pleural exudation, M. Lebert observes, "the fibrine becomes thus more and more condensed, and the appearance of *fibroid* stratification gradually changes into real fibrous tissue, but not through the intervention of cells and fusiform corpuscles; it is rather effected by condensation of the effused materials." Passing over the elegancies of diction of this sentence, its hybrid word, and its reference to an *appearance changing into a tissue*, let the reader note the honesty of the man who, in announcing such a theory, omits all mention of the name of Mr. Gulliver. In the next page we find M. Lebert assuming a quiet tone of superiority on the strength of the discovery, that in various states of morbid texture, described as "cartilaginous transformations," no really cartilaginous structure exists. Bless the man's eagle ken! Why Müller (to whose 'Archiv' M. Lebert is a contributor, be it observed,) showed the non-cartilaginous nature of many products, (to which the term had formerly been applied,) seven years ago; and the fact is as familiarly known to all persons acquainted with the progress of minute morbid anatomy—"practical surgeons" of course continue, and will continue for the next half century, to speak of "loose *cartilages*" in the joints, &c.; but it is to be presumed M. Lebert does not write for *them*,)—as one and one make two. Similarly is the statement made that in the so-called "osseous transformation" of false membrane no bone is to be found. True it certainly is, that the amorphous mineral matter, the patches of saline sub-

stance, found often in various other morbid productions as well as in pseudo-membrane, were set down as actual "ossifications" by certain morbid anatomists before the introduction of the microscope; and true it also is, we believe, that some of these morbid anatomists having once "said it, stick to it." But it is quite as true that the novelty of M. Lebert has been for years matter well known to all accurate observers. To all appearance M. Lebert fancies himself addressing an ignorant and a dishonest audience. What empty *fanfaronade* is this that meets our eye in his preface concerning his having chosen the French language as the medium for issuing to the world his production?—"It was for him a matter of conscience and of deep satisfaction to write in the tongue of that land (although not his maternal language) where he had received such support and sympathy!" &c. &c. No, M. Lebert! There may be a truer explanation found for your eschewing your mother-tongue, and taking refuge in the convenient (under the circumstances) language of France. Could you have written in the language of Valentin, of Henle, of Güterbock, of Gluge, of Ascherson, and of many other honest, honorable, and high-minded Germans, of whose patient labours your work seems too often an appropriation? No, you could not; and so you bethought you of your patrons, those excellent Parisians, who, with a few distinguished exceptions, never at any time see much beyond their own noses in such matters, and who (experience doubtless taught you this) would be ready, once you became "one of them," to fight your battles for priority, originality, and all forms and varieties of what, in their inflated bombast, they call "GLORY." We tell you, M. Lebert, "glory" is a fine thing, but common honesty is a better thing. And trouble us not with the ready reply of literary thieves belonging to *your* section of the fraternity: "You do me injustice—see how original I am—look at my scores of cases—my jottings down of measurements of cells—my post-mortem examinations, and my clinical records." Trouble us not with this, we say, for we know its value; we know, we have traced, the way in which the thing is done. Are you a very original man if you take in hand, for example, Abercrombie on the 'Brain,' get thoroughly up the details of symptoms and morbid changes of some particular cerebral disease, and, thus crammed, make your way into a ward where some example of the sort is to be found, and from memory make up a "case," with profound "original" remarks at its tail concerning the disease in question—all of which remarks are to be found in essence in the standard volume that has instructed you? Hardly, we should think. And yet this is the most lenient way of viewing the course you have pursued, and which is pursued by many by whom temporary notoriety is taken in barter for honour.

M. Lebert having found existing descriptions of the proper pus-corpuscle "much less accurate than the deep importance of the subject requires," has "long made this body the subject of attentive observation." The prolonged and attentive observation has unfortunately led to the detection of no single point of novelty in the anatomy or pathology of the corpuscle; though the account is got up in a style to deceive many readers into a belief that much of the matter before them is original. Talking, for instance, of the action of acetic acid on the corpuscle, he observes as follows: "Several authors have contended that the nuclei of these corpuscles originally formed a single nucleus only, which split up into several under

the action of the acid. We object to this mode of viewing the point, from the facts that the presence of several nuclei may be recognized without the action of the acid, and that we have never seen any appearance of intermediate degrees of the splitting up." Now, would not any human being, previously unacquainted with the subject, fancy that the merit of pointing out the error of the notion that the nucleus is invariably single in the natural state, and while uninfluenced by chemical reagents, belonged to M. Lebert? Yet the truth is, for years past, the fact, as put forward by him, has been known to all observers, and was *first* (as we believe) stated in print by Vogel in his work on Suppuration—a work, be it observed, with which M. Lebert is, on his own admission, acquainted. It may be advisable to add that the discovery that a double, treble, or quadruple nucleus might be seen in pus, to which no chemical agent had been added, was first made on pus that had stood for some time. The observation was consequently open to the objection that free acid might have been generated in the fluid, and acted, of course, as acid artificially added. Even this objection (of which M. Lebert appears, or desires to appear, ignorant) is without force. We, and doubtless many other persons, have frequently seen (with a power of 400 diameters even) a double or a treble nucleus in alkaline or neutral pus immediately on its removal from the body. On the whole, M. Lebert's description of the pus-corpuscle is greatly inferior both in accuracy and fulness to those of Henle and Vogel, though the former of these two persons was the originator of the error just referred to respecting the nucleus.

M. Lebert describes, as a constituent of pus, a corpuscle which he regards as a variety of the proper pus-corpuscle, and to which he gives the name of *pyoid*. "They are of spherical shape and composed of two elements; of a somewhat transparent substance, rather of solid consistence than liquid, and of molecular granules, varying in number from four to ten and upwards, irregularly scattered through their substance. They have no nucleus; and acetic acid, while it renders them somewhat more transparent, does not change them. They are larger and more spherical than the globules of tubercle, smaller and more granular in their substance than the white corpuscles of the blood, from which they differ in another essential character—their yellowish tint." We copy this description in order that our readers may inquire into its correctness. We have no acquaintance ourselves with a corpuscle of the kind, as a uniform or even habitual constituent of pus.

Pus contains more water than does the serum of healthy blood, or even blood in the state of inflammation. The sum of fibrine and of albumen is greater in pus than in the serum of inflammatory blood; as is also the quantity of fatty matter. The latter, as has long been known, abounds in some specimens of pus to such a degree as to render the fluid inflammable. Cholesteric fat, according to the recent numerous and most elaborate analyses of Bibra, chiefly accumulates—a fact confirmative of the statement of MM. Becquerel and Rodier, that the quantity of cholesterine in the blood constantly undergoes increase in inflammation. The quantity of ash proportionally left by the two fluids varies; there is sometimes more, sometimes less, in pus than in blood. The salts in the two fluids do not much differ. Hence the principal chemical difference is that the quantity of albumino-fibrinous substances is greater in the blood (the red



corpuscles being included in the analysis), and the blood contains iron. Both these differences depend on the fact that in the process of pus-formation the globules remain in the vessels, and that the fluid elements of the blood only transude through the vascular walls. A homœopathic dose of iron has, it is true, been sometimes found in pus, but this has arisen from accidental rupture of vessels, and consequent escape of red corpuscles. By the way, these chemical facts, and their aid in the demolition of that thrice-ridiculous notion, taught originally by Gendrin, and espoused by Donné, that the corpuscles of pus are transformed red blood-corpuscles. Doubtless the notion in question had been sufficiently well demolished by direct observation; but it is as well to point out the bearing of chemical evidence against its reception.

M. Lebert has endeavoured to trace the phenomena of pus-formation upon blistered surfaces, after the manner of Henle, Wood, Güterbock, and Vogel—to whose labours no reference is made. He has not been able to substantiate the affirmation of some authors that the nucleus is first formed, and the involucrum subsequently deposited around it. He fancies he has always observed that the pus-corpuscle, like many other cells, is formed with *all* its elements from the first—that these elements are at first excessively small, and that the ulterior change of the corpuscle consists in the increase of size, and in the clearer separation of the various elements which compose it.

M. Lebert is of opinion that there is no such thing as a special mucus-corpuscle, and that natural mucus, free from accidental admixture with substances foreign to itself, contains none of those corpuscles which have been described as belonging to it, and as very strongly similar in character to pus-corpuscles. When, he observes, the mucus examined was really healthy, young epithelial cells, or the large nuclei of old epithelial cells, were taken for special corpuscles, to which the name of mucus-corpuscle was assigned. Or, when the mucus examined was in a morbid state, these pus-corpuscles were mistaken for a novel variety of cell. We believe that this is a correct view of the question, which has certainly given rise to very contradictory statements both in this country and in Germany. But it is not to M. Lebert that the first establishment of the fact is due. The valuable observations of Vogel (in his work on Suppuration) contain the elements for the proper decision of the point; his experiments on the rapid production of pus-corpuscles, instead of epithelial cells, from the mucous membrane of the male urethra, under the slight irritation produced by the presence of a bougie, made the conclusion, which M. Lebert announces as perfectly novel, the only one tenable. Those experiments have also completely anticipated the results obtained by M. Lebert concerning the astonishing rapidity with which true pus-corpuscles are thrown off by mucous membranes. Vogel, too, goes into another and most interesting question, namely, to what extent can the formation of such corpuscles be considered to constitute true suppuration, while no proper fluid (no *liquor puris*) is at the same time produced, or at least can be shown to be produced. Here, as in many other places where M. Lebert puts himself forward as belonging to the vanguard, he is, in truth, bringing up the rear rather lazily.

In describing, (1) “the incomplete cure of suppurating wounds, and,

(2) cicatrization by granulation," M. Lebert attempts to introduce a modified version of the doctrine taught by various observers of the phenomena of cicatrization." The whole of the "novelty" here lies in a nutshell. The cell-structure of plastic exudation-matter, formed either on surfaces or in the interstices of organs, which in these positions forms the material of indurations, certain so-called hypertrophies, &c., and which is the product of chronic inflammation, M. Lebert describes under the name of fibro-plastic tissue. He describes the cells as going through the well-known series of changes from the spherical globule to the actual fibre. Now these corpuscles with their series of changes have certainly, as M. Lebert states, been by some writers described as the proper elements of granulations, and the means whereby perfect cicatrization is accomplished. To this doctrine he demurs, on the ground that the structure in question occurs under circumstances altogether opposed to the occurrence of perfect cicatrization, while this process is really effected on a very different plan. This plan may be made comprehensible according to M. Lebert by the following description: "Between the vessels of granulations," (the vessels are described in a loose manner, with not a tithe of the precision of Pauli and Miescher,) "lies a yellowish substance, appearing homogeneous and finely granular, when examined with a common lens, or with a glass of weak power; but if a stronger power be employed, the observer ascertains that this elastic semi-transparent tissue, this fibrinous gelatine is formed of a substance in which but few real fibres and few fusiform bodies are perceptible, but rather a stratification of fibrinous appearance, analogous in aspect to coagulated fibrine. Throughout this fibriform stroma lie round, or elongated, or deformed globules, measuring 0.1 millimeter in diameter, containing from two to four small nuclei, which are rendered more visible by the addition of acetic acid; these are deformed pus-globules in course of disintegration and solution. In a word, this intervascular substance is an organized pyo-blastema becoming transformed into coagulated fibro-albuminous gelatine, and holding imprisoned dissolving pus-globules." (vol. i., p. 81.) If this last sentence be not the quintessence of absurdity, we know not what it is. Coagulated fibro-albuminous gelatine, which said most curious gelatine is an organized pyo-blastema! As cicatrization advances, the number of vascular loops diminish, as does that of the globules of pus: these at last cease to be distinguishable, "being decomposed into molecular granules; the *coagulated gelatine* itself becomes paler, and real fibres, delicate and distinctly defined, of unequal width (from .002 to .003 of a millimeter) constitute the chief part of the mass; they do not form fasciculi." Now, except in respect of the absurd language employed by the author, (we might indeed say the actually incorrect language,) there is absolutely nothing in all this. But now for the contents of his nutshell. "These fibres rather *appear* to be the final result of coagulation and condensation, than that of a transformation of cells and fusiform corpuscles." Now we are certainly as far from denying that this proposition would, if proved, be one of deep importance, as we are far from contesting its importance of the cell-theory itself; but we ask where is the evidence of the exactness in the details we have given, and these we pledge ourselves contain the essence of all that the author says upon the subject. Observe that he ventures no further than to affirm that the phenomena "appear"

to be of the kind he would fain have them, though the pompous exordium to the discussion on cicatrization justified us in expecting a thorough demonstration of some novel doctrine. And further, even if M. Lebert had proved his point, *his* would not be the merit of originality, as it is well known that the suggestion that fibres form independently of cells fell first from Mr. Gulliver. And, lastly, what evidence have we in the coarse descriptions, and still coarser figures of M. Lebert, that (admitting *argumenti gratiâ* that the fibres in question were not evolved from perfectly-formed cells) those fibres were not, as Henle contends is in some instances the fact, produced from nuclei arranged in rows and modified in character? To Henle's doctrine M. Lebert makes not even a passing allusion.

The *fibro-plastic* elements, as M. Lebert terms them, or simple induration matter, as others would less ambitiously entitle them, form "the basis of certain tumours, which we have often seen in the mammary gland, where they were taken for encephaloid; and we congratulate the surgeons who committed this error, as they will entertain the belief that they have radically cured cancer of the breast by operation." (p. 80.) To congratulate a man on his committing an error seems to us a very unaccountable mode of proceeding.

M. Lebert comes to the terminations of inflammation, ulceration and gangrene. He joins in the wake of those who maintain these two conditions to be very closely connected, on the ground that both acknowledge as their immediate cause the obliteration of a certain number of vessels, which are not replaced by others of new formation. The parts thus deprived of nutritious fluid "fall into detritus," that is, they ulcerate; or they "become detached in fragments of variable size," that is, they sphacelate. Now, there is little doubt (the notion has for some time been generally admitted,) that the connexion here pointed out between these two processes really exists; but, in the determination of this point, though an obvious step has been made, the difficulty of the theory of ulceration is by no means removed. It is perfectly clear that there is something besides obliteration of vessels to be discovered; for, if this were the whole morbid state existing, why should visible and tangible results, so widely differing as ulceration and gangrene, be both produced?

M. Lebert remarks on the treatment of inflammation. He examines the question of the utility of venesection and other modes of bloodletting, and leaves the question exactly as he found it, that is, undecided. He appears, indeed, to be but imperfectly acquainted with the method of seriously investigating a point in therapeutics, and is beguiled by the fancy that vague generalization from loose recollections may be permitted to assume the significance of sound inferences from lengthened and recorded experience. The following passage will supply justification of our mode of judging M. Lebert's claim to distinction as a guide in therapeutics, while (this is our motive for extracting it) it suggests for examination a question of very considerable interest:

"Having practised in a mountainous region, where many of the inhabitants, far removed from medical practitioners, are in the habit of leaving the treatment of diseases the most serious to the sole care of Nature, I have observed that persons labouring under inflammatory affections, especially pleurisy and pneumonia (frequent in this country), for which medical advice had been had from the first,



and continued through the course of the disease,—I have observed, I say, that these persons generally recovered, and that, although they remained ill the length of time required by the course of the disease, they scarcely ever became the subjects of relapse or return of the disease during the subsequent years. Those patients, on the contrary, who had been allowed to remain without any kind of medical interference, often recovered, and sometimes sufficiently rapidly and completely; but, in general the mortality was much greater among them, though not in proportion to the want of care that had been their lot. And the fact which struck me most forcibly was, that they were much more subject to return of the disease; and I have seen non-tuberculous persons, who declared that they had three or four attacks of pleurisy in the course of a few years—a fact I have subsequently been enabled to verify myself." (vol. i., p. 98.)

We were not aware, until we read this passage (wherein the inference is forced upon us) that it is a very rare circumstance for persons to have three or four attacks of pleurisy in the course of a few years, even though they may have been bled, blistered, cupped, leeches, purged, nauseated, and the rest of it, during the first attack of the series. But, passing over this, we cannot avoid observing on the astonishing coolness with which this rural practitioner gives out a proposition of such deep importance as that we have just laid before the reader. Much as he is evidently disposed to overrate the value of his doings, we cannot conceive him so egotistical as to believe that a point so important as this should be considered to be established by his simple assertion; the most favorable view of the matter we can take is, that the vast bearings of the question mooted have altogether escaped his penetration.

Having passed in review the general phenomena of inflammation, M. Lebert turns to the consideration of inflammatory changes in various organs and textures. The numerous observations placed on record in this part of the volume are useful in some points, but, upon the whole, there is the usual deficiency of reference to the labours of others, and the same self-sufficient tone of a discoverer assumed here as elsewhere. In the description of "albuminous nephritis," we find the following statement concerning the "microscopical molecular composition of the renal granulations in Bright's disease:"

"I have found in them no other elements than very numerous molecular granules, varying in diameter from  $\cdot 2$  to  $\cdot 25$  of a millimeter, agglomerated in masses of some size. Frequently a certain number of these granules form into groups and eventually become surrounded with a membrane of investment,—a process analogous to the formation of the large globules of the yolk. These granular globules of  $\cdot 15$  to  $\cdot 25$  of a millimeter in diameter exhibit molecular movement in their interior; they sometimes exist in very notable quantity in the diseased kidneys. I have not succeeded in discovering them in the inside of the capillary vessels, notwithstanding all my efforts; and I believe that they are not found until after the escape of their elements by exosmosis through the walls of the capillaries. I have also found in the granulations fat-cells and fat-granules, sometimes even in tolerably large proportion." (vol. i., p. 147.)

We turn with M. Lebert to the anatomy of tubercle. The invariable elements of this substance he considers to be—1, a great quantity of perfectly round molecular granules; 2, a hyaline material uniting these granules, and the following element; 3, peculiar globules. These globules the author conceives to be the characteristic and essential element of

tubercle; we must consequently place before the reader the chief points of the description given of them:

"The form of the globule of tubercle is rarely perfectly round . . . . their somewhat angular outline depending probably on their close juxtaposition . . . . they are of clear yellow colour, and appear blackish under a powerful glass. Their interior is irregular and of unequal consistence, which gives it a spotted appearance independently of the granules it contains. We have never been able to discover true nuclei in these corpuscles, although they sometimes present in their interior the irregular appearance of a vacuity resembling a nucleus . . . . The contained granules, disseminated irregularly through the substance of tuberculous globules, cannot be regarded as nuclei; they are, in fact, merely molecular granules, scarcely reaching, and never exceeding, a diameter of  $\cdot 0025$  of a millimeter. They vary in number from three to five, or ten and upwards; are not regularly placed in the interior of the globule, and not all visible at once on the same plane . . . . The diameter of the *round* tuberculous globule varies between  $\cdot 005$  and  $\cdot 0075$  of a millimeter; the *oval* kind are of the mean length of  $\cdot 0075$  of a millimeter, by  $\cdot 005$  or  $\cdot 006$  in breadth. Their diameter increases when the process of softening commences." (vol. i., p. 353.)

Water is said not to affect these corpuscles of tubercle; acetic acid renders them more transparent without much altering them otherwise; above all, it discloses no nucleus in their interior. The latter statement stands in complete opposition to the description given by Henle.

Persons of much distinction as pathologists have, as M. Lebert observes, looked upon tuberculization as a modification of suppuration; and, besides, the existence of tuberculous matter in the interior of cancerous tumours has been frequently spoken of as a condition occurring in the dead body. The utter error of the first notion has been thoroughly proved by clinical observation; M. Lebert adds no fact to the evidence already existing on the subject in respect of microscopical anatomy, except his "tuberculous globule," as above described, be admitted among the acknowledged elements of tubercle: if so admitted, its presence or absence would of course constitute a point of testimony for or against the existence of tuberculization. With respect to the occurrence of tuberculous matter in cancerous tumours, much erroneous statement has gone abroad, and M. Lebert is right in exposing it; but, in exposing it, he is much less original than he supposes. Dr. Walshe, in his recent work on Cancer, has in several places made reference to the error in question, and (p. 83) even attempts, from the examination of two specimens of encephaloid, to refer the assumption of the tuberculous aspect by cancerous substance to the circumstance of the latter being associated with unusually large quantities of fat. Besides this, it was long since (as Dr. Walshe states) taught by Dr. Hodgkin, that encephaloid, having accidentally acquired a yellow tint, bears some resemblance to tuberculous matter.

Observing upon the well-known circumstance, that pus and softened tubercle are often found mixed together in particles examined under the microscope, M. Lebert insists, "*in contradiction*," he says, "*to the generally admitted opinion*," upon the fact that the pus does not come from the tubercle itself, but that it derives its origin from the circumjacent textures. This unquestionably takes us by surprise. As a matter of history in pathological anatomy we certainly have heard of the notion that tubercle was "a tissue, possessed of vessels, &c.," and as such "secreted" the

pus wherewith it was found mingled; but that any one of the smallest authority in this country has (since the error of Laennec was exploded) held such a doctrine, we altogether deny, nor have we heard of its being professed either in France or in Germany.

In commenting on the fact that black pigmentary matter is sometimes found in considerable quantity around tubercles, M. Lebert observes that this occurs "especially in individuals *placed in circumstances particularly apt for the production of pigment*, as, for example, charcoal-men, who *introduce in breathing particles of carbon into the lungs in quantities*,—which substance has been analytically shown by L. Lecanu to be contained in those organs." (p. 371.) We confess we have much difficulty in understanding what the author's meaning is in this sentence,—the only one apparent on the surface is too ridiculous to have been entertained by him.

Here follows a section on the chemical properties of tubercle. The existing knowledge on the subject is put together in a very intelligible form. The fact recently established by M. Boudet, that there is much analogy in composition between the ash of tubercle and calcareous concretions forming in tubercles of the lungs and bronchial glands, is one of extreme interest.

M. Lebert confirms with the microscope the conclusion, established by clinical investigation by Laennec and Louis, that the gray granulation is a first stage of yellow tubercle:

"The invariable microscopical structure of gray granulations is a mixture of fibres, of a grayish coloured hyaline substance and the proper corpuscles of tubercle. When the gray granulation is seated in the lungs, the *fibres* are constituted by the *areolæ* [a curious inadvertence of language] of the elastic cellular fibres of the pulmonary tissue. If, on the contrary, these gray semi-transparent tubercles be seated in the subserous cellular tissue of a serous membrane, such as the peritoneum or the pia mater, the fibres are long, delicate, parallel to each other, and fasciculated. The proper elements of tubercle are always, however, to be discovered. From the absence or presence of the latter it is that we are enabled to determine whether certain granulations are tuberculous, or simply fibrous productions, which often remain as the last trace of pre-existing inflammation."

This, if true, is a very important point,—but we confess that all our experience leans us to doubt the *practicability* of the proposed mode of establishing the distinction in question.

M. Lebert has satisfied himself that fatty degeneration of the liver is by no means peculiar to phthisis, but occurs in a variety of chronic diseases of the alimentary canal. As respects the manner in which the fat is deposited in the liver, he has commonly found it in the form of fat vesicles, sometimes secreted in such quantity that the cells of the liver had altogether disappeared. But "the fat is very often deposited also in the interior of the cells of the liver, which proves that these cells are capable of imbibition and of undergoing various changes by means of endosmosis." We do not believe for our parts that the fact requires any such mode of explanation. No reference is made to the valuable observations of Mr. Bowman—observations, we happen to know, transferred to the French journals.

M. Lebert examines the question of the relationship of tuberculous and

scrofulous disease—a question certainly of deep practical interest, and one which has been, from time to time, and is even at the present day, variously determined. He is of opinion that the affections, regarded by the majority of practitioners as scrofulous, may be divided into the three following classes:—1. Chronic inflammations, in which no specific character is really discoverable on careful inquiry, and which are not in point of fact of scrofulous nature. 2. Scrofulous diseases, properly so called. 3. Tuberculous diseases, which, although having a strong analogy to scrofula, should be separated therefrom, as well on clinical grounds as on the evidence of morbid anatomy. In respect to the latter point, he admits that the blood of scrofulous persons and that of tuberculous persons must be very closely related in character; but there is this difference, that tuberculous diseases generate a special substance, namely, tuberculous matter, characterized by peculiar globules, whereas the pus of scrofula does not, as far as M. Lebert's repeated experiments go, contain any special element distinguishing it from other morbid productions. A different doctrine has been taught concerning this latter point; it has been said that there is a distinct microscopical difference in the ingredients of scrofulous and healthy pus—the difference, or rather the material of the difference, has even been figured. We have not examined the point sufficiently to justify us in assuming the office of judge. We consider it right, however, to observe that the whole of M. Lebert's argument really turns upon his tuberculous globule and its non-existence in scrofulous pus. Now, of the complete accuracy of his narrative concerning that globule we feel particularly prone to doubt.

M. Lebert applies himself in his second volume to the consideration of tumours, and commences by dividing them into two classes, homœomorphous and heteromorphous,—a mode of classification which, though not very peculiarly novel, as the words merely take the place of the analogous and heterologous of Laennec and many of his followers, is put forward with the claims of an original thought. Cancer represents his heteromorphous class, because “the characteristic microscopical element of that product is not found in the natural state, either as a permanent or transitory element.” Thus, then, he assumes there is a fixed character in a certain microscopical element, distinguishing cancerous from all other productions. Excellent well does this look upon paper; but what says experience of it? Let us hear what Müller, a not insignificant spokesman in such a cause, declares: “The most innocent growths does not differ in their *minute elements*, nor in their origin, from carcinoma.” M. Lebert continues: “as regards the distinctive characters of carcinoma, which have *hitherto* [!!] been given, such as its tendency to relapse, to invade adjoining textures, to become constitutional, to pass into the state of destructive ulceration, &c. &c., we recognize the value of these characters, especially when combined; but, on the other hand, we by no means discover in them characters exclusively proper to cancer.” One would fancy that Müller had never written his book on Tumours, and, above all, that M. Lebert (though he quotes it frequently) had a most convenient faculty of forgetting its contents when seized with the ambition of appearing vastly original. There is no point on which Müller dwells more strongly

than the inefficiency of these very characters as decisive of the cancerous nature of a tumour.

There is a good deal of novelty in M. Lebert's description of epithelial tumours, and we shall dwell more fully upon this chapter than upon any of his previous contributions. Epithelial tumours may be surrounded with a membranous investment, which is "either fibro-cellular, or wholly composed of globules." But epithelial tumours are also met with destitute of any special investment, and constituted under these circumstances by real hypertrophy of the epithelium: such, for example, are the nature and composition of opaque staphyloma.

"The principal element of these tumours is composed of membranous layers of tessellated epithelium, closely juxtaposed, in the globules of which nuclei are generally very distinctly visible; vessels may exist in them in very small quantity. These stratified tumours are commonly of a dull white colour, sometimes of a yellowish tint; sometimes they are considerably vascular, and they then are of a reddish colour, while numerous vascular arborizations and sanguineous infiltration, caused by minute capillary effusions, are discoverable under the microscope. ....Fibrous elements, but in small quantity, and principally under the form of fibro-plastic globules, fusiform globules, fibres widened in the middle, and perfect fibres, are also visible."

To the head of epidermic tumours the author refers corns, callosities, and condylomata, and besides a—

"Species of pure epidermic tumour, of papillary form, occasionally found on various parts of the body, and by no means always traceable to the syphilitic virus as its cause. Tumours of this kind may even acquire considerable bulk and become tolerably vascular; they are susceptible of becoming inflamed, of ulcerating, and of suppurating. Viewed with the naked eye they then appear covered, first, with a layer of variable thickness; when this layer is removed a yellowish liquid is discovered, composed of globules of pus and epidermis, mixed sometimes with sebaceous matter. These tumours are in general of yellow or reddish-yellow colour, elastic, and composed of papilliform lobules, somewhat elongated or united into groups, and exhibiting the cauliflower aspect. The microscope only discloses the elements of epidermis and a few vessels, and in the crusts an admixture of pus and epidermis. In this class we place a good number of tumours of the lower lip, supposed to be cancerous, but which on careful examination are found deficient in the elements of cancer, and composed in reality of hypertrophied papillæ, protruding from the surface of a superficial ulceration, and seated on an indurated basis, resulting from chronic inflammation."

To the fact that such sores as this are frequently mistaken for cancerous ulcers, M. Lebert ascribes the long current belief that carcinoma of the lip is more effectually curable by operation than the same disease in other parts.

A fourth species of epidermic tumour the author calls the fibro-epidermic; none other than warts of different kinds. A fifth species, of very rare occurrence, is formed by the simultaneous development of a great number of papillæ, accompanied with hypertrophy of the sebaceous glands, and constituting large cauliflower tumours, covered with a sebaceous or purulent fluid wherever ulceration has taken place. A sixth species of epidermic tumour is found, of a "fibrous stroma, resembling that met with in cancer. But this stroma, instead of containing the juice and the tissue proper to cancer, is filled with a whitish grumous substance,



in which microscopical examination discloses nothing but epidermal elements." A seventh species consists of epidermic tumours formed of cutaneous cysts, the investment and contents of which are composed of epidermis.

Ophthalmologists are much interested in the question of the precise nature and mode of origin of certain tumours of the eye, quite as certainly curable by operation as cancerous productions in that organ are beyond the reach of surgical cure. There is no doubt that some such tumours are purely fibrinous in composition, and that they are the reliquiae of extravasations of blood. M. Lebert gives a description of a little tumour of this sort which was seated in front of the retina, and probably owed its existence to hemorrhage from one of the vessels of the choroid. The tumour was of about the size of a cherry; it was of blackish-red colour internally, a fact showing that it was probably of recent origin. In the midst of the portions thus deeply coloured appeared others of a grayish-yellow tint, having precisely the aspect of coagulated fibrine. Delicately granular fibrine in coagula, "condensed under the form of muscular fasciculi," was alone discoverable under the microscope. Blood-corpuscles, more or less completely deprived of their colouring matter, were also visible.

M. Lebert very correctly raises his voice against the use of the term *sarcoma* as deficient in precision, and as having been applied, more especially perhaps in its compound form *osteosarcoma*, to all varieties of morbid growths; but M. Lebert is not the first who has perceived the error, or denounced its followers. That a true and real tumour exists, possessing an active power of growth, and assuming many of the outward characters of cancer, to which the name *sarcoma* has been applied, has long been known; and the difficulty of distinguishing the two kinds in all cases was made even more evident than it had before been by Müller, who showed, as he believed, that the microscopical constituents of the two growths might be, and were in some cases, perfectly similar, and also discovered that the chemical basis of both was albumen. M. Lebert, descrying the clinical importance of the distinction, engages in an attempt to establish it. He describes these tumours under the name of fibro-plastic tumours, and considers them to be composed of the species of globule of that name, of which we have already given a condensed account. The distinctions will then turn on the characters of the globule; and upon this point we must allow M. Lebert to speak:

"These globules are characterized by having a pale cell-membrane as their wall, and a nucleus with very clearly-defined and black outline under the microscope. The involucrum gives a mean diameter of  $\cdot 015$  of a millimeter, and the nucleus varies between  $\cdot 0075$  and  $\cdot 01$  of a millimeter. The entire globules are either spherical or ovoid; and it is only on superficial examination that they can be confounded with the globules of encephaloid. The latter are much paler; they are generally only found under the form of round or oval nuclei, tolerably isolated, larger than the nuclei of the fibro-plastic globule, and also containing nucleoli larger than those of the globule of sarcoma, which are scarcely to be seen under the microscope, except as small black points. The involucrum of the encephaloid globule, when well developed, is much less regular and commonly flattened, finely punctuated, often infiltrated with fat, either in the state of granules or liquid; whereas fatty elements are rarely met with in fibro-plastic tissue, and when they do exist in it are always present in small quantity. Cancerous globules may cer-

tainly be more or less elongated; but the intermediate degrees between the fibre and the cell, which characterize the elements of cellular tissue in progress of formation identical to that of the fibro-plastic tissue, are much less frequent in cancerous substance. *In pointing out the existence of such elements in encephaloid cancer, observers have fallen into the serious error of taking an accidental for an essential element; and the cellular tissue formed out of fibro-plastic globules in cancer (and which, it may be added, does not often occur therein under that form), no more constitutes its characteristic and essential element than the various fatty substances which exist in it much more frequently.*" (vol. ii., p. 133.)

It remains for future observation to decide whether the points of more or less nice distinction referred to by M. Lebert really prevail in the two kinds of cell referred to. We have printed a portion of the above extract in italics, in order to show that the idea of the passage was also struck out by Dr. Walshe. This writer, speaking in the volume to which we have before referred, of the production of fibre in the substance of cancer, observes :

"The spherical cancer-cell being formed, the production of the caudate cell follows as an effect of elongation of opposite points of its circumference. And these caudate cells eventually pass into the state of filaments, and form the elements of fibrous formation. Such is the generally admitted notion. But it appears to me that, as matter of theory, caudate cells must also be produced from spherical non-cancerous cells; inasmuch as fibres, in no important point distinguishable from those of natural cellular tissue, are produced in these growths. *And it is certain that the caudate corpuscles visible within cancerous tumours do exhibit different characters; though to classify them, and connect one more particularly than another kind with the subsequent fibre is at present beyond my power.*"

It appears from the admission in this last sentence, that if M. Lebert's statements be proved to be correct, he has advanced a step beyond our countryman.

The other microscopical elements which M. Lebert has met with in these tumours are—2. Mother cells which occasionally reach a twelfth of a millimeter in diameter, and contain eight, ten, twelve, or more nuclei and fibro-plastic globules in their interior. 3. The greater part of the mass is composed of fusiform corpuscles. 4. True fibres are almost always met with in tumours of this kind. 5. In fleshy sarcomata a fair portion of the growth is composed of very small globules, which only present from  $\cdot 005$  to  $\cdot 0075$  of a millimeter in diameter; they are possibly fibro-plastic nuclei.

In the author's description of fibrous tumours we recognize the evidence of minute and diligent study of these products, but there is no novel fact established or brought forward.

The account of enchondromatous and osteoid tumours is borrowed (with acknowledgment) from Müller.

M. Lebert's chapter on Cancer is elaborate and valuable. The chief fact put forth in it of novel character is that the cell of cancer is distinguishable by its properties from all other cells, either of morbid or natural origin. We shall not submit this chapter to analysis, however, as much of our present Number is devoted to the subject of cancer in connexion with Dr. Walshe's recent volume. But we believe that we may interest some of our readers at least by noticing a few points in which we have remarked, in perusing these two works, that the Swiss and English ob-

servers differ. Dr. Walshe lays singular stress upon the property possessed by cancer of infiltrating the natural tissues amid which it is developed. This property, indeed, he goes so far as to maintain gives cancer its place apart from the *GROWTHS* properly so called. M. Lebert also makes much of this character of cancerous formations. But the mode of explaining the phenomenon adopted by the two writers is very widely different. The explanations are thus given:

"Cancer," says M. Lebert, "like tubercle, possesses the property of being deposited by infiltration," [tubercle is not a *GROWTH*, however; so that here there is not any *expressed* difference of opinion from Dr. Walshe,] "because, being often constitutional maladies, they are deposited by a much greater number of capillary vessels, than are, in general, local hypertrophy and the adventitious production of a normal tissue." (vol. i., p. 246.)

This idea, not very elegantly expressed, certainly appears in contradiction with opinions taught by the author himself in other parts of his work. Thus he regards fibrous and certain other tumours as evidences, in certain cases, of a constitutional state, and yet these tumours do not infiltrate the circumjacent textures. The following explanation of Dr. Walshe, if not destined to be ultimately received, is at least less easily overturned:

"Considered abstractedly, there seems nothing very terrible in this property [of infiltration]. So clearly is this the fact, that it appears to me we must look elsewhere for an explanation of the evils practically connected with infiltration, than to the mere physical phenomenon itself; in a word, we must seek elsewhere some condition, of which the process is but a consequence or involution. But the natural place to look for this condition is *in the tissues themselves*, which undergo infiltration; and in these—in some special morbid change within them—must reside the source and origin of the process. And this view is confirmed by the fact, that there is nothing in the mode of vegetation of cancer itself to explain why it alone, among vegetating new formations, should possess the power of infiltrating the natural tissues."

We do not find in Dr. Walshe's work any distinct attempt made to establish an anatomical diagnosis (if we may so speak) between the cells of encephaloid, scirrhus, and colloid. The continuous enlargement of the cells of colloid, while those of scirrhus appear to retain persistently their primitive dimensions, and the fact that the nucleus of the encephaloid cell is larger than that of the scirrhus cell, are the chief points he draws attention to. M. Lebert aims, as follows, at more precise distinction:

"The globule of fibrous, hard, and *scirrhus* cancer is generally supplied with a membrane of investment of round, ovoid, or irregular shape. The mean diameter varies between  $\cdot 015$  and  $\cdot 02$  of a millimeter; it is finely punctuated round the nucleus. The latter is small, varying between  $\cdot 0075$  and  $\cdot 01$  of a millimeter in diameter, with a strongly marked outline, and exhibiting in its interior granules amorphous particles, and not unfrequently nucleoli.

"In the *encephaloid* globule the involucrum is regular or irregular, and of a mean diameter of from  $\cdot 02$  to  $\cdot 03$  of a millimeter. The nucleus is spherical, very often elliptical, pale, shaded at the periphery, and containing from one to three very distinct nucleoli. These nucleoli in general are more abundant than the complete cells." (vol. ii., p. 258.)

But whatever precision of information may be deduced from this comparison undergoes very serious modification from a statement which is



added, that "all intermediate forms between these two types are observed." The question is briefly this: Can the naked-eye characters (in other words, the species) of a cancerous tumour be always decided from microscopical inspection of its *elementary cell*? We are certainly disposed to believe it cannot; but we admit our inability to decide the question.

We notice a statement here by the author, that he has sometimes found a "fibroid rete," containing cancerous cells within it, and "perfectly resembling coagulated fibrine." This is an important observation, which has also been made by Dr. Walshe, who (*op. cit.* p. 58) draws an inference of some consequence from it, as to the production of blastema in connexion with extravasated blood.

M. Lebert conceives that the yellow discoloration often witnessed in cancerous tumours (particularly in encephaloid of the testicle) is due to a particular species of colouring matter, for which he proposes the name of *xanthosis*. Attentive examination has, he says, proved that this material is not altered colouring matter of the blood, but that it is constituted by a particular species of fat or oil.

There is little to interest in the account of cancer given by M. Lebert, except his purely anatomical and microscopical details. Some few vague observations on the influence of age and sex on the disease are really amusing from their simplicity. A distinct paragraph, for example, is devoted to the important announcement: "In general, I *think* that cancer is more frequent in the female than in the male." Think! And why should not M. Lebert think? A man of his powers is doubtless much more likely to come to accurate conclusions on such a question by thinking, than by consulting the statistical tables supplied by the government returns in this country, and the numerous valuable numerical communications of various foreign writers on the subject.

Two detached essays, one on the formation of callus, the other on the minute anatomy and treatment of true porrigo favosa, close the volumes. There is no new mode of treatment of the latter affection suggested, except that of picking out with the point of a needle the little vegetables which constitute its essential element. At least this mode of treatment is new to us, though possibly it may have already been put in practice by others. M. Lebert speaks well of the iodide of sulphur; but we *know* that it will repeatedly fail, and doubt its ever having cured the disease. The whole account of this disease is, however, very excellently done, and will well repay perusal.

Had the work we have now passed in review been put forward as a mere compilation, accompanied with illustrative observations—had it been less crowded with the author's self-gratulations—had it not started with an announcement that M. Lebert commenced his medical studies with a sort of contemptuous pity for the false philosophy prevailing among his predecessors—had justice been done to those predecessors—had any shadow of improvement been made upon the philosophy of those predecessors—had all this been the case, M. Lebert's claims to the courteous reception of his brethren would have been far different from what they now are. While we do not see that he has added anything of real consequence, either in fact or doctrine, to what was already made out, he has, however,

produced a work of laborious and, in some respects, useful character. Yet, on the other hand, his accuracy of description may often be doubted. We do not, for example, believe in his tuberculous corpuscle, nor in his "pyoid" corpuscle. Nor can we avoid commenting unfavorably on the figures in his plates. Many of the objects are new to us—of these we therefore say nothing; but wherever the thing represented is one with which we are familiar, the wretchedness of the delineation has deeply impressed us. In fact, we can affirm that it requires much familiarity with the objects to guess what they mean; and, in a word, the figures are apparently made from the *descriptions* of the objects, and not from those *objects themselves*.

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### ART. III.

*Glanders and Farcy in the Horse*. By WILLIAM PERCIVAL, M.R.C.S.,  
Veterinary Surgeon of the 1st Life Guards, &c.—*London*, 1845. 8vo.

THE diseases of those domestic animals which are the constant associates of man are entitled to our best consideration. Comparative anatomy has contributed largely towards the advancement of human physiology, in dispelling error and eliciting truth in various points connected with the economy of the functions; so likewise comparative pathology may be made subservient to that of the human subject. Until recently, however, veterinary medicine has received but little aid from those whose province it was to cultivate it; for the researches of all the older writers upon this subject are a singular compound of fanciful speculation and irrational empiricism. We seek in vain in veterinary works for unity or precision in the various accounts of diseases of the lower animals; in which the pathological conditions are either wholly overlooked, or else so imperfectly described that we turn away in despair from descriptions which seem to have no other purpose than that of mystifying the nature of disease.

Brute pathology is still almost wholly unexplored; yet there is no department of science which offers a wider field of interest and instruction to the medical philosopher than the pathological history of the mortal diseases of those animals which approach nearest in the scale to man. The extreme facility with which several of those maladies are transmitted from the brute to the human subject, and their invariably fatal termination in the latter, are facts sufficient to enlist our sympathy and arrest our attention. If further stimulus were wanting, we have it in the consideration that a rigid inquiry into the causation of the class of diseases referred to cannot fail to expand our views by disclosing new facts, elucidating points hitherto obscure in human pathology, and otherwise contributing to improve the science of medicine. In the most important of all the branches of medicine, therapeutics, a virgin field of the greatest possible value lies here before us; where at least we may, with a safe conscience, and without trepidation, endeavour to ascertain the natural history of diseases, the remedial powers of nature, and the positive effects of medicines. Although we shall not be justified in reasoning positively from the results thus obtained, as to results to be obtained in the human organism, it cannot be doubted that the strong analogical grounds thence derived will far exceed

the majority of those on which we are accustomed to rely in practical therapeutics.

The murrain, an epidemic disease of cattle as fatal and as mysterious in its operations as the cholera, is transmissible to man. Yet this malady has not been described in medical works, and doubtless is considered as foreign to the subject of medicine as glanders was some twenty years ago. Men have died of this disease; and cases are recorded in which animals and their attendants have been attacked with the epidemic, exhibiting symptoms precisely similar, under which both have finally succumbed. "Those persons," says Dr. Slare, in an account of this epidemic, published in the thirteenth volume of the '*Transactions of the Royal Society*,' "that carelessly managed their cattle, without due regard to their own health, were themselves infected, and died away like their beasts."

It were easy to relate facts showing the great importance of the subject of brute pathology with reference to state policy, as well as in a medical point of view; but as a branch of hygiene its importance is so obvious that we feel confident the day is not distant when a "Board of Health" will see the necessity of preventing, by stringent measures, so enormous and palpable an evil as that of animals infected with a mortal disease, and snorting a poisonous virus from their nostrils, from plying in the crowded thoroughfares of the metropolis and of other large towns.

Taking a lively interest, as we do, in the progress of comparative pathology, we looked forward with no small degree of pleasure to the perusal of Mr. Percival's contribution to that science now before us. A monograph on glanders and farcy emanating from such a respectable source led us to expect more, perhaps, than the present condition of veterinary medicine warranted. Be that as it may, however, the work before us has not fulfilled our expectations. It may be useful to refer to as an epitome of what has been written on these diseases in the quadrumana; but beyond this its utility does not extend. Of novelty it contains nothing. It leaves the pathology of glanders precisely where it was before; and, in fact, fulfils no other end than the subordinate one of supplying a text-book of what has been already done upon the subject. As, however, the volume may be considered to express the views of the veterinary profession on the nature of glanders in the *horse*, a summary of the principal facts it contains may be read with profit, and will tend to facilitate the study of the disease in man.

Mr. Percival's work consists of three parts. The first embraces an account of the "symptoms, diagnosis, varieties, causes, seat and nature of glanders." In the second are described the "symptoms, causes, seat, and nature of farcy;" and the third is occupied with "the treatment of glanders and farcy." In the body of the work there are about twenty pages describing the author's own views on the subject, which are, in point of fact, but an amplification of those of M. Leblanc, of Alfort.

The following is Mr. Percival's definition of glanders: "Glanders consists in a discharge from one or both nostrils of matter which, by transfer or inoculation, will produce the disease in another animal (of the equine or human species), and which discharge is sooner or later accompanied by vascular injection and chancrous ulceration of the schneiderian membrane, by tumefaction of the submaxillary lymphatic glands, and by farcy." This de-

inition is imperfect. All mention of propagation of the disease by *infection*, or through the medium of the atmosphere, is omitted; and to prove that the disease is not altogether confined to the "equine or human species," we may mention the fact, which came under our own observation, of a dog which sickened and died with symptoms very similar to those of glanders, after inoculation with the virus of that disease taken from the horse. We may also refer to the 'Bull. de l'Acad. Roy. de Méd.,' (31 Dec., 1839, et 15 Fevr., 1840,) in which M. Renault relates two cases of glanders produced in the sheep by inoculation; and to M. Hamont's 'Memoirs on Glanders,' where the disease is described as having been transmitted, by inoculation, to a lion and to dogs.

**SYMPTOMS.** The symptoms of glanders in the horse present several peculiarities which are not met with during the progress of that disease in the human subject. One of the most singular of these is the apparent restoration to health of the animal after the febrile indisposition usually present during the incubation of the disease, has subsided, and the discharge from the nostrils is freely established. This peculiarity is the cause of much mischief; for it blinds the incautious and unprofessional observer, who naturally enough supposes there is nothing the matter with the horse further than "a running from the nose and kernels," which may be the result of cold; and hence the propagation of the disease to other animals is facilitated. How different is the progress of glanders in man! Here its brief course is unbroken by any of these deceitful signs of returning health; and the only termination of his sufferings which the patient can hope for is death itself. As a general rule the first external physical sign of the disease developed in the quadrumana is the enlargement of the submaxillary lymphatic glands; but unfortunately the first symptom that *attracts notice* is the discharge from the nose. This discharge is at first scanty and limpid, and usually trickling from one nostril only. In the course of a day or two it becomes thickened with mucous and albuminous matter, loses its watery appearance in a great measure, and quickly passes from a limpid to a glairy condition, finally to become of a yellow tint,—and this terminates the first stage. The second stage commences with decided appearances of purulent matter, scanty at first, but soon flowing abundantly; this gradually thickens, becomes as glutinous as birdlime, impedes respiration, and produces a snuffing noise in the nostril not unlike the mucous or bronchial rhonchus. This latter symptom is decisive as to the nature of the disease. As the destructive ulceration of the bones and cartilages of the nose advances, the discharge changes from yellow to green, or to a dirty brown or leaden colour, streaked with blood, and gives rise to a horribly fetid and disgusting smell, which marks the last stage of the complaint, and continues unabated until dissolution takes place. This is the regular course of the acute form of glanders: when it assumes a chronic character the discharge may continue from the first aqueo-mucous, or turn to a nasal gleet only. The discharge of the acute form when analyzed shows albumen in abundance, from the presence of which, perhaps, it derives its glutinous character. The diseased parts present the usual appearance of inflammation—vascular injection, thickening of the pituitary membrane, infiltration of the cellular tissue, ulceration, &c. Of these different lesions, the only one which has a specific character is the ulcerative pro-

cess, and that presents one or two striking peculiarities pathognomonic of the disease. We shall relate these in the words of the author :

“Introduce into a scratch the *virus* taken from a glandered animal, and the result will be that, losing all disposition to heal, the lesion will inflame and secrete an ichorous matter, and become converted into a transparent vesicle, surrounded by an *areola* or circular blush upon the membrane. The next day the vesicle has broken, and we perceive in the place of it a pale, foul, superficial ulceration, which in the course of another day acquires the genuine character of the glanderous *chancre*, an elevated, circular, pinkish border, including a base of faint yellow albuminous matter, which on being wiped or irritated bleeds, and on the matter being removed exposes, when the ulcer is deep, the bare cartilage beneath ; when superficial, a red, spotted, rugged, foul, bleeding bottom. From its tendency to spread, the ulcer speedily loses its circular figure, for one too irregular and variable to admit of precise description ; it has, in fact, now become a foul, spreading ulceration, extending on every side, coalescing with similar ulcerations in its vicinity, having for its base the cartilage of the *septum nasi*, which alone, from its comparative insusceptibility of the ulcerative action, puts a temporary arrest to its devouring activity.”

In virulent forms of the disease the turbinated bones are often ulcerated through into the nasal sinus, and Mr. Percival has seen cases in which these bones have been almost entirely removed, through the ravages of the ulcerative and exfoliating processes. The chronic form of glanders presents a different variety of ulceration but equally characteristic, which is called by veterinary writers “miliary ulceration,” giving to the diseased parts the appearance of worm-eaten wood, or as if the membrane was covered with pin holes. This condition is the result of the degeneration of miliary tubercles, according to M. Dupuy. He describes the ulcerations as being superficial, having thin, irregular edges, and following the course of the large veins upon the septum ; they are frequently mistaken for the dilated orifices of the mucous follicles. Tumefaction of the ala nasi is a frequent symptom of the acute form of glanders, and when present is highly characteristic of the disease. It is seldom seen in the subacute or chronic forms. The left or near side of the head is considered by several veterinary writers, and amongst others by M. Dupuy, to be much more frequently the seat of the disease than the right ; an opinion which is not substantiated by facts, and which our author very properly discards, in the absence of more precise data than the mere assertions of writers however respectable.

**DIAGNOSIS.** The diseases with which glanders is most likely to be confounded are catarrh, nasal gleet, and strangles ; but the characteristic features of the former when developed are so striking that a mistake in the diagnosis is not likely to occur to any careful observer. “The signs by which the disease may be known,” says Solleysel, “are when a horse too old to be troubled with strangles, without a cough, voids matter by the nose, and has a kernel sticking to the bone ; and besides in glanders the matter usually flows from one nostril, whereas in a cold it runs almost always out of both.” It sometimes happens, however, that some local disease about the head, neck, or lungs, excites a discharge from the nose, and when this discharge flows from one nostril only, it is likely to be mistaken for glanders. A carious tooth, for example, may produce symptoms of this kind ; hence the necessity of carefully examining into the history and origin of these symptoms before forming the diagnosis, and in doubtful



cases time should be allowed for their development before the animal is condemned. The most effectual way, however, of establishing a diagnosis is by inoculation of another animal, in good health, with the matter taken from the suspected glandered sore. The result of this experiment will finally settle the question.

**VARIETIES.** Mr. Percival recognizes three varieties of glanders—the *acute*, *subacute*, and *chronic*—a division which has the merit of conciseness, and is preferable to the involved and complicated classification of the early veterinary writers; for, after all, the disease is essentially the same in all its types, and its variations are merely in degree, and are modified according to the tissue and locality in which it is principally seated, as the lungs, the schneiderian membrane, the sinuses, &c. Mr. Percival considers what he calls “inoculated glanders” to be the best example of the acute form of the disease, the symptoms of which we have already described; the “typhoid glanders” of French writers being no other than a malignant form of this variety.

Mr. Percival describes a variety under the name of “*pulmonary glanders*,” which supervenes on the subacute, and even on the chronic species, whenever the lungs, in which the disease has been either unceasingly or in relapses creeping on all the while, have arrived at a point of disorganization to fail in their functions, and so as to create constitutional irritation. The author has remarked in these cases that the disease has generally confined itself to the lung of the side corresponding to the affected nostril. The animal's health and strength continue to decline for months, and sometimes for years before the final break-up takes place. In this variety death is never occasioned by suffocation as in the acute form, but, to use the author's expression, “through consumption of the lungs by the disease, assisted by a wearing, hectic sort of fever, the same as is seen in human phthisis pulmonalis; and, moreover, bearing a resemblance to that mode of ending life, inasmuch as the brute as well as the man, retains his senses to the last.”

We wish the author was more precise in explaining what he means by “consumption of the lungs.” Does he mean tubercular phthisis, or merely pulmonary abscess? If the latter, which we suspect to have been the cause of death in these cases, there was no necessity for a separate description of “pulmonary glanders,” which is nothing else but an extension of the disease from the nostrils to the lungs in cases of long standing, and does not require to be described apart any more than “glanders of the frontal sinus,” or “glanders of the schneiderian membrane.”

*Subacute glanders* is the variety most frequently met with. After the disease is established the symptoms gradually subside, and remain in a state of inactivity for a considerable period, until some occurrence takes place to derange the health and excite the disease into renewed action. It then assumes the acute form, and progresses rapidly to a fatal termination.

*Chronic glanders* differs both symptomatically and pathologically from the acute and subacute forms of that disease. As regards symptoms, it differs from them in the absence of any thing like inflammation, or vascular injection, or chancre, or, in fact, of any perceptible change in the aspect of the schneiderian membrane denoting morbid activity. The flux from the nose

and the submaxillary tumefaction being the only indications of a deviation from health. It differs pathologically from the varieties above named in selecting for its especial seat the lining membrane of the sinuses of the head. Some veterinarians allege that a slight prominence is to be felt over the frontal sinus, and when the part is tapped with the knuckle the sound elicited is duller than natural, and the parts tender to the touch; but this appears to be mere speculation, and is not supported by facts. It is in this variety of glanders that the peculiar ulcerative process we have noticed in a former page, called "miliary ulceration," is observed.

During the perusal of this section of Mr. Percival's monograph we have been forcibly struck with the two leading faults which prevail throughout the work, and which materially detract from its merits. The faults we allude to are want of order, and a vague, loose method of expression in his pathological descriptions. The *lucidus ordo* seems to be unappreciated or not understood by the author, and, as might be expected, confusion and frequent repetition are the result. Although there are separate sections devoted to descriptions of the "symptoms" and "diagnosis" of glanders, in the present section—which, without inflicting any injury on the history of the disease might have been incorporated with the first,—we have the same story over again. We have detailed accounts of symptoms already described, and as a wind-up to the section we are favoured with two pages on diagnosis, although that subject had been already discussed in the preceding chapter.

**CAUSES.** The origin of glanders is a subject of extreme interest, and seems, from the very dawn of veterinary pathology, to have been a *questio vexata* with the writers on and professors of the veterinary art. Although the contagious nature of the disease is now established beyond all doubt, there are still writers, and respectable practitioners too, who zealously support the opposite opinion. The arguments of this class of reasoners are frequently put forth with such total disregard to the commonest rules of logic as well as of physiology, that the dispassionate reader is inclined to view them rather in the light of party contentions than as attempts at the solution of an important question in science.

Mr. Percival considers the causes of glanders to be twofold—predisposing and exciting. Horses of an inferior breed, ill-fed, ill-conditioned, and over-worked, are highly obnoxious to this disease; the ass more so than the mule—the mule than the horse; and it appears from statistical tables that horses of the adult or middle ages are much more frequently attacked than foals and geldings. Mr. Percival does not seem to be acquainted with the valuable memoirs of M. Hamont, read before the Academy of Sciences in 1842, nor of the interesting work of Dr. Levin on the transmissibility of glanders from animals to man, reviewed in this Journal (January, 1842). Mr. Percival says the disease "is absolutely unknown in hot climates, in Arabia and Africa, to which we may add India," whereas M. Hamont states the very reverse; and from his position as director of the veterinary school at Abon Zabel, in Egypt, ought to have some weight. The latter writer describes glanders as of frequent occurrence amongst the inferior horses of the poorer Bedouins, whereas the blood horse in Egypt scarcely ever falls a victim to the disease. He considers glanders to be a disease of privation (*misère*), and only attacks impoverished animals, whose con-

stitutions are broken down by over-work and bad feeding, or those of a deteriorated breed. Solleysel, who wrote in 1669, seems to have had a pretty shrewd notion of this disease. He describes glanders as one of "the most contagious distempers to which horses are obnoxious; for not only does it communicate its venom at a small distance, but infects the very air, and seizes on all horses that are under the same roof with him that languishes from it. There are several kinds of glanders, some of which are not so infectious as others, though there are none that ought not to be suspected." If the foregoing statement was qualified by the remark that all horses are not equally susceptible of the disease, and that instead of there being several kinds of glanders (for it is essentially the same disease in all its forms), it is more contagious at one period than at another, and that its transmission from animal to animal is influenced by the ordinary laws of animal poisons, it would not be far from the truth.

We cannot give a better illustration of the vague and indefinite expressions and want of precision of Mr. Percival's style, than by noticing the indiscriminate use by him of the words "contagion" and "infection." In the same work they are sometimes used synonymously; sometimes to express different meanings; so that it is almost impossible to arrive at what the author really means. The term "contagion" is usually meant to imply the propagation of disease by actual contact, in contradistinction to "infection," which is understood to express the propagation of disease through the medium of the atmosphere—at least this gives a distinct and definite meaning to each of these terms, and would render the author's views much more intelligible. In the following remarks we shall use them in this signification.

Amongst the non-contagionists the most formidable were Dupuy and Coleman, and certainly these names are a host in themselves. Coleman says that not one horse in ten thousand receives the disease through contagion! He adds "the poison of glanders is bred and diffused in an atmosphere rendered impure by repeated respiration, and by gaseous impregnations from the dung, urine, and perspiration emitted in hot and foul stables." In support of this view he alleges that it rarely or never spreads amongst horses at pasture, though a glandered subject may have been grazing amongst them. With every respect for Mr. Coleman's name we dispute the accuracy of the above statement, and cannot but believe that if he had written at a later period he would have materially modified his views. When we find such passages as the following in his work, totally at variance with *facts*, it is not too much to suppose that further experience would have led him to alter his opinion, that not one horse in ten thousand died of glanders through contagion. "If," says Mr. Coleman, "*man had been susceptible of contracting diseases from horses, oxen, hogs, sheep, &c. &c.*" We believe, however, that Mr. Coleman did subsequently admit the transmissibility of the disease from the horse to man. As a specimen of the kind of arguments often advanced by the non-contagionists we submit the following from a work on glanders, by Mr. Smith, a veterinary writer of some note: "If the mucus issuing from the nostrils of the horse be so infectious as it is generally supposed, how is it that those animals which have access to the places where they stand, and in which they are frequently confined, escape the disease—especially dogs, who also feed on such horses



immediately after death? It is very well known that horses are affected with hydrophobia when bitten by a mad dog. It is reasonable, therefore, to suppose that if glanders were equally contagious the disease would be equally reciprocal." This writer evidently forgot, or did not know, the recognised fact that all animal poisons are governed in their operation by certain laws peculiar to themselves; that one animal is more susceptible than another; and that the same animal is more susceptible at one time than another. We may inoculate the disease without success from insusceptibility of the subject inoculated, as we see every day in vaccinating children, on the same principle that we may administer the same quantity of aloes to two horses, and effect violent purgation in one, but make no impression upon the other. The fact that the most virulent animal poison may be swallowed with impunity, whereas if applied to an abraded surface would produce instant death, seems also to have been unknown to this writer, although familiar enough to most people. M. Leblanc, of Alfort, an eminent veterinarian of the French school, believes that glanders and farcy are infectious in all their forms, though in different degrees.

Mr. Percival, who was educated in the Coleman school, is now a decided infectionist, although in his early life he was opposed to that view. A series of facts illustrative of the transmission of glanders through the medium of the atmosphere, occurring under his immediate observation, convinced him of the fallacy of his former views, and led him to adopt those he now entertains.

The 1st Life Guards, to which the author is veterinary surgeon, was ordered on the 14th March, 1833, from the Regent's-park Barracks to out-quarters at Barnet, Highgate, and Hornsey. The horses were in good health at the time, and no case of glanders had occurred in the regiment since the author joined, about seven years previously. They remained in out-quarters eight days, and the day after their return the colonel's horse first, and subsequently three others, became infected with glanders. It appears that these places are notorious for propagating glanders; which is accounted for by the number of hard-worked horses crowded together in them constantly, to supply the public conveyances on the great northern roads. On the horses returning to their own stables, and those diseased being removed from the healthy animals, the malady soon disappeared.

Mr. Percival's view of the *modus operandi* of contagion (infection?) in this as in all other similar instances is as follows: "Glandered horses having formerly tenanted these stables, the animals infected became so by inhaling the effluvia caused by moisture and heat to arise from the desiccated besmearments, and that these effluvia entering with the air into the animal's air passages, therein became absorbed, and thus infected his system; the contamination, therefore, mediate instead of immediate, breaking out afterwards in the form of glanders and farcy." The author does not believe that infection was produced by contact of the animal with the debris of glandered matter adherent to the woodwork of the stable or elsewhere. He believes that the aerial membrane is the medium through which the virus of glanders becomes introduced in cases of infection; in fact, that it is inspired. Here again we have to complain of our author's vicious style. From a carelessness of expression he is made to contradict himself in the most direct manner. After stating as we have just seen that it was by in-

haling the morbid effluvia that glanders was developed in the cases above mentioned, he begins the following paragraph with the query—"Can glanders be propagated through the medium of the air?" and answers it thus: "I think myself it is possible, *though a very unlikely incident*, that the air may become the medium of contagion!" We should like the author to explain what he means by "effluvia" if it is not "air" modified, but still air. However, on reading a little further, we discover that the author was in a reverie, from which he awakes with this notable reminiscence: "Indeed, if we come to consider, the air ought to be regarded as the communicating medium in the cases of stables, &c.!" and a few lines further on, when the author's memory had more completely returned, he says, "the probability, therefore, is that the air plays a more important part in the ordinary work of contagion (infection?) than we are in the habit of imagining." (p. 238.) Not a bad *amende* this for the previous obliviousness. This, however, is not all. At page 243 we find the author making further admissions as to atmospheric infection, which we cannot resist noticing, especially as they favour our own views of one of the causes of glanders. Coleman, as the reader will recollect, believed that the ordinary source of glanders was "a poison generated in a confined atmosphere, out of exhalations from the breath, the dung, the urine, and the perspiration of horses pent up in it:" and this is what Mr. Percival calls the "miasm of the stable." In commenting upon Coleman's view, the author observes:

"To my mind, however, Coleman's own reasoning on the *modus infectandi* of this poison is in every way sufficient to prove that the disease once generated is capable of spreading by contagion, and through the medium of the atmosphere, too, from one horse to another. If the atmosphere of the stable, charged as we know it to be with humidity, can carry a miasm from the excretions and secretions into the nose of the horse, sufficiently concentrated to produce glanders, is there any good reason why the same atmosphere may not convey the virus of glanders itself emanating from the nose or lungs of a glandered horse? Surely that which can conduct poison from the dung or urine upon the floor of the stable can transport virus from one horse's nostrils into those of another."

While rejecting the doctrine of Coleman, the author has at length arrived at a correct view of the case; and in spite of this contradiction we are willing to give him credit for the correctness of these incidental observations.

We have ourselves paid considerable attention to the etiology of glanders for several years past, and we shall now state in a few words the result of our observations as to the origin of that disease. It appears to us that glanders may be produced in three different ways: 1st, Spontaneously, through privation, over-work, and foul stabling; 2d, by infection, i. e. by inhalation of the effluvia of a glandered animal, or of the atmosphere of a stable wherein a glandered animal was kept; 3d, by contact or inoculation.

1. We have observed that the same depressing agents which predispose to fevers of a typhoid character in man, by lowering the *vis vitæ*, predispose to glanders in the quadrumana, especially when broken down by age, over-fatigue, bad feeding, and more particularly if the animals are of an inferior breed. We have seen the disease on several occasions developed under these circumstances in large, airy, and well-ventilated stables, and

perfectly coincide with the statement of M. Hamont that moisture and cold, narrow, and ill-ventilated stables will not of themselves produce glanders. Take a horse of a good breed and in health, feed him well, give him ordinary exercise, and put him into the coldest and dampest stable you can find, provided it is not glandered, and we feel confident he will not become diseased; but if you over-work an inferior horse, feed him badly, and keep him in the best ventilated stables that Mr. Coleman could devise, the animal will in all probability become glandered; of course allowing for the caprice of susceptibility. We have also noticed that the higher breeds of horses rarely ever generate the disease, i. e. spontaneously, unless very old and then over-wrought. We perfectly agree with M. Hamont that the *original* causes of glanders do not exist in stables, and that the habitation exerts but a secondary influence towards its development.

2. That glanders is transmissible from one animal to another through the medium of the atmosphere we firmly believe, and we have adopted this view from experience, for originally we did not entertain such an opinion. There is now no doubt in our minds that glanders is infectious, and can be transmitted from animal to animal through the inhalation of a contaminated atmosphere, precisely as hooping-cough or scarlatina passes from one human subject to another. We have seen the disease reproduced in a stable where there was no possibility of contact with glandered matter, for the woodwork was either painted or removed, and the walls were replastered and the pavement altered; and although some months had elapsed since it was occupied by the glandered patient, and after these alterations were made, still the first horse that was put into it again took the disease and was destroyed. Dr. Burgess, who entertains similar views of the infectious nature of glanders, thus expresses himself on this subject: \* "Glanders and farcy are essentially contagious diseases, whether developed in man or the quadrumana. They are, moreover, decidedly *infectious* as well as contagious in the latter, i. e. the contagious principle may be transmitted through the medium of the atmosphere, as well as by actual contact from one animal to another. I have known several instances in which there was no possibility of contact with glanderous matter, and yet the disease was developed in healthy horses. A gentleman of fortune in the west of Ireland had his stud of horses infected with glanders. Every particle of woodwork in the stables, including stalls, rack, manger, &c., was taken down or replaced with new materials. The plastering on the walls was completely removed, and the pavement ripped up, and all was replaced with entirely new work; but the first horses that were again placed in these stables became infected, and they were ultimately razed to the ground. It would even appear that the contagious principle remains for a lengthened time, sometimes for years, in any stable or shed where glanders or farcy may happen to be developed." It is recorded that when the Duke of Marlborough took Lisle from the French, glanders broke out there among the horses with such virulence that the stables were obliged to be shut up, and remained so for thirty years, until another war broke out, when they were opened again, and the horses that were put into them became immediately glandered. That animals often escape the disease,

\* Cazenave's Manual of Diseases of the Skin: art. Glanders. By T. H. Burgess, M.D. London, 1842.

although breathing a contaminated atmosphere, we freely admit, and have seen several remarkable instances of this kind ; but that circumstance does not in the slightest degree affect the doctrine of infection. We have merely to refer to the general laws of contagion to render this argument against infection invalid. Six persons may visit a patient in typhus or scarlet fever, and one or two may contract the disease, whilst the others escape ; yet no one would think of saying that typhus or scarlatina was not infectious, but that all these individuals were not equally susceptible of the disease. It would even appear that glanders has been communicated to man through the medium of the atmosphere, and Mr. Percival is labouring under a mistake when he supposes that man has taken the disease in no instance save through inoculation. (p. 225.) In the 'Lancet' for Feb. 11, 1832, the author will find a case of glanders transmitted from father to son without inoculation. In the Bull. de l'Acad. de Médecine, for Nov. 1841, he will find the case of a dresser who had the care of a glandered patient and contracted the disease, "not by inoculation, but in the same way that small-pox or scarlatina is contracted—by infection." And Dr. C. Williams relates a case of a girl sleeping over a stable where a glandered horse was kept, who became affected with a disease very analogous to glanders, although she did not come in contact with glanderous matter. (Med. Gazette, Dec., 1841.) Dr. R. Williams, in his work on 'Morbid Poisons,' relates another case. Also Tarozzi, an Italian writer, gives a graphic description of a pestilential disease which was developed in a stable where a glandered horse died. Out of thirty-five persons who visited that stable eleven were attacked with a malignant complaint, characterized, from its invasion to its termination, by fever and an eruption of boils and gangrenous pimples. These facts, however few, show us the possibility of contracting a malignant disease from the horse without contact, and teach us the necessity of precaution in going about glandered animals or their stables. That the majority of persons similarly situated escape infection is no argument against the danger of the disease.

3. The propagation of glanders by contact or inoculation is so fully established already, and especially as the author has nothing new on that subject, we shall not enter into it at present, but proceed at once to the next section of the work.

*Seat and nature of glanders.* That glanders and farcy are fundamentally the same disease, resulting from a common cause,—a specific poison—and differing from each other only in situation, is, we believe, now pretty generally admitted. That the aerial membrane, including that of the sinuses, is the *seat* of glanders proper, is also a proposition which the experience of veterinary pathologists has fully established. Dupuy, as we have already seen, was of opinion that glanders attacked the membrane of the frontal and other sinuses first, that of the nose next, and finally the lungs ; whereas Rodet, the expounder of this doctrine, alleges that the schneiderian membrane is only affected secondarily, through extension of the disease from the lungs. Mr. Percival adopts the views of Dupuy on this point, in preference to those of Coleman, who considered the *septum nasi* to be the part first affected, and peculiarly susceptible of the disease. "Next to the sinuses of the head," says Mr. Percival, "we find the lungs taking on the disease ; not by direct extension of the morbid action to

them through the windpipe and its branches, but from the continual inhalation of the glanderous effluvia arising from the diseased surfaces in the nostrils and sinuses. The lungs, however, do not prove diseased in all cases of glanders." (p. 283.) The larynx is not infrequently the seat of glanders, but the author is of opinion that there is not the same susceptibility in the "membrane lining the windpipe as in other parts of it, or even in those divisions of it constituting or lining the pulmonary air-cells."

Mr. Percival enters, *con amore*, into the views of the pathology of glanders propounded by M. Leblanc, of Alfort, in an admirable monograph published by him in 1839. The author tells us that he is indebted to accident, a few months before writing his present article on glanders, "for a small work wherein, to his great satisfaction, he found notions entertained such as for many a year had been floating about in his own mind." It appears somewhat singular that M. Leblanc's important memoir should have escaped the notice of a writer of a text book on veterinary medicine for nearly five years, and then that he should be indebted to accident for its discovery. However, when he did find it out, he makes up for former negligence by adopting wholesale the doctrines of that excellent veterinarian. The principal novelty in M. Leblanc's views is—that glanders, like farcy, is a disease of the *lymphatic* system; that the pimples or tubercles observable in the incipient stages of glanders are nothing more than so many farcy-buds, which in time become pustules, burst, and end in turning to so many open, foul, and spreading ulcers,—in short, that the little eminences which Dupuy called "tubercles" are nothing more than so many farcy-buds, and that these consist of albuminous matter. M. Leblanc considers the pulmonary glanderous tubercle to be precisely the same morbid product as a farcy-bud, and not, as Dupuy supposed, identical with the tubercle of phthisis in man. Mr. Percival, with whom we perfectly agree, does not believe there is any analogy between the elementary lesion of glanders in the horse and that of phthisis in the human subject.

M. Leblanc thus explains the alteration which takes place in the lymphatic fluid during the progress of the disease. It first turns yellow and becomes coagulated within the canals of the lymphatics and the cavities of their glands, the tunics of the vessels thickening and turning opaque, exhibit red points upon their inner surface, and adhering in places to the coagula within, and in other places growing more or less softened, without, as yet, showing ulceration. In time all the thickened parts of the vessel partake of this softening, spreading from a single point upon its circumference, the coagulum within softening likewise, and the cellular tissue corresponding to the point of ulceration becoming tumefied, then hardening, and lastly softening. And now a little tumour exists, having its seat in part in the lymphatic vessel, in part in the cellular tissue, close upon the situation of the lymphatic valves, which accounts for the elevations, the lymphatic fluid in its incrassated or coagulated condition not being able to pass the valves. This explains the knotted aspect of the corded swellings in farcy. M. Leblanc explains his views of the nature of what has been called the "glanderous tubercle" in the following terms, with which Mr. Percival coincides:



“Examination of the mucous membrane of the nose of a glandered horse will show, in a certain stage, that it becomes *thickened*. And that this thickening, which is owing to an accumulation of fluids of a white or whitish-yellow colour, precedes the appearance of the tubercles, the same as tumefaction of the cellular membrane precedes the formation of farcy-buds. In this (thickened) condition the membrane assumes a shiny and more humid aspect than it has in health. Then, upon divers points of its surface, and notably upon the middle part of the nasal septum and within the doubling of the nostril, make their appearance little white or yellowish-white pimples (*élévures*), rather prominent at their centres, with borders insensibly declining to a level with the surrounding membrane. *These pimples or tubercles correspond to the course of the bundles of lymphatics, and very probably have their seat in those vessels.* ‘At least,’ continues Leblanc, ‘I have been able to prove that little shreds (*masses élongées*), in composition absolutely like what is found within the lymphatics of a farcied limb, were inclosed within their canals, from which it was easy, with the points of the forceps (*d’un instrument*) to extract them: they proving adherent only in certain places, marked by some increase of redness. I have found the greatest analogy between these alterations and those which the lymphatic fluid commonly undergoes in farcy.’” (p. 288.)

We perfectly agree with Mr. Percival in his views of the specific nature of the *virus* of glanders, in fact, that no other animal poison will produce that disease but its own; but we differ from him when he says that the lymphatic system is not liable to derangement or inflammation from other causes. Septic matter, as Rayer observes (*Cyclopædia of Surgery*, art. Farcy), different from that either of farcy or glanders, may give rise in the quadrumana to inflammation of the lymphatic vessels and ganglions, accompanied by general phenomena of infection, and often terminating fatally; but nevertheless it is not glanders. We agree with the author in looking upon glanders as a *constitutional* and not a local disease. The constitutional disturbance, the eruption of the disease in other parts of the body in the form of farcy, the contaminated state of the blood, as shown by transfusion into another animal, and the inefficacy of topical remedies, are surely sufficient to prove that the disease is not local.

FARCY.—As the history of this disease is for the most part involved in that of glanders, we shall be brief in our remarks upon this part of Mr. Percival’s work.

The disease called farcy consists of small tumours along the course of the lymphatic vessels, in the form of a knotted cord, which in time ripen into pustules, and terminate in ulceration. The disease most frequently attacks one or both of the hind legs. On viewing the limb from behind a fulness on the inside of the thigh may be seen, along the course of the femoral vein. Tracing the cord upwards from its place of origin, which is usually above the hock, the hand is carried into the groin, and there discovers a lobulated tumour, a swelling of the inguinal glands, which is called a bubo. The disease is sometimes developed in the course of a night, whilst on other occasions it appears in a more insidious manner. It may assume an acute, subacute, or chronic character. The solid buds gradually grow soft, from centre to circumference, and at length become *pustules* or little abscesses, which burst as soon as they are ripe. Instead of breaking, however, the farcy-bud sometimes hardens and becomes indolent, and remains so for a considerable time, but ultimately gives way, and, like the former, terminates in ulceration of a chancrous

nature. Farcy seems to have a peculiar predilection for parts where the skin is thin and nearly free from hair. The inner aspect of the thighs and arms, lips, nose, &c., are most commonly attacked. Mr. Percival seems to be at a loss to account for this result, although it seems clearly enough to be owing to the number of large vessels and lymphatics which traverse these regions. Coleman supposed the reason of the hind legs being most frequently affected was in consequence of their "distance from the central force of the circulation." Mr. Percival thinks it depends chiefly on the extra-work the hind legs have to perform in progression.

There is a variety of farcy called "button farcy," which does not select the course of the lymphatics especially for its seat, but is diffused in the form of numerous small tumours over the body, and as they remain small through all their stages, the author thinks they bear a resemblance to the *miliary* ulceration of glanders. They are also more intimately connected with the *cutis vera* than the other forms of farcy-bud. Farcy, especially when situated near the head, almost invariably terminates in acute glanders, and, on the other hand, M. Rayer observes, in the article already referred to, that the majority of horses which he saw labouring under acute farcy were also suffering from chronic glanders. This disease bears a striking resemblance to the diffused inflammation produced by dissection wounds, the lymphatics being the seat of both lesions. M. Hamont considers tubercular lepra of man to be identical with the farcy of the horse, and that the former disease is confined to the poorer classes of society, and never attacks the rich and well-fed, exactly as the latter is developed in an ill-fed and low breed of horses.

With reference to the *causes* of farcy, it may be stated that whatever produces glanders is also capable of producing that disease; for it cannot be otherwise if they are identical in their nature. For example, if an animal be inoculated with the virus of glanders, we cannot tell which disease will be developed; one is just as likely as the other, and we can only explain why glanders is produced instead of farcy, and *vice versâ*, by reference to the laws of predisposition.

Farcy has for its especial seat the skin, the *cutis vera*; that of glanders being the mucous membrane; and we entirely concur in the following remarks of Mr. Percival on this point:

"In strict pathology, glanders and farcy constitute one and the same disease of the lymphatic vessels and their glands; the disease originates in these vessels, and for a time confines itself to them; in the course of its progress, however, it extends into the contiguous tissues, affecting in one case the *cutis*, in the other the mucous lining of the air-passages. No wonder, therefore, that the appearances of farcy should differ so much from those of glanders, and that the lesion of the one should be much more manageable than the other. Inflammation in the *cutis* is a different disease from that of a mucous membrane, productive of different phenomena, &c., hence the apparently wide differences between two diseases in their nature alike." (p. 318.)

Leblanc describes the farcy-bud to be the result of the coagulation of the lymphatic fluid accumulated in the vessels in an incrassated form, and obstructed by the valves, which latter, as Coleman first pointed out, are unsusceptible of the farcinous inflammation, and this will account for the peculiar plump spheroid shape of the farcy-bud, and of the pustule which

succeeds it. The skin in the neighbourhood of the bud becomes indurated and thickened, sometimes to a remarkable degree. It becomes white, tough, and hard, cutting like so much white leather rather than skin, especially in the immediate vicinity of the buds, and several of the superficial buds which have already become pustular will be found imbedded in its thickened substance. When this indurated cartilaginous-like cutis is cut through, chains of farcy-buds and pustules are exposed, invested with cellular tissue, full of infiltration, of a jelly-like citron-coloured fluid. The disease sometimes extends deep between the muscles, forming abscesses, and in inveterate cases, according to Dupuy, even the bones of the limbs become involved.

With reference to the *treatment* of glanders Mr. Percival has nothing new to offer, and frankly admits that it is still the opprobrium of his art. It is, in fact, an incurable disease, and, in this respect has for its analogue pulmonary consumption in man; either of these maladies once established may be palliated but not cured. Prophylactic measures alone are those from which we can expect any aid.

#### ART. IV.

*Untersuchungen und Erfahrungen im Gebiete der Chirurgie.* Von Dr. FRIEDRICH PAULI.—Leipzig, 1844.

*Researches and Practical Observations in Surgery.* By Dr. F. PAULI.—Leipsic, 1844. 8vo.

THIS is one of a class of works which are always welcome. It affords us the experience of an able and judicious practitioner on some of the principal diseases he has been called on to treat, as well as his opinions and criticisms on some of the contributions of others to the current medical literature. Dr. Pauli is a surgeon of eminence, practising at Landau, in the Palatinate, a fortified town containing about 6000 inhabitants; and the present volume he states to be the result of fifteen years' diligent observation at the bedsides of his patients, chiefly, if not entirely (we believe), in private practice. We mention these points as showing that Dr. Pauli possesses no particular facilities for observation beyond what are at the command of numbers of our provincial brethren in this country, from whom we would fain see works of this character more frequently emanate.

We purpose to select here and there from Dr. Pauli's small volume his observations on some of the topics on which he has treated, and shall take them pretty much in the order we find them, passing over such chapters as are least interesting.

*Hereditary brittleness of bones.* The author is acquainted with a family in which three generations have suffered from extraordinary fragility of the bones. Three members of one generation had broken an arm twice, and one thrice. Indeed one of them had five times fractured a bone in one or the other limb; and their father and grandfather had both suffered from fractures. The disposition appeared to come on in the advance from childhood to puberty. In all these persons the bones readily united;



offering a difference in this respect from the bones of persons which have become brittle from spirit drinking, as in these union is difficult. Dr. Pauli mentions a case of this sort, where the ribs were found, after death, as brittle as glass, and the other bones very brittle.

*Struma.* This article consists chiefly of a criticism on a paper, in Graefe and Walter's Journal, on Bronchocele by Dr. Heidenreich; in the course of which the author introduces an account of some cases occurring in his own practice of encysted and other tumours about the thyroid gland. In one case of bronchocele he found it necessary to tie the superior thyroid artery; the following is the narrative of the operation:

"I tied the superior thyroid artery on the 4th of July, 1833, in Eva Margaret Medard, of Ilbesheim, aged 18 years. The bronchocele had been coming on for six years, it was divisible into three portions, was rather parenchymatous than properly vascular, and was of considerable extent, but the limits were not easily defined. A pulsation was felt in the two upper portions, and most strongly in the left. The so-called purring sound was not heard, nor was there any proper aneurismal tumour. The difficulty of breathing, always present in a considerable degree, was increased by paroxysms, and was accompanied by headache, vomiting, &c. As the patient had taken iodine without benefit, she agreed, after some delay, to my proposal to tie the two superior thyroid arteries. The operation cannot be considered as one of those in which a ligature is applied with great difficulty, since even where one cannot depend on strict rules for finding the vessel, its pulsation will afford a safe guide. The integuments being stretched, I carried my incision through the skin from the middle of the submaxillary gland to the lower edge of the thyroid cartilage; then divided the platysma in the same line. A cutaneous vein, which bled freely, prevented my immediately finding the artery. As soon as I had succeeded, I proceeded to lay it bare, chiefly with my finger and a sound, and tied it with the aid of Deschamp's needle. As the operation, with the exception of the bleeding from the vein, had been easily completed, I hoped one bleeding would have been sufficient; but a difficulty of breathing, with general convulsions, which came on three hours after, rendered it necessary to repeat this in the evening. But with this the whole treatment ended; for on the following day she felt quite well. On the 15th day the ligature came away, and the wound cicatrized. The swelling disappeared by degrees, so that I found it unnecessary to tie the thyroid on the right side. Along with this diminution, which was to the extent of half the swelling, the patient lost all the difficulty of breathing she before felt."

Our worthy author seems to entertain a decided contempt for the subcutaneous method—at least carried to the extent proposed by Guerin—and lets slip no opportunity of showing this. He concludes his observations on bronchocele with the following rather *tranchant* critique:

"In the 21st number of the 'Bulletin Thérapeutique,' in which are given two cases of the cure of bronchocele by ligature, from 'Bach's Clinie,' Rigal delights us with his proposal for the cure of bronchocele by subcutaneous ligature—I say delights us, because one is always delighted to have a laugh; and I enjoyed a hearty one over Rigal's subcutaneous ligature, and I have to thank him for the merry moments he has afforded me by his proceeding. Of all the subcutaneous fooleries, not even excepting Guerin's subcutaneous herniotomy, Rigal's subcutaneous ligature for bronchocele is the *ne plus ultra*. In truth, he gives us most ingenious directions for performing this piece of sleight of hand, which would be not unworthy of the Wizard of the North, as those who feel interested may see in the journal I have mentioned. But the good man does not tell us what is to be done with his bronchocele confined there under the skin. Just fancy the sloughing of

such a tumour confined below the skin. Generally in surgery we do our best to get rid of putrid and sloughing parts as quickly as possible out of the system, whilst Rigal's proposal is to make a part slough, and to prevent its escaping so as to ensure the absorption of the putrid matter, and excite the consequent mischief as soon as possible."

We see Mr. Liston\* mentions a grave proposal having been made to him by a subcutaneous operator, which for absurdity may fairly match with the above. 'This was to cut across an abscess connected with carious bone by a subcutaneous incision, and so to distribute the diseased pus amongst the healthy cellular tissue!

*Polypus of the uterus and inversion of the uterus.* The object of this paper is to show that the cases which have been published in various periodicals (chiefly German), of late years, as instances of inverted uterus successfully extirpated by ligature, have been really cases of polypus of the uterus. We have not had an opportunity of referring to many of the cases which our author quotes, and will only therefore say that his explanation of the nature of the disease appears to us to be the correct one, and that we are much more ready to suppose an error of diagnosis than to believe that the uterus has been separated by ligature in a few days, with little or no inconvenience to the patient, as we find related in more than one instance.

As an evidence of the difficulty which is not unfrequently experienced in coming to a correct diagnosis in these cases, Dr. Pauli relates an instance in which, after repeated careful examinations, himself and five other surgeons could not agree in opinion as to the nature of the case—four supposing it to be an inversion of the uterus, whilst himself and Dr. Dom-pierre maintained it to be polypus. The woman died of internal inflammation, and the latter opinion was found to be the correct one.

*On hernia, and the operation for hernia.* Hernia is stated by Dr. Pauli to be of frequent occurrence in the neighbourhood of Landau, and in the course of fifteen years he has been called on to operate for it twenty-nine times. His 'Remarks' contain a detail of some of the most note-worthy of the cases which have come under his care, to which he has prefixed general observations on hernia; some of these we shall proceed to notice.

In speaking of the immediate cause of strangulation, after objecting to the opinion entertained by some that the internal oblique and transverse muscles may have some effect in narrowing the external ring, he goes on to say:

"The cause of the strangulation has also been sought for in the hernial sac, and doubtless the changes produced in it by chronic inflammation may produce a predisposition to this; but it can become the immediate cause of strangulation only in cases where it has been torn through (im Falle seiner zerreissung) by some powerful mechanical force—a case which seldom occurs, and which at least I have not met with in practice."

And he adds further:

"Strangulation most frequently occurs in ruptures, which, whether large or small, and whether supported by a truss or not, generally remain up, which occasionally come down, and which have been of long standing. The constriction is

\* Lectures on Surgery, *Lancet* for July 30, 1845.

usually attributed in these cases to the neck of the sac, in consequence of the peritoneum at that point having become thickened by chronic inflammation; and this view is no doubt correct. Still, it is not this alone which by its elastic force produces the strangulation; the symptoms would not follow so quickly were not the protruded bowel itself in a state of organic disturbance, which renders it more voluminous. This disturbance consists at first in a sort of cramp excited by change of weather, cold, indigestible and flatulent food, &c., in the displaced and irritable portion of intestine. If called to a patient at this period it will be well to administer one or two doses of opium, and to gently rub the swelling. In this way the muscular spasm may be removed, the colicky pains lessened, and the bowel may return."

Our author evidently attaches more importance to internal disorder, and less to mechanical causes in the production of strangulation, than most other writers on the subject. Indeed, if we understand his meaning in the first part of the above quotation, he states that he has never met with strangulation from a sudden protrusion of the bowel by over-exertion or other mechanical cause. This is remarkable; since it is generally admitted that one, at least, of the most frequent causes of strangulation is the forcing down of a portion of bowel into the sac, in addition to that usually contained in it, whether omentum or intestine.

As regards the use of opium, we can scarcely gather from what Dr. Pauli says whether he has himself employed it frequently, and with what success. Several cases have been mentioned in our weekly journals of late, in which large doses of laudanum were given with the best results; and Scarpa long ago pointed out the necessity of distinguishing what he called acute strangulation from that of a more chronic kind, of employing a soothing treatment in the former, as warm baths, &c., and abstaining from the taxis. Others, however, have employed only a temporary alleviation of the pain and vomiting by the use of opiates. These differences are no doubt in part dependent on the period at which opium is employed.

Dr. Pauli objects to the use of purgatives after the hernia has been reduced, as likely to excite irritation, and says he finds them unnecessary, as evacuation of the bowels takes place in due time without them. Here he is certainly right.

He thinks the difficulty of the operation, and the variety exhibited by the parts have been exaggerated, and that a surgeon having a proper knowledge of the natural state of the parts, and of the changes which inflammation produces in such structures, is not likely to commit any serious errors. "If," as he observes, "a man's whole knowledge of hernia is that he is to cut down until water flows out, and then to cut up, he may indeed cure some cases of strangulated inguinal hernia, but will be ill prepared for the differences he will meet with resulting from the pathological changes of structure in the parts." He then goes on to mention the performances of a professor who then occupied an important post at a university, but is since dead, the recollection of whose operations for hernia even now makes his blood run cold; as well it may, for of twenty-four operated on twenty-two died!

*Observations on amputations of the limbs.* After giving an abstract of Malgaigne's statistical researches on the operations performed in Paris, the substance of which will be found in a former Number of this Review, and

of similar researches by Norris and Hayward in America, our author gives the results of his own operations on the limbs, amounting to thirty in number. Two of this number died.

12 were of the thigh—8 for organic disease; 4 on account of injuries (2 of the latter, both men, died).

8 were of the leg—6 for organic disease; 2 for injuries.

4 were of the arm—3 for organic disease; 1 for injuries.

6 were of the fore-arm—4 for organic disease; 2 for injuries.

Of the above, 8 were females—2 under 20; 4 from 24 to 50; 2 over 50. 22 were men—7 under 20; 13 from 24 to 50; 2 over 50.

In addition to these he has operated sixteen times on the fingers, toes, feet, and hands, without losing any of his patients.

The above result must be considered as very favorable; since, according to Malgaigne, out of 584 amputations of the limbs performed in Paris, 306 died. And the results of amputations in the American hospitals, though much more favorable than in Paris, yet give a much larger proportion of deaths than 1 in 15. Of the smaller operations also in Paris about 1 in 10 died.

Dr. Pauli attributes his success not to any particular form of operation, as the circular, the flap, &c., but to paying great attention to the following points. The use of very sharp instruments; allowing a proper supply of integument; tying the vessels quickly, before the smaller ones have contracted and so escaped notice, which often leads to subsequent hemorrhage—to avoid which he also defers dressing the stump for half an hour; great care to the after-treatment of the patient; early use of remedies when inflammation is threatened—not, however, that he advises bleeding, for he only bled one of his thirty patients; narcotics, and especially opium, where there is pain, or the patient is nervous and excitable, are very necessary; local applications, either cold, or, if more agreeable to the patient's feelings, warm; and a nutritious diet.

The remaining subjects treated of are Paralysis, Squinting, Hydrocele, Intussusception, Imperforate Anus, Contagion, Hydrophobia, Laryngostenon, Rhinoplasty, (with two plates), Aneurism, Injuries of the Head, Operations for adherent Fingers, on Good Fortune in Surgery; and the whole is finished off with a collection of Aphorisms, in which the author indulges himself sometimes in a hit at Rationalistic Physicians, sometimes at their coadjutors the apothecaries; now he favours the reader with a useful practical hint, and anon branches out with poetical ardour in praise of his profession. We give the reader a touch of his quality in this line:

#### APHORISMS.

1. The deficiencies of medicine are best learned from the systems which from time to time spring up, and upset those that have preceded them.
2. Dissertations generally advance a science but little. I not long ago examined critically the literature respecting one of the most important diseases; I laboured through one hundred and forty dissertations treating of it without finding a single profitable idea.
3. Practitioners in university towns know that professors are not infallible.
4. Whence arises our want of a true collegiate spirit? From the imperfection of our science, and the want of a candid mutual confession of this imperfection.

5. A medical journal is wanted which should communicate only cases that have ended unfavorably. It would be of more service than a number of others.
6. In all medical narratives the subjectivity of the observer plays its part, and injures the objectivity.
7. Medicine, in order to its advancement, should assume its old Hippocratic state—i. e. one should learn to observe without writing a recipe.
8. It would be well worth while to collect together all that is positive in medicine, and of which not a jot is transitory. It would make but a small book.
9. Rationalism, with Hippocrates in the mouth, and the quill for writing recipes behind the ear: such is the true picture of many practitioners.
10. One hears often of favorite medicines; but can disease have to-day a partiality for one medicine, to-morrow for another? Partiality is a weakness in men. Partiality in science arises from defective knowledge and one-sided views.
11. "La méthode c'est la médecine," says Barthez truly. Those physicians should learn this who, year after year, wander from one medicine to another, but overlook the application of method to their multifarious pharmacopœia.
12. Thick darkness besets the subject of etiology. Not unfrequently causes are found for diseases which, after a time, are found to be completely false. How often are colds and wettings cited as causes of disease! In the course of the musical festival which took place in the Palatinate, under the direction of Hofzollmeister Frank Lachna, a party of at least 20,000 was assembled near the ruins of Magdeburgh castle. The whole of them, with scarce an exception, were wet through without any disease ensuing, as many feared there would.
13. Dr. C. Walther praises roses in a particular treatise, published at Stutgard in 1837, entitled the 'Healing Virtue of Roses in Persons threatened with Atrophy and Consumption.' The sick are daily to inhale air impregnated with the fragrance of roses, to drink rose tea, rose water, and rose wine; to eat preserved roses, honey of roses, and rose cakes; to rub in rose salve and rose oil: and all this will not save them from the churchyard roses—the so-much dreaded roses without thorns.
14. In stomatitis, peppermint drops allowed to melt in the mouth produce a feeling of coolness, and are a good palliative.
15. Cold foot-baths at bedtime are a valuable remedy in that sleeplessness caused by loss of blood.
16. In ganglions, acupuncture is an easily managed, little painful, and generally successful mode of treatment, and, according to Barthélemy, (*Gaz. Médicale*, 1839,) to be preferred to the subcutaneous incision.
17. Ergot has such a specific action on the uterus that it should be more variously employed. In painful menstruation, accompanied by spasms, it acts like a charm, as also in the eclampsia of childbed.
18. Specific medicines are generally too little employed. Thus tincture of cantharides, judiciously employed in retention of urine from cold, and where no organic diseases exist, will often obviate the necessity of catheterism.
19. Brandy-drinkers can generally bear very hot baths; the peripheral nervous system being dulled in them.
20. I have twice seen those horn-like prolongations of the toe nails which sometimes occur in old women, accompanied with misshapen toes; that is to say, there was an exostosis surrounding the nails like a wall.
21. Persons, especially men, who eat and drink freely, are threatened with apoplexy when, though their general *embonpoint* remains, their arms and legs begin to waste.
22. The therapeutic skill of many physicians consists in prescribing *oleum jecoris aselli*. Would it not be well henceforward to name it *oleum jecoris asini*?
23. I would lay down the following as an axiom which may be easily proved:  
"In every local inflammation, unaccompanied with fever, general bleeding, if

not absolutely hurtful, will be at least useless. Half the cases of ophthalmia are examples of this.

24. Not unfrequently medical men repent of their choice of a profession; this arises from their being 'unable to find a compensation in the science for the daily toil in the practice of their art. A physician, to be happy, must love science with the admiration of a lover. She must fill his whole soul; she must be incorporated with his whole being; and then she will daily appear to him new and interesting. Medicine is exciting, precisely because no man can ever arrive at a complete knowledge of her; and in this she differs from sciences which are complete in themselves. Whilst in them at best one yawns over quiet undisturbed possession, in medicine there is daily new life, new excitement. I shudder when I fancy myself a jurist or a theologian. Heinrich Heine says somewhere the Frenchman loves liberty as his bride, the Englishman as his wife, and the German as his grandmother. I say that jurists, mathematicians, theologians, &c., can at most love their science as a wife; to the physician alone is it granted to love his as a bride with all the ardour of first love.

#### ART. V.

1. *An Essay on the Use of Narcotics and other remedial agents, calculated to produce Sleep in the Treatment of Insanity.* By JOSEPH WILLIAMS, M.D.—London, 1845. 8vo, pp. 120.
2. *Practical Notes on Insanity.* By JOHN BURDETT STEWARD, M.D.—London, 1845. 8vo, pp. 120.

I. To the essay of Dr. Williams was awarded a premium placed at the disposal of the King and Queen's College of Physicians, by the Lord Chancellor of Ireland, for an essay on some subjects connected with the treatment of Insanity.

The author commences his essay by alluding to the very varied causes producing the insomnolence which so frequently precedes an attack of insanity, and forms, in general, so important a feature of the disease throughout its progress; and the consequent necessity, in a work treating on the means of procuring sleep in insanity, of discussing remedies which cannot be regarded as narcotics. Accordingly, we have in his essay articles on bleeding, general and local, purgatives and emetics, narcotics, stimulants and tonics, warm baths, exercise, &c.

After a brief allusion to the several forms of the disease and its varieties, as adopted by systematic writers, together with an outline of the causes most frequently conducing to its development, he refers to the importance, at the commencement of an attack of insanity, of accurately discriminating its cause, and the state of system under which it appears, as upon the treatment pursued at this period most frequently depends the result of the case, and whether it shall rapidly subside or terminate in a chronic form of the disease.

"There is always extreme irritability in incipient insanity. Generally the brain suffers first, then some other organ. The great object, however, is always in the first to allay irritation, and to endeavour to ascertain whether the brain was primarily affected, or whether insanity followed some visceral affection. It is of the greatest importance to determine whether insanity is symptomatic or idiopathic; whether the result of mere error of perception, [?] or whether the



medium through which we reason is at fault. Mind is independent of matter [!] and it may by some sudden shock become incapable of perceiving, discriminating, or judging correctly; and it is in such cases when tranquillity has been restored by narcotics, that the metaphysical treatment has been successful. If the excitement consequent on reaction in these cases be not speedily lulled, the brain itself often becomes congested or inflamed; and this continuing, symptoms increase, and those alterations in the brain and membranes so frequently observed more or less speedily occur." (p. 28.)

*Bleeding.* Dr. Williams agrees with most writers on this subject, in recommending caution in the use of general bleeding, which he regards as applicable only to the cases of the disease appearing in strong, robust persons, living in country situations, and in whom the attack has followed the suppression of accustomed evacuations, as epistaxis in men, or the suppression or cessation of the catamenia in females. In these cases, also, it is rarely, if ever, applicable, except in the early stage of the disease, and must be employed sparingly. In ordinary cases he prefers local to general depletion; the abstraction of blood by the cupping-glass, or by leeches, being less likely to produce undue depression afterwards, or to be followed by reaction. He alludes to what we have frequently observed, the soothing effect produced on maniacal patients by the gradual abstraction of blood by leeches, and regards this mode of treatment as not having received sufficient attention. In all cases when antiphlogistic measures are about to be had recourse to, it becomes of the greatest importance to discriminate between the symptoms resulting from active inflammatory action and those originating in irritation; and in many cases where general or local depletion is required, it will be necessary carefully to support the strength of the patient, or even to exhibit stimulants at the same time.

"Many cases of insanity arise from extreme irritability dependent on prostrated powers; and to support the person by good nutritious food, and sometimes even with brandy and wine, at the same time soothing the system by procuring refreshing sleep at night by morphia, will speedily evidence the advantages of such treatment. The great error originally was allowing the powers to sink; it is of the greatest importance that those powers should be supported; the nervous excitation must be calmed. In these cases mistakes are but too frequently made; irritation is confounded with inflammation. The maxims so ably taught by Mr. Travers are forgotten; the object being to calm the action, not to diminish from the power—this nervous power being much more easily depressed than raised. Should this advice be neglected, and bleeding be ordered, stupor, or coma, or confirmed mania may be the consequence. In many cases, when there is the most ferocious delirium with great muscular power, yet the pulse is very quick, weak, and fluttering, and even the slightest depletion at once knocks down the powers; but even if the patient should again rally, there is great danger of his becoming idiotic." (p. 32.)

Purgatives, antimonials, and emetics are applicable in conjunction with the general antiphlogistic treatment, but more especially may be employed in those cases of inflammatory excitement when depletion has already been carried as far as may be regarded as safe, or when the more active antiphlogistic measures are inadmissible.

"Many cases of vigilantia, dependent on monomania, or even furious mania, will yield to ant. potass. tart., and often, on the vomiting ceasing, refreshing sleep will follow. It has been remarked by Dr. Cox that one third the usual dose of tartar emetic will prove efficient if a narcotic has been given the night before; generally,



however, full doses are required. . . . . The continued action of tartar emetic cannot be too much lauded in some incipient cases of mania ; while under the influence of antimony, the patient seems rational ; it is withdrawn, reaction occurs ; the eyes roll or remain fixed, noise succeeds tranquillity, the head again becomes hot. As evening sets in symptoms increase, and the patient, with unclosed eyes, passes a restless and boisterous night ; whereas, had the action of antimony been kept up, placidity, if not actual sleep, would have been substituted for extreme restlessness and violence." (p. 45.)

**Opium.** "Opium is contraindicated when there is great heat of skin with extreme restlessness, and determination of blood to the head ; and all authorities seem agreed that it should never be administered when the system is plethoric, unless depletion or purgation, or both, have preceded it." (p. 47.)

"Delicate and debilitated constitutions, with spasmodic irritability, generally bear opium well, and this perhaps accounts for its less frequently disagreeing with females than with males. When the nervous system is the most highly developed opium is often the most useful, and is especially indicated in those cases of vigilantia and restless cases resulting from nervousness. In puerperal mania, when it has been necessary to deplete or purge, large doses of opium are doubly necessary ; and should sleep follow, the attack will generally be alleviated or suspended. Opium is essentially indicated when the system is depressed, when it often acts as a charm ; and by its stimulating properties is far more useful than Battley's sedative, or the preparations of morphia." (p. 52.)

Opium is generally very useful in suicidal cases, or those resulting from continued intoxication, and after loss of blood ; or when the person is much exsanguined and depressed, and has a dry, cold skin. Generally it will be well to give it in a full dose (from two to five grains). Dr. Burrows begins with three grains, and repeats one grain every two or three hours, never allowing the quantity given to exceed twelve grains. In cases where there is febrile excitement, it may be combined with calomel, antimony, or James's powder. It may also be given in combination with camphor, hyoscyamus, or digitalis, or in the form of pulv. cretæ co. c. opio, and may be applied by enema or injections. If the purely sedative effect be desired, the preparations of morphia, or the sedative liquor, may be substituted for the opium.

**Digitalis.** "When there is considerable arterial action with vigilantia, after being well purged, digitalis will very often reduce the power and frequency of the pulse, and sleep follows. In mania, with or without henbane or camphor, digitalis will be found a most useful narcotic ; but if the excitement be excessive, and this arise from acute inflammation, tartarized antimony must precede it." (p. 67.)

The tincture is the most eligible form for exhibition, and may be given in doses of 10m. every six hours ; if the pulse does not become less frequent in five or six days, it must be discontinued.

**Hyoscyamus.** "Hyoscyamus is especially useful in nervous habits, and is particularly indicated in monomania, and even the temporary quiet from it in mania is often of the greatest benefit. When there is excessive nervous irritability, it has often a remarkably calming and soothing effect ; it may also be given when there is vascular excitement, when opium is strongly contraindicated ; it does not excite the brain in these cases, and is often found to reduce arterial action. When patients awake after the sleep caused by henbane there is not that confusion of thought, that stupified expression, nor heat of skin, and dryness of the fauces and tongue, so often seen when other narcotics have been taken. . . . . When a sedative has to be continued from day to day, or several times during the day, hyoscyamus will often be the very best we can select ; as, in addition to its tranquillizing effects, it

will not check if it does not actually cause diaphoresis, while it promotes the flow of the urine, and also relaxes the bowels. It acts almost as a specific in some cases of monomania, causing tranquil sleep, and a quiet, placid waking." (p. 69.)

Hyoscyamus may be given in combination with opium or camphor, and other remedies, according to the circumstances. Of the extract five to ten grains may be given as a dose, and five grains may be repeated every four hours. Some prefer to give it in large doses at once, and in this way ten to fifteen or thirty grains may be given. It should never be exhibited as an enema, as fatal results have attended this practice.

*Warm baths.* "There are many cases of insanity where there is want of sleep, in which it would be perfectly useless to prescribe narcotics, but very good effects invariably follow the judicious employment of warm tepid baths. Much discrimination is necessary to determine as to the heat best adapted to the particular case, and the greatest care must be taken not to raise the temperature too high; it should never exceed 98° F., or it will act as an excitant, and may even induce apoplexy; 96° F. may be usually considered as the best temperature of a warm bath for the insane. Persons of a nervous temperament bear a higher temperature than the bilious; and the warm bath is more decidedly useful in cases of melancholia than in other forms of insanity; but it will be generally found a very powerful means of diminishing cerebral congestion, and allaying irritation in most maniacal cases. . . . It is often asked how long should a patient remain in the bath, and how frequently should it be repeated. It may be necessary to order a bath daily, or even twice a day, and the patient may be immersed half an hour, one hour, and even two hours, the time depending on the effect produced. . . . The warm bath will be found very effectual when the circulation is sluggish, skin and feet cold, when often half an hour's immersion will ensure a good night's rest. The first effect produced is languor; as soon as this is perceived the patient must be removed, or actual syncope may occur." (p. 91.)

With these extracts we take our leave of Dr. Williams's essay. In a treatise of this kind we must not expect any very novel or original views. Dr. Williams has, however, bestowed great attention on his subject, and presents us, in a small compass, with a large amount of practical and judicious observations on the various remedies employed in the treatment of insanity. His metaphysics are of the old school, and his pathology is not of the newest; still we commend his little volume to the members of the profession, who, whether engaged in general practice, or in the more exclusive treatment of mental affections, will find it a useful compilation.

II. The notes of Dr. Steward have reference to the treatment of insanity generally, though the largest share of attention is devoted to the discussion of the moral management of the insane.

In the preface, and repeatedly throughout the course of his work, Dr. Steward alludes to the, as we think, now universally admitted necessity for confiding the care of the insane only to qualified medical men; and his remarks on the demand for a change in this particular appear more applicable to what was the state of matters five or ten years ago than to the practice which prevails at the present time. So far as our observation serves us there are now very few institutions, whether public or private, for the treatment of insanity, in which the superintendence is not vested in resident medical men. We are, indeed, far from thinking that much does not remain to be accomplished in the improvement of the system of treating these cases; but we believe that the change has already com-

menced, and is rapidly advancing. In proof of this we may refer to the attention which is at present paid in the selection of medical superintendents to our public institutions, to secure those who are not only regarded as possessed of a full share of medical knowledge, but also of an adequate experience in the management of establishments for the special treatment of insanity. We may also instance the facilities afforded by the managers of some of our public asylums for the acquirement, by the rising members of the profession, of a practical acquaintance with the treatment of insanity (as in the Lectures delivered by Dr. Connolly at Hanwell, and Dr. Sutherland at St. Luke's, and in the resident studentships at the Gloucester, Morningside, and Glasgow asylums) as affording proof that the enlightened portion of the public is already fully alive to the necessity of a practical study of those cases to medical men, whether engaged in general practice or specially devoting themselves to the treatment of insanity.

The intention of Dr. Steward's work being entirely practical, he abandons the ordinary systematic arrangement of the subject, and adopts the divisions into idiopathic, symptomatic, and organic insanity, as better calculated to render clearly intelligible the comparative curability of the different classes of cases.

"Pure *idiopathic insanity* is that form of the disease in which the vital and animal functions are healthily performed, the patient at the same time labouring under some particular delusion, inconsistency of conduct, or general incoherence, accompanied by altered habits and manner. Under this head will, of course, rank every case of insanity in which there is no bodily disorder or disease, whatever be the state of mind—from the first shade of disturbed intellect to total annihilation—from insanity in its mildest form to idiotcy.

"*Symptomatic insanity* I consider that form of the disease comprehending all cases, accompanied by disarrangement of the general health, evinced by a morbid state of the secretions and excretions; or, as regards any particular organ, as the uterus, by interruption or impediment of function; or when insanity is attended by an unusual state of the circulation, the heart's action being above or below the natural standard.

"By the term *organic* I mean to designate that form of insanity accompanied by disease of the brain or some other internal organ.

"This division of insanity, adapted as it is for practical purposes, leads to another, with a view to prognosis, equally important, viz. curable and incurable. First, it is to be recollected, as a fact confessedly admitted, that the curability of insanity is in an inverse ratio to the time of its duration. Upon proper and early treatment, therefore, I submit that the following forms of insanity, *there being no organic disease*, are curable, viz.:

"Cases accompanied by quickened circulation through, or congestion in, the vessels of the brain or its membranes.

"Cases accompanied by derangement of the general health, or impairment of functions.

"Cases apparently originating in excess of any kind.

"Cases accompanied by simple œdema, with general constitutional derangement.

"Cases following the sudden cessation of rheumatic pains.

"Cases following the suppression of any accustomed evacuation or secretion.

"Cases following the sudden recession of cutaneous affections.—Also probably curable, if seen within the first three months after the commencement of the attack, are—

"Cases where the delusion is limited to one subject, and the mind remains sound as regards all others, the health being undisordered."

The incurable cases are, of course, the idiopathic and the various forms of organic insanity. Predisposition must also be taken into account in the prognosis, as when this exists "few cases continue well more than two or three years at most."

Passing over the sections on the symptomatology and etiology of insanity, we shall quote from the portion of Dr. Steward's work devoted to the treatment, medical and moral. The following is Dr. Steward's opinion on the use of—

*Sedatives.* "Except in cases of insanity, occurring in persons who have been in the habit of drinking freely, and whose constitutions are shattered, and the whole frame tremulous, sedatives almost invariably do harm; so far from subduing excitement they increase it. In fact, sedatives with the insane act generally, if not invariably, as stimulants. They exercise little or no influence over the insomnia of mania, which seems, as it were, a part of the disease, which resists all remedies, and which yields only when nature, fairly tired out by long exertion, sinks exhausted, or when sleep comes, the harbinger of returning health. In what doses opium, conium, or hyoscyamus, &c., might each produce its sedative effect on the delirium of mania I know not; neither should I dare to press the medicine so far, lest its sedative effects might be fatal." (p. 59.)

This passage we cannot but regard as containing too general a condemnation of the use of sedatives in the treatment of insanity; and certainly the opinion of Dr. Steward does not accord with our own experience any more than we believe that it will with that of most other practitioners. As justly observed by Dr. Williams, the use of sedatives, like the employment of all other active remedies in these cases, requires caution. When given in cases of delirium connected with acute inflammation of the brain they are calculated to produce the evil effects ascribed to them by Dr. Steward; but administered after this inflammatory action has been subdued by antiphlogistic means, or in the cases of insanity where irritability rather than excited action exists, their use is generally highly beneficial.

*Moral treatment.* "Moral treatment may be defined the employment of means best fitted to restore the sufferer to a healthy habit of thought and action. Every experienced person knows that moral management is effective or otherwise according as its basis—classification—is well or ill chosen. The chief objects of classification ought to be security and comfort; I, therefore, found the classification I am about to suggest, not upon the different forms of insanity, but upon the self-evident principle of protection to the weak, quiet to the tranquil, comfort and security to all. With this view I divide the insane into two classes, viz. *quiet* and *noisy*; and I adopt this division, first, because it includes, I think, every possible case; and, secondly, because it is of equal importance in a curative point of view, as it is essential as regards good moral management. The arrangement, to be perfect, ought to be such that the noisy cannot be heard even by each other, for a single noisy patient will generally infect the rest; and not only those generally noisy, but even those occasionally—and at that time quiet—their rest being disturbed, will become noisy. The division into quiet and noisy leads, of course, to subdivisions, viz. first into cleanly and dirty; and those again into patients *not* dangerous either to themselves or others; and patients dangerous to themselves or to others; and then again into mischievous and destructive. . . . For the mischievous and destructive separate galleries ought to be provided. They are,

indeed, common disturbers; they incense the violent, and absolutely persecute the inoffensive; for their destructive and mischievous habits, though all in good humour, are not confined to themselves or to the property around them, but they will often destroy the clothes, &c., of their companions; besides which they are, almost without exception, disposed to appropriate to themselves whatever they can lay their hands upon. This of course leads to quarrels, if nothing worse, and ought therefore to be prevented. With respect to epileptics, some are subject to furious mania before and after a fit, are easily provoked at all times, and never quite rational. Others are only dangerous when a fit is approaching, being during the interval of convalescence in possession of their reason." [They require to be classed according to the circumstances of the case.] (p. 75.)

*Locality—Rooms and fittings—Restraint.* "No system of moral management can be good which is not founded on classification; and no classification can be perfect except when the means of accommodating all classes of patients are, *strictly speaking*, suitable. It is almost needless to suggest that the situation should be airy and cheerful, and the rooms, both day and sleeping, of ordinary size and well ventilated. This is easily accomplished as regards the orderly and convalescing, but those who most demand our sympathy and attention, who constitute our difficulties in management, who so often have been the objects of harshness and mistreatment—are the violent, the mischievous, the helpless, and the dirty. For such cases common rooms and bedding are wholly unfitted; and unless rooms, adapted to each particular state be in readiness, neither can restraint be avoided, nor the proper means of cure or management supplied. The sleeping-rooms for these patients ought to be of convenient size and properly secure; the floor should be under-warmed, and so constructed as to admit of being readily and effectually cleansed; the bed should be such as can be removed, and another supplied in a few minutes. Of these rooms there ought to be one in every house, even though there be but a single patient. If the number exceed five, there ought to be two; and for every additional five one more; otherwise a paroxysm occurring, unjustifiable restraint cannot be dispensed with, and as restraint, *with properly warmed and properly fitted apartments*, in almost ninety-nine cases out of a hundred, is unnecessary, such arrangements ought to be enforced; for as all know, or ought to know, whatever be the present tranquillity of a patient, a change, frequently sudden, is always possible, and in most cases probable.

"I am most anxious to impress upon the minds of my readers the necessity that the floors, both of sleeping and seclusion rooms for bad patients, should be under-warmed; for as nothing but restraint will keep a patient in bed, when suffering under high excitement, it stands to reason that without under warmth he must, if at liberty, be exposed to the danger and misery of walking about, or lying, all night upon a cold, damp, and very often wet floor—boards being out of the question in these cases—should he, as constantly happens, destroy his bed or refuse to occupy it. It may, therefore, be accepted as a self-evident fact, capable of perfect demonstration, that unless the floors of at least the sleeping-rooms for bad patients be warm enough to lie and stand upon with impunity, the non-restraint system cannot be carried out with benefit or even safety to the patient. The ventilation, too, in these rooms requires to be freer than in a common room, and should be therefore of the most perfect kind.

"In a curative point of view, the construction of the rooms and beds for what are called violent patients—which are, more frequently than otherwise, curable ones—is of the utmost importance; and I firmly believe that the want of such arrangements no less conduces to the increase of incurables, than it most certainly inflicts upon the poor patients a greater and unnecessary degree of suffering.

"In commonly fitted rooms, restraint in case of violence is inevitable, and, once employed, there is no limit to its use; for, not only must it be sufficient to keep the patient in bed, but it must be so increased as to prevent him from breaking or dislocating his limbs in his efforts to free himself, and thus he remains the



whole night, and often longer, the attendant being afraid to remove the restraint, lest evil to himself or the patient should follow. This continued night after night—the patient generally keeping one position—the sustaining parts soon give way, and then follow those dreadful ulcerations, so frequently terminating in the sacrifice of life.” (pp. 84 et seq.)

We have now abstracted some of the more interesting and important portion of Dr. Steward's notes. His remarks, though—for the extent which his work embraces—necessarily very brief on many subjects, uniformly exhibit him to be possessed of much experience and judgment in the management of the insane.

Dr. Steward concludes his work by alluding to the necessity of some change in the law of lunacy as it at present exists, by which the anomaly shall be removed, that, while the liberty of the individual may be restrained, there exists no means of legally managing his estate, except by issuing a commission of lunacy—a step which, from the expense attending it, and the objection on the part of the friends to the publicity it entails, is usually deferred till the lunatic becomes incurable, and when after his property has sustained great injury. As the remedy of this evil, Dr. Steward suggests the delegation of a power to the commissions to enter upon the private investigation of cases reported by the friends of the patients, and to appoint provisional trustees, whose office should expire when the patient shall be deemed sufficiently recovered to reenter upon the charge of his affairs, or at a time to be fixed beforehand, or when, being deemed incurable, no objection is felt to the ordinary mode of proceeding. We think the suggestion worthy of consideration.

#### ART. VI.

*Traité des Maladies des Articulations ; accompagné d'un Atlas.* Par A. BONNET, Professeur de Clinique chirurgicale à l' Ecole de Médecine de Lyon, &c. Paris, 1845.

*A Treatise on Diseases of the Joints.* By A. BONNET, Professor of Clinical Surgery in the Medical School of Lyons, &c.—Paris, 1845. 2 vols. 8vo, pp. 1229.

In our January Number (Brit. and For. Med. Rev., vol. xxi, p. 124) we gave an outline of MM. Bonnet and Richet's views respecting the general pathology of diseases of the joints, and we shall now proceed to examine M. Bonnet's doctrines as to the general etiology and general treatment of those affections.

The causes of diseases of the joints may be internal or external, and the most important of the former, on which M. Bonnet especially dwells, are those general or constitutional dispositions termed diatheses. The existence and character of a diathesis can only be known by its effects—its intimate nature is entirely unknown; but if we see fungosities, pus, or tubercles deposited (especially if they are so in several organs simultaneously), without any applicable external cause, we infer that there is a disposition to such deposition, and that disposition is a diathesis. M. Bonnet, as we have seen, divides the morbid products in diseases of the joints into



three classes : those which become perfectly organized—those whose organization is arrested—and those incapable of organization ; and he characterizes the diatheses that most commonly produce diseases of the joints according to their tendency to excite the deposition of one or other of those species of morbid products. The first is the *rheumatic* diathesis, the most favorable of all, in which there is a tendency to deposit plastic lymph ; the second he terms the *fungous* diathesis, as it is characterized by the formation of “*fungosities* ;” the *purulent*, the *tubercular*, and the *gouty* diathesis belong to the third class, being accompanied by the deposition of tubercle, pus, and uric acid, all incapable of organization. Often, however, there is no marked line of distinction between some of those diatheses. However distinctly marked in some cases, in others they may be mixed ; *fungosities*, for example, are frequently infiltrated with pus, or may coexist with chronic abscess in some other situation. Indeed this liability to fusion has caused, M. Bonnet maintains, several really distinct constitutional conditions to be confounded under the common appellation of scrofula. Thus he considers the tubercular diathesis as different from scrofula ; and this distinction made, he urges that among the other patients commonly termed scrofulous, individuals presenting very different general characters may be found. Some are pale, emaciated, with sallow cheeks, thin eyelids, lips, and septum nasi, and exhibit no trace of enlargement of the glands ; these are the characters of the *purulent diathesis*, and they prevail in persons affected with chronic abscess. In others again the face is full, the eyelids, lips, and alæ nasi tumid, the glands of the neck usually large, and the complexion is commonly more or less florid, at least until the health has been undermined by the progress of disease. Such patients are usually liable to congestions with mucous secretions, their eyes are frequently red, the eyelashes adhering from a tenacious mucous secretion, slight causes excite obstinate catarrhs, and in children a crusted exudation often occupies the hairy scalp. Such are the signs of the scrofulous diathesis properly so called, which M. Bonnet prefers terming the “*fungous*” diathesis, as it is with this diathesis that “*fungosities*” most frequently coexist. (Vol. ii., pp. 25 et seq.) Still, as has been already said, those diatheses shade gradually into each other and often coexist ; and this circumstance shows a close relation between them, which, M. Bonnet argues, is confirmed by other considerations. Thus their causes are all more or less common ; they are all hereditary, and such is their connexion that parents affected with one diathesis may transmit either of the other to their children ; their most frequent occasional causes are also the same, viz. chills, damps, suppression of the cutaneous secretion, eruptive fevers, &c. Their general symptoms also agree in this, that the functions of calorification, of the digestive organs, and of the skin, are deranged in each ; they also frequently demand the same treatment, e. g. stimulating the skin, sulphureous and saline spas, iodine, alkalies, tonics, &c. Such points of agreement indicate that however widely different in other respects, something essential is common to all. What is it, then, that so greatly modifies this common term ? To this M. Bonnet replies :

“ I am inclined to believe that the same morbid principles introduced into the economy produce sometimes one, sometimes another, of these affections, solely because of the different predispositions which are peculiar to each individual and

to each age; children are predisposed to the secretion of pus and fungosities, they become scrofulous; youths and adults have a tendency to inflammations more rapid in their progress, they are predisposed to acute rheumatism; in middle and advanced life there is a tendency to maladies slow in their progress, and chronic rheumatism is of frequent occurrence. In my mind, scrofula and rheumatism have this in common, that they are caused by the same morbid principles, and their differences solely depend on the way in which each age and each constitution reacts under the influence of this cause." (Vol. i., p. 104.)

M. Bonnet founds his diagnosis, and consequently his prognosis, of *chronic* diseases of the joints mainly on the external characters indicating the existing diathesis. Thus, when fluctuation cannot be detected in a diseased joint, it is often impossible to determine by manual examination whether the fibrous or fungous tissue deposited in the joint is or is not infiltrated with pus; no doubt the more indurated the tumour the greater is the deposit of fibrous tissue and the more favorable the prognosis, while, on the contrary, the more yielding it is, the more do "*fungosities*" predominate; but in doubtful cases a tolerably accurate conclusion can only be arrived at by considering the general condition of the patient. Thus, if a chronic abscess coexists with a diseased joint of a doubtful character, we may certainly conclude that there is a tendency to the formation of a chronic abscess in the joint; while if fungosities exist elsewhere they are present in the joint also. But, above all, the general condition of the patient must be scrupulously examined—the countenance, complexion, &c., the capability of resisting fatigue and vicissitudes of temperature. If the signs of the purulent diathesis predominate, though "*fungosities*" only are deposited, they will probably become infiltrated with pus, while, if the scrofulous diathesis is well marked, fungosities, however soft, may escape suppuration, and become converted into fibrous tissue; and, finally, when the general signs of a good constitution are present a favorable issue may be anticipated, as organized matter has most probably been secreted. The same considerations are our only guide when there is no appreciable lesion of a joint in which the patient experiences some pain and some restraint of motion; in such a case it is only possible to say what will be the tendency of the disease should it become aggravated. If the chronic purulent diathesis exists those pains forebode a chronic abscess. If the scrofulous diathesis prevails fungosities are imminent. We have set forth M. Bonnet's views in this matter pretty fully, inasmuch as the following brief quotation shows the cardinal importance he attaches to them:

"If this work differs from those already published on diagnosis, it does so particularly in the care taken to estimate concomitant lesions and the general state of the constitution." (Vol. i., p. 113.)

The two great external causes of diseases of the joints are cold, and especially damp. Their influence indeed is generally admitted; but M. Bonnet complains that the special conditions which render them innocent or injurious have not been determined, and this point he investigates at great length. As regards cold, M. Bonnet's doctrine is just this—1st, that the chill produced by cold air is much more injurious than that caused by cold water; and, 2d, that though the action of cold, especially cold air, is very dangerous when perspiration has been excited by bodily exertion, it is almost innocuous, and if the refrigerating medium is cold water completely

innocuous, when passive perspiration exists, such, for example, as is produced by enveloping the body in flannel or other bad conductor of heat. This opinion M. Bonnet chiefly founds on the results of the hydropathic treatment, which he asserts has never been followed by inflammatory mischief in any case, including some hundreds in his own practice. Damp, however, is the most frequent agent that leads to diseases of the joints, and its influence is the more mischievous, as it acts by deteriorating the constitution. The vapour of pure water, or simple atmospheric moisture, is indeed, M. Bonnet thinks, quite harmless, but the moisture exhaled from walls recently built, frequently wet, or otherwise rendered damp, or derived from any similar source, is eminently injurious; producing in early life the fungous or purulent diathesis, or both, and generating rheumatism in adults. It is true that many under such circumstances apparently escape with impunity, but M. Bonnet maintains that their health is nevertheless almost uniformly really impaired. Thus the parents of a scrofulous child will often deny that the dampness of their dwelling has caused the disease, alleging that their own health is unimpaired though they inhabit the same house; but M. Bonnet has almost invariably found that their health is impaired, they are liable to catarrh, sore throat, ophthalmia, wandering pains, &c., on exposure to those transitions of temperature which are inseparable from the ordinary occupations of life. In support of these statements M. Bonnet enters into several details respecting the construction of the houses, and the habits of the manufacturing population in Lyons and the adjacent districts, to which we can only thus generally refer. (Vol. i., pp. 92 et seq.)

In the chapter on *general therapeutics*, M. Bonnet first discusses the local and then the general treatment; but we shall consider the latter subject first, as the chief indeed the only point therein requiring notice is directly connected with M. Bonnet's etiological views.

In constitutional diseases of the joints, according to M. Bonnet, the functions that are most frequently deranged are those of the skin. In many scrofulous and rheumatic patients those functions appear to be natural; but they are really disordered unless the perspiration is acid—possesses its peculiar smell—contains its normal salts—and unless the individual, when suitably clothed, sustains no sensation of cold in ordinary alterations of temperature. But all those conditions seldom exist in constitutional diseases of the joints, and, where they do not, our primary object should be to restore them; in effecting which, according to M. Bonnet, hydropathic baths are the remedy to which there is *nil simile nil secundum*.

We of course shall not describe the mode of administering those baths, as any of our readers who may not be acquainted with the process will find a full account of it in a former Number of this Journal. (No. XXVIII., p. 429.) But we shall briefly state the circumstances which M. Bonnet says specially indicate and contraindicate the practice, with the precautions he recommends in adopting it.

The hydropathic baths, then, are always indicated—when the perspiration is suppressed on the one hand, or is passive and copious on the other, and also when it has lost its odour—when the cutaneous circulation is languid—when the extremities are habitually cold, or when a sensation of coldness is experienced in or near the affected joint—when damp has

been the exciting cause of the disease—when the pain, stiffness, &c., of the joint increase in winter and autumn, and diminish in summer, or generally are influenced by changes of weather—and, finally, when the patient is subject to wandering pains or transient shiverings. (Vol. i., pp. 167 and 553.) On the other hand, the treatment is contraindicated when the thorax, to use M. Bonnet's phrase, is *impressionable*, when there is habitual cough, and *à fortiori* when pulmonary tubercles exist; there is little hope of success when the patient is pale and much enfeebled, the favorable effects of the baths being the more decided the stronger and the greater the power of secretion of the individual. At first, if the patient is feeble the bath should be at the temperature of 18 or 20° C. (64 to 70° F.) and gradually lowered to 15°, 12°, and 10° C. (59, 53, 50° F.), and ultimately, when opportunity offers in winter, gradually lowered to 0° C. (32° F.) The time of immersion in the bath should be similarly graduated from two minutes to five or even ten minutes, which latter period should seldom be exceeded; but, irrespective of time, whenever the shock is severe, the patient shivering and his teeth chattering, he should be at once removed from the bath, as otherwise the action may be imperfect, or even entirely fail. M. Bonnet confirms the statements of others, that the perspiration during the first stage of the process, has very frequently an odour *sui generis*, and in scrofula especially, is often unsupportably fetid. Under the influence of the treatment the skin becomes soft and perspirable, the fetid perspiration, the coldness of the extremities, the sensitiveness to alternations of temperature disappear, and the skin becomes more vascular, and assumes a healthy colour. From all this M. Bonnet thinks that there is no hypothesis involved in the conclusion that the hydropathic treatment acts by eliminating morbid principles, re-establishing natural perspiration, and invigorating the function of calorification. He is also quite satisfied that the injurious consequences popularly attributed to chills never follow the use of hydropathic baths, and accounts for this by the distinction he seeks to establish in his chapter on etiology between perspiration excited by exercise and that caused by warm clothing, and also between the chill produced by cold air and by cold water. We have already stated the general indications specified by M. Bonnet for the employment of hydropathic baths:—now for a few words respecting their applicability in particular diseases. M. Bonnet disapproves of them in acute inflammation, whether rheumatic or not; but he was induced to try the wet sheet in a few such cases, and without a favorable result. When, however, the malady is protracted, say beyond two months, and tends to become chronic, the fever having subsided, but swelling, pain, stiffness, &c., remaining, then he has found the hydropathic treatment most efficacious. (Vol. i., pp. 369-73.) In the chapters on chronic arthritis and chronic rheumatism several cases thus treated are detailed. One is a case of chronic rheumatism of ten years' duration; there was hyarthrosis, with thickening of the soft parts of both knees and one wrist, displacement of the lower extremity of the ulna, nodosity of the joints of several fingers, and a similar condition of the joints of the tarsus. The patient suffered great pain, especially at night, and was unable to walk, the state of the hands preventing the use of crutches. The disease, moreover, had been contracted in the rainy season in the West Indies. After the administration

of seventeen hydropathic baths it is reported that "the hydarthrosis of both knees, of the feet, and of the wrist is completely dissipated, the swelling of the fingers is scarcely perceptible; the only remaining anatomical alteration is the displacement and mobility of the ulna, with slight doughiness of the adjacent soft parts. For the last fifteen days the pain and sense of coldness have entirely disappeared, the motion of the knees, feet, and wrist, are restored, and any object can be firmly grasped with either hand." (Vol. i., pp. 497 et seq.) We have abstracted this case, as M. Bonnet says "the results are undoubtedly the most remarkable I have obtained from the use of cold baths in chronic rheumatism." (p. 531.) Notwithstanding this favorable result, however, M. Bonnet elsewhere says that in chronic arthritis and chronic rheumatism there is no legitimate hope of success unless the disease is recent, and the anatomical alterations slight (p. 416); and, again, that a complete cure is impossible if fibrous tissue is deposited within or external to the joint; that when serum is effused a cure is possible, but much more difficult than when pain and stiffness only are present. (p. 520.) In acute gout, M. Bonnet, though he would not risk the bath, applied the "wet sheet" in one case with the most satisfactory result. (p. 562.) He has had no opportunity of treating chronic gout, but has no doubt that the hydropathic treatment in its full vigour would be eminently beneficial in it. (p. 569.) Hydropathic baths are strongly recommended in hydarthrosis, but no cases in which the treatment was employed are given. (p. 441.) In "*fungous tumours*" of the joint the same treatment is strenuously recommended as the most potent means of re-establishing the general health. In proof of its efficacy in modifying the scrofulous diathesis, M. Bonnet says that he has found it most successful in obstinate scrofulous ophthalmia, it being indeed the only general treatment by which he has cured the disease without the intervention of local applications. He admits that it is much less efficient when "*fungosities*" or pus exist in the joints, but even then he has always found the plan safe, and, despite a certain number of failures, he says, "I have recently seen two cases in which the success was very remarkable. The first was that of a child, aged 4, affected with a number of intermuscular abscesses in both legs, round the elbow, and in the neck. After three months' treatment a perfect cure was obtained. "The second case was that of a child, aged 3½, whose hands were covered with fistulæ communicating with the bones of the phalanges and carpus, which were necrosed and infiltrated with pus;" there was a perfect recovery in four months. (Vol. ii., p. 34.) The baths are not recommended in the purulent or tubercular diathesis. And, finally, we are told that in chronic wandering pains, without any appreciable material alteration, provided the patient be not feeble, the plan "is of marvellous efficacy; no method can be compared to it especially in removing susceptibility to cold." In this instance M. Bonnet says *probatum est*—as he derived the greatest advantage from the plan in his own proper person; and he warns the patient not to be discouraged if he finds his sufferings increase at first, and to recollect that pains which have existed for years cannot be permanently cured without continuing the treatment from three to six months. (Vol. ii., p. 117.)

As to internal remedies in chronic diseases of the joints, M. Bonnet does not draw very largely on the *Materia Medica*. He thinks very favor-



ably of sulphureous and saline mineral springs, both as baths and internally. (pp. 178-88.) Iodine he thinks may be useful when the "*fungous*" diathesis is well marked; but he deems it injurious when the patient is pale and thin; and its full utility in any case cannot be obtained, he is convinced, unless iodine baths, according to M. Lugol's method, are conjoined with its internal administration. Tonics he rejects, except special indications call for them. Iron he deems quite useless. Cod liver-oil he speaks favorably of, without specifying the cases he gives it in. (Vol. ii., pp. 26-32.)

*Local Treatment.* We have stated the great importance M. Bonnet attaches, in all diseases of the joints, to placing the limb in a suitable position, and keeping it motionless in that position during a certain period; the former object is effected by manual extension, or permanent extension by means of a suitable apparatus, with or without tenotomy, according to the circumstances of the case; permanent immobility of a limb cannot, M. Bonnet urges, be obtained by simply confining a patient to bed. Instrumental means must be resorted to, so contrived that part of the joint shall remain exposed, that no pressure shall be exerted on the joint, and that the posterior surface of the limb shall be supported. For this purpose M. Bonnet prefers hollow or cup splints, made of a trellis of iron wire, which combine the advantages of being cheap, light, cleanly, and easily made, and, above all, of being easily moulded into the shape suited to each particular joint. M. Bonnet gives a minute account of the mode of preparing and applying those splints to each joint, for which we must refer to his work and the accompanying plates. We have also already stated M. Bonnet's views as to when immobility should be replaced by passive motion of the joint (vol. i., pp. 124 et seq.); but we must however here refer to the *results* of this practice.

In a recent Number of this Review (No. XXXVI., pp. 359-60), we had occasion to advert to the question of straightening the knee-joint during the existence of active disease, which has been long practised by Lugol, Duval, and some other French surgeons, and we expressed our more than doubts of the safety of the proceeding. M. Bonnet has, we believe, carried this practice further than any other surgeon, inculcating it, in fact, as a rule without limitation or exception; and it will therefore be well to give the general results of the cases detailed in the present work in which the method was adopted. With respect to acute cases, M. Bonnet gives six cases in which the knee, three in which the hip, and one in which the ankle were affected with "very intense acute arthritis, in which the antiphlogistic treatment proved useless until the joint was brought into a suitable position, a proceeding which was immediately followed by immediate and permanent amendment." (Vol. i., pp. 313 et seq.; vol. ii., pp. 205-10, 356-61, 447-50.)

"Those cases," M. Bonnet says, in his observations on some of them, "show the extent to which the most powerful antiphlogistics and the means best suited to allay pain may be powerless in inflammation of the knee, so long as the patient is permitted to retain the vicious position he has adopted; and how greatly we relieve the patient and check the progress of a formidable inflammation by placing and firmly retaining the limb in a suitable posture." (Vol. ii., p. 208.)



It is not however pretended that the method is infallible, M. Bonnet goes on to say—

“In all the cases which I have detailed, the constitution of the patient was good, and the acute had not supervened on a chronic inflammation. In all those cases the amendment consequent on straightening the limb was immediate and permanent. But the result has been different with patients of deteriorated constitution, or who were antecedently affected with chronic inflammation of the joint; in the latter circumstances indeed, even where the constitution was good, though the treatment produced a rapid amendment, it did not effect a complete cure, which, if achieved at all, required a long time to accomplish.” (Vol. ii., p. 210.)

As to the utility of the practice in chronic diseases of the joints, M. Bonnet seems to regard that as an admitted point, as he gives but one case as illustrative of its advantages; this is one of *fungous* tumour of the knee, and rectifying the position of the limb seems to have had a very beneficial effect. (Vol. ii., p. 227.)

M. Bonnet performed numerous experiments with the *actual cautery* and the *moxa*, which he regards as essentially different in their action, from caustic, inasmuch as they cause a high degree of local excitement. This excitement he attributes to the heat communicated to the subjacent structures, and he examined the circumstances which influence the propagation of the heat. The most important of these is the integrity of the skin. The transcurrent cautery heats the tissues to the depth of about half an inch; but if the cautery is applied so as to destroy the skin, the heat scarcely passes beyond the slough in contact with the iron, both because the contact of moisture cools the iron, and chiefly because the skin, being a better conductor of heat than the other structures, its integrity favours the propagation of the heat. The heat of a large moxa slowly burned penetrates much deeper than that of the cauterising iron. Those considerations, M. Bonnet thinks, explain the aggravation of symptoms sometimes observed after the application of the cautery or moxa to diseased joints, before the complete subsidence of any intercurrent inflammation, which he considers especially liable to occur in a superficial joint, being scarcely ever observed in disease of the hip or of the spine. (Vol. i., pp. 148-62.) M. Bonnet has often employed the transcurrent cautery in Hyarthrosis, and thinks its efficacy is greatly overrated; he has always seen the quantity of serum greatly diminished by its use, but it never completely disappeared, and the motions of the joint remained as much or even more embarrassed than previously. (Vol. i., p. 446.) “*Fungosity*” of the joints is the disease in which the cautery is generally indicated, and here M. Bonnet prefers the moxa, which he deems the most efficacious local means at our command in this affection. Excepting only placing the limb in a good position, and the judicious alternation of rest and motion, little is to be expected from a few applications of the moxa; it must be employed with energy and perseverance to do good, and in the tolerably numerous cases in which M. Bonnet has seen the moxa effect a slow but complete resolution, the whole anterior surface of the joint had been burned in succession; the moxa, too, he considers should be much larger than those usually employed. (Vol. ii., p. 48.) In the chapter on chronic abscess of the joints, M. Bonnet incidentally states that in chronic abscess of the cellular tissue, the best practice unquestion-

ably is to destroy the entire surface of the abscess with caustic or the cautery; and in the chapter on diseases of the hip-joint, we find four cases of large intermuscular abscesses in which this practice was pursued, and they are certainly sufficiently remarkable to demand notice. The first is the case of a girl aged 21, who had been for seven months affected with an abscess which occupied the entire outer aspect of the hip, and which it was presumed did not communicate with the joint. The abscess lay so deep that three successive applications of potassa fusa c. calce failed to open it, and it was then laid open by a vertical incision,  $7\frac{1}{2}$  inches long, and  $1\frac{1}{2}$  inch deep, made with the metallic cautery. A quantity of thin serous pus escaped, and on exploring the cavity, the finger was found to pass behind the great trochanter and neck of the femur. M. Bonnet now concluded that the abscess did communicate with the hip, notwithstanding the absence of symptoms to that effect, and hesitated for some time, but at length persevered in his original intention, and *extinguished* twelve cauterizing irons in the abscess so as to destroy its entire surface, including the sinus leading behind the neck of the femur. On the third day violent inflammation set in, which promptly subsided on the fifth day, after the action of an emetic. By the twelfth day the separation of the sloughs left a clean granulating surface, and in three months the patient recovered, with perfect use of the limb. Three other very similar and equally successful cases are detailed, in one of which the abscess was even larger than in the foregoing case. There is likewise a fifth case, in which M. Bonnet (though he elsewhere repeatedly says the practice is inapplicable when an abscess communicates with a joint), having to deal with an abscess connected with the hip, which had opened on the front of the thigh, cut away the undermined integuments to the extent of a hand's breadth, and applied chloride of zinc to the whole surface of the abscess, and to the fistulous passage leading to the joint; the condition of the patient, it is said, became much improved, but it does not very accurately appear to what extent. (Vol. ii., pp. 379-86.) M. Bonnet was assuredly very fortunate in the issue of those cases, but for our part, we should be slow to tempt fortune by following his example.

M. Bonnet attributes the mischief that so usually follows the opening of a joint, or of a large chronic abscess, to the admission of air,—not that he thinks air exerts any irritant action, but he attributes its bad effects entirely to its enabling the fluids (whether synovia, blood, or pus) which it comes in contact with to putrefy; and the decomposition of the liquids, he maintains, always precedes and accompanies the bad symptoms, the discharge becoming fetid and charged with air; so that the reality and progress of the putrefaction can be traced with test paper, showing that sometimes ammonia alone, and sometimes hydrosulphuret of ammonia is evolved. (Vol. i., pp. 24 and 263.) This decomposition leads to what M. Bonnet calls *putrid absorption*, in contradistinction to *purulent* absorption—the former characterized by the putrid discharge, by occurring at a later period, by being accompanied with intense continued fever, with the characteristic symptoms of extreme fetor of the stools and delirium, and by no internal abscesses being found after death; while the latter affection is ushered in by rigors, assumes a remittent form, is never accompanied with delirium, and abscesses are disseminated in

various organs. (Vol. i., p. 259.) It is to avert this *putrid* absorption that M. Bonnet cauterizes the surface of a chronic abscess; the surface thereby ceases to be an absorbing one, and on the detachment of the sloughs there is a healthy granulating surface disposed to heal, while *purulent* absorption is not to be apprehended—as, according to M. Bonnet, it never (?) follows the application of either the actual or potential cautery. (Vol. i., p. 381.) Our author is so persuaded that such is the fact, that he has adopted the practice of cauterizing the surface of a wound whenever purulent absorption appears imminent, preferring chloride of zinc for this purpose, as it alarms the patient less, and dries the surface of the wound more thoroughly than the actual cautery.

“I apply,” says M. Bonnet, “this method especially in wounds, the result of cancers of the breast, &c. . . . So convinced am I from experience of the prompt and evident utility of the practice, that when after an operation I see the wound ash-coloured, yielding a serous or fetid discharge, with the swollen painful margin foreboding phlegmonous erysipelas, there being also burning fever and thirst, I unhesitatingly cauterize the entire surface of the wound. Amputations excepted, I have not yet met with one case in which the mischief was not arrested by the practice, which more than any other circumstance proves the power of the cautery to limit and localize the lesion.” (Vol. i., p. 385).

In the case of joints, however, M. Bonnet admits that the cautery or caustic is inapplicable, for reasons too obvious to require being detailed. How, then, are we to proceed in cases of obstinate hydarthrosis, or chronic abscess of joints? Evacuating the fluid by a simple puncture is safe but inefficient. Subcutaneous section of the synovial membrane would in hydarthrosis be quite safe, and, M. Bonnet is inclined to think, very probably efficient, as he saw one obstinate case of the affection cured by a fall which ruptured the synovial membrane of the knee, and diffused the fluid into the cellular tissue; but still he says he has never adopted the practice. (Vol. i., p. 453.) By the way, we find he did; for we may here mention that M. Bonnet failed in attempting to repeat M. Goyraud's operation for extracting a loose cartilage from the knee-joint by the subcutaneous method; for he found it impossible to push the foreign body into the cellular tissue, though the abundant escape of synovia evinced that the synovial membrane was freely divided; after the operation, a previously existing hydrops articuli returned, which however the presence of the foreign body perhaps sufficiently accounts for. (Vol. i., p. 488.) Neither has M. Bonnet tried M. J. Guerin's proposal of evacuating the liquid, whether synovia or pus, by means of an exhausting syringe, being convinced that the liquid would again collect, as its mere evacuation would not modify the surface that secreted it, and the method he has adopted to attain this latter object is the employment of *irritant injections*.

The use of *iodine injections* in the treatment of hydarthrosis, serous cysts, chronic abscesses, &c. has latterly been warmly recommended by several French surgeons. M. Velpeau lays claim to having first applied the practice to the treatment of hydarthrosis, but M. Bonnet vindicates the priority in this particular for himself, as in March, 1841, he injected the knee-joint with iodine, and, encouraged by the success of this first attempt, repeated the operation ten times within the year in cases of

hyarthrosis and abscess of the knee. An account of some of those operations was published in May, 1842, whereas M. Velpeau did not communicate the result of his first two cases to the Academy of Sciences until the 8th October, 1842, in which communication it was stated that he had injected the knee with iodine in July, 1839. The details of those two cases have been since published by M. Velpeau, in his '*Recherches sur les Cavités closes*,' whence it appears that the injection was in each of them thrown, not into the joint, but into synovial cysts on the inside of the leg, probably formed by the sheaths of the tendons constituting the "*Patte d'oie*." M. Velpeau, however, concludes that the injection did enter the joint because of the inflammation of the whole articulation which followed. M. Bonnet urges that even if the fact was so, it occurred accidentally, and that the priority remains with him, but fully admits that the idea of the operation was suggested to him by the general principles for the treatment of serous accumulations contended for by M. Velpeau. So much for the question of priority; but it is more important to inquire what is the value of the matter in dispute; as a preliminary to which, the method itself must be described, and the origin of the practice noticed. It was in hydrocele that M. Velpeau first employed iodine injections; for deeming the wine injection, though very efficacious, too irritating, and seeing that it caused gangrene if accidentally thrown into the cellular tissue, he sought for some safer substance, and at length selected diluted tincture of iodine, being it would seem not aware, or at all events having forgotten, that, first, Mr. Martin, of Calcutta, and subsequently Dr. Oppenheim, of Hamburg, and several others, had anticipated him in the practice. M. Velpeau imagined that the resolvent action of iodine thus locally applied might be beneficial, as serous collections frequently depend on, or are connected with, congestion, hypertrophy, &c. of some adjacent organ, and he therefore sought to determine—1st, if tincture of iodine excited adhesive inflammation in close cavities; 2d, if it dispelled the chronic alterations which sometimes complicate dropsy of those cavities; and 3d, if, like wine, it caused gangrene when infiltrated into the cellular tissue. The first two questions he answers affirmatively, from the permanent cure of some hundreds of cases of hydrocele, accompanied with the almost constant disappearance of the hypertrophy of the testicle, which not unfrequently accompanies that disease. The third question he answers in the negative, because, on injecting tincture of iodine into the cellular tissue of animals, he never found it excite gangrene or even suppuration; and in seven or eight cases in which the injection accidentally escaped into the cellular tissue in the human subject, it was either harmless, or merely caused very limited suppuration. M. Velpeau being persuaded that tincture of iodine produces in close cavities purely adhesive inflammation, which scarcely extends beyond the point of the cavity it comes actually in contact with, was emboldened to extend its application from simple hydrocele to congenital hydrocele, and hydrocele of the hernial sac; and even in three instances attempted the radical cure of hernia by this method, pressure during all those latter operations being made at the inguinal ring to prevent the injection passing into the abdomen. In the two former of those affections he succeeded, but failed in the latter, the difficulty of injecting a simple hernial sac being such, that in his first attempt

he thinks the whole of the injection was thrown into the cellular tissue, whence copious suppuration ensued; and though in the second and third cases he succeeded in obliterating the sac without the occurrence of the slightest unpleasant symptom, the hernia returned after two months. M. Velpeau also employed the iodine injection with perfect success and safety in two cases of serous cysts in the iliac fossa—in an encysted sero-sanguineous collection behind the uterus, ascending to the right iliac fossa; in a serous collection situate between the triceps and the femur, five inches long by four in breadth; in two serous cysts of the axilla, three of the female breast, five of the neck, an unspecified number of similar tumours of the thigh, groin, and parietes of the abdomen, and also in four cases of encysted goitre; several of those various tumours being upwards of double the size of the clenched hand, and one as large as an ostrich egg. He further employed the injection in two *vast* purulent collections, one subclavicular, the other substernal; in the latter case with success, but in the former the tumour enlarged, the skin inflamed, and the cavity had to be laid open with the knife. Except in this last-mentioned instance, no unpleasant symptoms occurred in any of the foregoing cases, save in one case of encysted goitre, into which 100 grammes of the iodine injection were thrown (gramme = 15.444 grs.); on the sixth day there was high fever, and on the tenth an icteric tinge appeared on the face, neck, and greater part of the body, the fever gradually subsided, and the goitre was permanently cured. The icteric tinge was not exactly similar to that of jaundice, and M. Velpeau thence thinks it not impossible that it arose from absorption of iodine. Similarly favorable results were obtained with the various bursal cavities, except that the operation failed to effect a cure in about one half the cases of bursal tumours over the patella and the olecranon, a failure which M. Velpeau attributes to the inertia, low vascularity, and denseness of the structures in those regions. The next step was to try the method in tumours of the synovial sheaths of the tendons, in which it proved equally successful, when the tumour was unilocular and contained serum only, unmixed with fibrinous concretions. Thus M. Velpeau succeeded in several ganglions on the back of the hand and of the wrist, in two synovial cysts of the ham, each as large as a goose egg, in one between the tendon of the biceps and the radius, and in the case of the synovial sheath of the tibialis posticus; while he failed in a case of hour-glass tumour, containing hordeiform concretions, which extended from the palm of the hand beneath the annular ligament some way above the wrist. M. Velpeau now seriously entertained the idea of applying the iodine injection to the treatment of hyarthrosis, but was restrained, not so much from the dread of suppurative inflammation occurring, as from the apprehension of exciting adhesion of the synovial membrane, and thereby impeding the functions of the joint; but as this did not occur in the cases already referred to, in which M. Velpeau concluded that he had accidentally injected the knee while operating on a synovial tumour, which was supposed not to communicate with the articulation, he injected the knee directly in a series of cases, which we shall examine, together with those of M. Bonnet and other practitioners. (*Recherches sur les Cavités closes, &c.* pp. 112-65.)



To inject the knee-joint the leg must be extended. M. Bonnet pinches up a fold of the skin and passes in a hydrocele trocar at the base of this fold, in order that when the operation is complete the external and internal puncture shall not correspond, and thereby admit air. M. Bonnet also always holds the trocar as vertically as is consistent with the escape of the fluid, as air can scarcely enter while the trocar is full of liquid. M. Velpeau neglects these precautions, as he is satisfied by experience that the admission of a few bubbles of air is harmless. Both agree that it is not of much consequence at what point the joint is perforated, but prefer the upper and outer part of the joint, or rather whatever point fluctuation may be most evident at. M. Bonnet seems to use an ordinary trocar; M. Velpeau attaches great importance to employing an instrument not larger at the utmost than a hydrocele trocar. M. Bonnet merely allows six or eight drachms of synovia to escape, a quantity, in fact, equal to the bulk of the injection he throws in; M. Velpeau, on the contrary, empties the joint as completely as he can. M. Bonnet in almost every case injected pure tincture of iodine, calculating on its being diluted by the fluid left in the joint; M. Velpeau injects tincture of iodine diluted, it is not precisely said to what extent, but it would seem with from two to four parts of water. M. Bonnet allows either the entire or the greater part of the fluid to remain in the joint after the injection has been performed; M. Velpeau, on the contrary, seems to allow the whole of the injection to escape—we say *seems*, for there is great want of precision in many of M. Velpeau's statements. M. Bonnet would in future prefer M. Velpeau's method, as it excites greatly less inflammation than occurred in his own cases.

Ten cases of the operation are given by M. Velpeau in the work already quoted; but as two of them are doubtful, eight patients and nine operations remain, one patient having had hydarthrosis of both knees. We further find, from the *Gazette des Hôpitaux de Paris*, and *Gazette Médicale de Paris* (Sep. Oct. and Dec. 1845, and Jan. 1846), that he has since performed other operations, amounting to about fifteen in all, and also that M. A. Bérard has operated five times on two patients, and M. Robert once. M. Bonnet's operations are twenty-four in number—ten for hydarthrosis of the knee, performed on seven patients, both joints having been affected in three cases, and the same joint having been twice injected in another; and thirteen injections, some with alcohol and brandy, were practised in five cases of chronic abscess of the knee, six times within the space of three months in one case, three times in a second, and twice in a third. Finally, we may add, that M. Jules Roux, of Toulon, operated on a large hydarthrosis of the shoulder-joint, which contained 500 grammes of fluid. (*Bulletin de l'Acad. Roy. de Méd.*, 15th Jan. 1846.)

In all the cases which are detailed with any approach to precision, it appears that the operation was followed within a few hours by inflammation of the joint and fever; and in almost every case considerable, sometimes severe, pain was experienced at the moment of the operation. It is, however, very remarkable that in the great majority of cases the inflammation was moderate, though in some it assumed a very formidable character. Thus M. Bonnet, who writes with most praiseworthy candour, records some cases in the '*Bulletin de Thérapeutique*,' t. xxiii., Nov. Dec. 1842, which are only alluded to in the present work, and we there find the following statements:—



"The inflammation ran on with alarming rapidity and intensity. The patient shrieked with pain during the entire day. Forty leeches were applied in the evening without giving any relief. The swelling, which was much greater than before the operation, was rapidly increasing. The tension of the skin was extreme. At seven in the evening, alarmed at the rapid increase of the swelling, I could devise no other expedient to restrain it than again plunging the trocar into the joint." And in another case, "The inflammatory reaction was intense: during the night there was fever, restlessness, and even commencing delirium. The joint swelled, and the skin was red, tense, and burning; nausea and vomiting set in, and those symptoms lasted three days."

The only instance in which suppuration occurred was in M. J. Roux's case of hydarthrosis of the shoulder; and though the patient ultimately did well, the case sufficiently shows, together with those just quoted, how utterly unfounded is M. Velpeau's quiet assumption that "the safety of iodine injections performed according to certain rules is now beyond dispute." Indeed, during the academic discussion on M. Velpeau's report on M. J. Roux's case, enough transpired to show that an iodine injection, like any other stimulating injection, should be employed with great caution, and that it does not enjoy the peculiar mildness of action attributed to it by M. Velpeau. According to M. Velpeau, an iodine injection, if infiltrated into the cellular tissue, never excites gangrene, and when thrown into a close cavity never produces suppurative inflammation—two propositions which he founds on surgical experience, and on experiments performed on animals by himself and others. But during the discussion in question (to be found in the journals already referred to), it appeared that MM. Bavot and Bouley had seen suppuration, gangrene, and death caused by injecting iodine into the cellular tissue and joints of animals; that gangrene of the scrotum had occurred after the injection of iodine in a patient of M. Jobert's, and a patient in the Hospital St. Louis; while a third case is published in a recent number of the '*Clinique de Montpellier*.' The assertion that the iodine injection never causes suppuration in serous cysts is already disproved by M. J. Roux's case; and, moreover, suppuration and death ensued from injecting a cyst on the neck of a patient in the Hospital la Charité. It appears, then, that the iodine injection may produce precisely the same mischiefs that may follow the employment of wine and other stimulant injections proposed to be superseded by it; and we fully agree with MM. Blandin, Roux, and others, that though no mischief is yet known to have occurred from injecting it into the joints, disastrous results must inevitably follow from perseverance in the practice. There are few proceedings in medicine or surgery, however hazardous, which cannot be supported by an array of successful cases; and we would just as soon infer the safety of injecting the knee-joint in hydarthrosis from the cases we have enumerated, as we would infer the safety of passing a seton through the knee in the same disease from the result of ten cases reported to have been so treated with success by Dr. Müller. (*Gaz. des Hôp.* June, 1842.)

But, supposing the practice safe, what is its efficacy? We must premise that, as regards hydarthrosis, the cases are, for the most part, so loosely described that it is often difficult to determine their nature; but it is yet quite clear that many of them were not cases of simple hydrops articuli, but that some more serious chronic alteration coexisted with the serous

accumulation. Two only of M. Bonnet's cases terminated in a perfect cure; but M. Bonnet candidly says: "Both these patients were young (16 and 18), and in both the hydarthrosis was recent, having existed but eight days in one, and not quite three months in the other case. There was no crepitation in the joint, nor any tumefaction of the superficial soft parts." (Vol. i., p. 464.) In his third and fourth cases some stiffness of the joint remained, notwithstanding the employment of various accessory treatment. In the fifth case, the motions of the joint were somewhat *more impeded* after the operation. We have an accurate account of nine only of M. Velpeau's cases; in three the operation failed to effect a cure. We are not told the precise condition of the joint when the other six were pronounced to be cured; but only three recovered quickly, and those were simple uncomplicated cases, of recent date, in young healthy patients. We may here remark that one of these six patients died of fever three months after the operation, and the cartilage of incrustation was found thin and somewhat eroded, and the reticular structure of the condyles of the femur was highly vascular; death was, however, clearly unconnected with the operation. The three cases operated on by MM. Bérard and Robert were neither benefited nor injured; the disease, whatever it was, ran on its course, and amputation in the three cases was performed at a remote period.

It is useless to comment on these facts, beyond the simple inference that the operation cured those cases, some of which would certainly, and others probably, have recovered under the usual methods of treatment, and failed, in like manner, where those methods would have failed. We cannot, however, avoid noticing an amazing discrepancy between the precepts and practice of M. Bonnet and M. Velpeau. They both urge that blisters, moxas, &c., should be "*exhausted*" before recurring to injection in the treatment of hydarthrosis. M. Velpeau, in particular, dwells upon the efficacy of "*monster*" blisters enveloping the entire circumference of the joints, and mentions some apparently intractable cases in which he found his plan succeed; yet we find M. Velpeau adopting the injection in the first instance in almost, if not all, his latter cases; and M. Bonnet actually 'resorted to it in simple hydarthrosis of eight days' standing! We are not in possession of materials to form an opinion founded on facts respecting the value of iodine and similar injections in chronic abscess, but, from the results of the methods more or less analogous which have been tried, we would anticipate that it is a hazardous proceeding. The instances in which M. Bonnet has adopted this practice are but few. In one case of chronic abscess, double the size of the clenched hand, situated over the scapula, he evacuated half the pus and replaced it—not, indeed, with iodine injection, but with camphorated spirit of wine, which was left *in situ*. As no effect was produced, the injection was repeated three days after; violent inflammation, severe pain, and high fever now ensued, and to relieve the patient's sufferings caustic was applied to the tumour, where it seemed most disposed to open; in about six weeks recovery was complete. In the cases of chronic abscess of the knee on which M. Bonnet operated, the tumour partly consisted of fungous and lardaceous tissues; to use M. Bonnet's language, the "*fungous*" coexisted with the purulent diathesis, and three of the patients were of the respective ages of two, seven, and

nine years. In one case alcohol, first, and three months after, undiluted tincture of iodine was injected. We are merely told that the reaction was very trifling, and that the patient left the hospital in a very satisfactory condition; as the joint contained no liquid, was exempt from pain, and its motions free. In a second case undiluted alcohol was injected, which caused scarcely any reaction; after thirty-four days the injection was repeated, very slight inflammation followed, and in three weeks some slight stiffness of the knee alone remained. In the third the abscess had existed for a long time and was very large, extending to the outer side of the joint and under the triceps to the middle of the thigh. Six irritant injections, the two first tincture of iodine, the third brandy saturated with camphor, were practised in the space of three months; the latter injection excited some reaction, the puncture opened, pus escaped freely, and the cavity under the triceps became gradually obliterated, but the joint continued to secrete pus copiously. A fourth injection (tincture of iodine) producing no effect, nine days subsequently the joint was injected with the still more stimulant liquid Fioraventi's balsam, which caused moderate inflammation without fever. The suppuration now gradually lessened, increased for some days after another injection of tincture of iodine, then again diminished, and in a fortnight the last fistula closed, the joint being free from liquid, and merely somewhat stiff. The fourth case only differs from the foregoing in that the injection (tincture of iodine three times repeated) caused severe pain; and though the patient left the hospital with an useful limb, the disease returned some months afterwards, in consequence of an injury of the joint, and the limb was amputated. The fifth patient was a woman, aged 28, who died of peritonitis fourteen days after the knee was injected with tincture of iodine. On dissection there was no trace of inflammation of the synovial membrane. In this case M. Bonnet says the tincture of iodine seemed almost inert; and he finally condemns the operation as useless in adults, restricting it to young patients whose constitutions are not too much deteriorated. (Vol. ii., pp. 84-96.) Since the publication of M. Bonnet's work, several other surgeons have employed iodine injections in the treatment of chronic abscesses, but we have not met with the particulars of their cases. We find it, however, generally stated (Gaz. des Hôp., Dec. 1845) that M. Jobert has cured several large chronic abscesses by this method, one being an "*enormous*" chronic abscess on the cervico-dorsal region, so large that little expectations of recovery were entertained. M. Velpeau's two cases already indicated are not given in any detail; suppuration, however, occurred in one of them. With respect to serous and other analogous collections, the occasional, perhaps frequent, efficacy and safety of iodine injections, cannot be disputed, but we are strongly convinced that their application in large accumulations of the kind is attended with great risk. We have already adverted to some facts which sufficiently bear out this conviction; and we may here observe that M. Bonnet's case of chronic abscess over the scapula shows how utterly unable we are to predict confidently what the effect of an irritant injection in a given case shall be, camphorated spirit having in the same patient at one time proved inert, at another having excited severe inflammation.

We have entered rather fully into the question of iodine injections, as

they are now very extensively used on the Continent, and the confidence with which M. Velpeau and others proclaim their constant safety and almost constant success, is calculated to induce others to imitate their boldness. We may here mention, as illustrations, how indiscriminately these injections are employed by some, that Dr. Suytgaerens, of Puers, after performing paracentesis of the thorax in a case of empyema, repeatedly injected the pleura with an iodine injection, the result, we must add, was favorable, and the patient recovered. (*Gaz. des Hôp.*, Fevr. 1845.) More recently, M. Dieulafoy, of Toulouse, in a case of ascites, threw a quantity of iodine injection into the peritoneum, and, after diffusing it over the entire cavity, drew off about half the quantity of fluid injected. Febrile reaction, with slight pain in the abdomen, followed; cataplasms and mercurial frictions were directed. A month after, about half the cavity of the peritoneum seemed obliterated, but the fluid, having again collected, superiorly, the injection was repeated with similar consequences. In about six weeks a small quantity of serum had accumulated, forming a circumscribed globular tumour; a third injection produced the same febrile and inflammatory symptoms. The ascites now finally disappeared, and was replaced by general anasarca, which yielded to purgatives. The patient ultimately recovered after a long convalescence, and experienced no inconvenience, save a dragging sensation in the abdomen on assuming the erect position. Finally, to conclude this subject, it may be noticed that, during the academic discussion so often referred to, it was suggested by MM. Laugier and Caventou, that the stimulant effects of diluted tincture of iodine were solely due to the alcohol it contained, and that the iodine was, so to say, mere surplusage. It may be, and we incline to the opinion, that there is nothing special in the local action of iodine, but unquestionably it powerfully contributes to the stimulant properties of those injections, as might be inferred from its action on the skin, and as is directly proved by the fact, that M. Bonnet has sometimes employed an injection composed of iodine, iodide of potassium, and water, and found that it excited active inflammation.

We have now noticed pretty fully whatever seems most peculiar in the general methods of treatment recommended by M. Bonnet, and what we have said respecting their application in particular cases precludes the necessity of examining that part of his work which treats of the diseases of each particular joint, especially as the author for the most part confines himself to the exposition of his own views, which have, we trust, been already sufficiently explained. A few points, however, require notice.

More doubt and difficulty occasionally arises in pronouncing an accurate diagnosis in certain cases of diseases of the hip than occurs in affections of perhaps any other joint. Thus, the apparently simple questions of determining whether the affected limb is really or apparently longer or shorter than its fellow? and, in either case, what is the cause and symptomatic value of the phenomenon?—have received the most contradictory answers from writers of acknowledged eminence. The investigations of MM. Bonnet, Maisonneuve, and Parise, especially of the latter (*Archiv. Gén. de Méd.*, July—Aug. 1843), have done much towards elucidating this difficulty.

Sir B. Brodie particularly pointed out the influence of lateral inclination of the pelvis in producing elongation, and occasionally also shorten-

ing, of the limb in morbus coxæ, according as the pelvis is elevated or depressed towards the affected side; and, since his observations, many, probably most, surgeons admit that the elongation is always, and the shortening frequently, apparent. M. Malgaigne next observed the curious fact that, if one side of the pelvis is strongly depressed, the lower extremities being perfectly parallel, both inspection and measurement then indicate a difference of eight or nine lines between the length of the two limbs—the limb corresponding to the depressed side of the pelvis *appearing longer to the eye*, but seeming *shorter by measurement* than its fellow, while the opposite limb, on the contrary, though the *shorter to the eye*, seems the *longer on measurement*. M. Malgaigne attributes this entirely to the lateral inclination of the pelvis approximating the crest of the ilium and the great trochanter on one side, and separating them on the other; while M. J. Guerin says this is but a particular case of the general rule, that any obliquity of the thighs to the axis of the pelvis modifies the apparent length of the lower extremities, whether determined by inspection or by measurement. Fricke also observed that the apparently elongated limb was found to be shorter than its fellow on measuring from the iliac spine to the outer malleolus, and mistaking this for real shortening. He attributed it to spasmodic action of the muscles, compressing the head of the femur into the cotyloid cavity. Such seems to have been the state of the question when MM. Bonnet and Parise commenced their inquiries.

MM. Bonnet and Parise both admit two varieties of elongation and shortening of the limb in morbus coxæ—*apparent* lengthening and shortening, and *real* lengthening and shortening—the former entirely produced by the position of the limb in relation to the pelvis, the latter depending on various causes. And first as to *apparent* variations of length.

The axis of the cotyloid cavity looks downwards, outwards, and slightly forwards, and the head of the femur, during every motion of the thigh, may be considered to revolve on an imaginary axis, antero-posterior in adduction and abduction, transverse in flexion and extension, parallel to the axis of the limb in rotation, and constantly changing during circumduction. All these axes traverse a common point, the centre of motion, which is *quam proxime* the centre of the head of the femur, and therefore of the cotyloid cavity. The distance between the centre of motion and the anterior iliac spine is invariable; but if lines are drawn from the centre of motion to the anterior iliac spine and to the external malleolus, those two lines move on each other like the legs of a compass, and, as they approach or recede from each other, their extremities approximate or separate. It is obvious, then, why measurements from the spine of the ilium to the malleolus vary with the position of the limb; and M. Parise's experiments, which seem to have been most accurately conducted, show—1st. That adduction increases and abduction lessens the length of the limb as *measured* from the anterior superior spinous process of the ilium to the malleolus. 2d. That the maximum elongation occurs during abduction combined with extension, as, on complete extension of the lower extremity, the iliac spine, the centre of the hip, and the outer malleolus, are in the same line. 3d. That abduction with flexion causes the greatest shortening. 4th. That, on measuring from



the *posterior* superior spinous process of the ilium, the length of the limb also varies with its position, but inversely to the variations in the foregoing cases; for, as the posterior spinous process lies on a plane posterior, superior and internal to the centre of the hip-joint, the line joining those two points, runs upwards, backwards, and inwards, or in the reverse direction to a line joining the centre of the hip and the anterior iliac spine; whence the position which gives the greatest length on measuring from one of those points gives the least on measuring from the other.

Admitting that apparent difference of length is caused by the position of the two limbs being different in relation to the vertical axis of the pelvis, it would seem very easy to restore symmetry, and so remove the cause of error; and so it is when the disease is recent; but after some time we generally can only bring the trunk and the lower extremities into the same vertical line, and a deviation of the pelvis still exists, which cannot be removed, and the limbs are examined while really unsymmetrical with the axis of the pelvis. What, now, is the cause of the deviation of the pelvis?

MM. Bonnet and Parise both dispute Brodie's well-known explanation of the lateral depression of the pelvis towards the affected side in the early stage of morbus coxæ; for, were it true, the phenomenon should not occur, as it does, in cases where the patient has been confined to bed, and M. Parise maintains that when the pelvis is depressed on the diseased side, the position, so far from being selected by the patient as most commodious and least painful during standing and walking, is a false position and assumed from necessity, though the least commodious. M. Bonnet proposes the explanation, that patients in the early stage of morbus coxæ lie on the affected side, in which position the thigh is flexed on the pelvis and somewhat abducted, while, at a more advanced stage, pressure on the joint being painful, they lie on the sound side, whence the diseased limb is flexed, adducted, and rotated inwards. This amounts to saying that the pelvis is really not deviated, but that the limb makes an angle with the pelvis, and not the pelvis an angle with the limb. M. Parise's explanation is also based on this latter principle, but his exposition of its causation and operation is more ingenious and more consonant with facts than that of M. Bonnet. He maintains that the inclination of the pelvis is always subordinate to the position of the femur, and thus traces the influence of the position of the limb in the several stages of hip-joint disease. In the first stage the limb is almost constantly abducted, flexed on the pelvis, and slightly rotated outwards. The cause of this position is, M. Parise maintains, the presence of more or less fluid in the capsule of the hip-joint, for analogy and observation show that one of the first effects of the disease is to cause accumulation of more or less synovia in the joint, and M. Bonnet's experiments on injecting joints, repeated and confirmed by MM. Parise and Maisonneuve, prove that distension of the capsule of the hip with liquid causes the thigh to assume the position in question, a position which the patient more willingly observes, as pain is relieved from the capsule being then at its maximum capacity. But if the patient tries to walk when one limb is thus abducted, in order to avoid walking with the limbs separated, which would be very inconvenient, he approaches the sound to the diseased limb, that is to say,



*adducts* the sound limb, and then the lower extremities form, with the vertical axis of the pelvis and the trunk, a re-entrant angle towards the diseased side. But now the body is in a state of unstable equilibrium; and this, again, is corrected by flexion of the trunk on the pelvis towards the sound side, so as to bring the axis of the trunk in a line with that of the lower extremities. In the recumbent posture the same thing occurs also, for, unless the patient lies with the legs separated, the sound leg is approached to the other, and the resulting angular posture being very inconvenient, is as far as possible remedied by flexing the loins towards the sound side. M. Parise thus concludes that the pelvis itself is not deviate, that it is in fact a fixed point, on which the lower extremities are inclined laterally in one direction, and the trunk in the opposite direction. It is unnecessary to go in so much detail through the effects of the other deviations of the lower extremities. Each of them produces an inverse deviation of the trunk on the pelvis, in obedience to the law of equilibrium for preserving the line of gravity perpendicular, or nearly so, to the ground. Thus, the *flexion* of the thigh induces concavity of the loins from inclination of the trunk backwards, whence is caused apparent inclination of the pelvis forwards; and its *rotation outwards* induces torsion of the trunk, whence, as the position of the iliac spine is compared to the anterior plane of the trunk, the ilium on the affected side seems carried forward, while the apparent torsion of the pelvis will be in the reverse direction when the femur is *rotated inwards*. Again, at a certain advanced period of the disease, and sometimes at its commencement, the thigh becomes *adducted* and rotated inwards, continuing flexed. The cause of this *adduction*, M. Parise says, is, when it is primary, the existence of but a small quantity of fluid in the joint; and, when it is secondary, the gradual yielding of the capsule, but chiefly the escape of fluid from its erosion during the progress of the disease; but, be its cause what it may, when established, its effects are the reverse of those of *abduction*, viz. elevation of the affected side of the pelvis. The explanation of the results of inspecting and measuring both limbs in the first and second stage of morbus coxæ is obvious. *Apparent differences* to the eye result from our examining the lower extremities of two levers of equal length, whose upper extremities form unequal angles with the horizontal line connecting them. The cause of *apparent differences by measurement* has been already seen. It is equally obvious that, provided the malleoli are in contact, apparent shortening to the eye always accompanies apparent elongation by measurement, and vice versa. It might thence be naturally inferred that these two modes of examination would exactly correct each other; but such is not the fact, for it is curious that *lengthening to the eye* from abduction follows a rapid progression, and is not constantly proportional to the *shortening by measurement* of the same limb.

With respect to *real lengthening* of the limb in morbus coxæ, we shall merely notice such points in M. Parise's researches as contradict some pretty generally received ideas. It is, we believe, the prevalent opinion, founded both on theoretical considerations and on M. Fricke's experiments, that neither accumulation of fluid nor thickening of the structures within the hip-joint can protrude the head of the femur so as to cause

elongation of the limb. Even M. Bonnet, who ascertained that liquid injected into the hip collects between the head of the femur and the bottom of the acetabulum, denies that any appreciable lengthening of the limb is caused thereby. M. Parise, however, and also M. Maisonneuve, found the limb very obviously elongated after injecting the joint, and from this and other experiments, such as introducing folds of lint into the acetabulum, &c., which it is unnecessary to dwell on, they conclude that accumulation of liquid or thickening of the structures within the joint must necessarily cause lengthening of the limb so long as the upper wall of the cotyloid cavity is intact. With respect, however, to increase in the size of the head of the femur causing lengthening of the limb, though M. Parise, on repeating Fricke's well-known experiments, found them erroneous, yet he concludes that in the rare cases in which the head of the bone is enlarged, lengthening of the limb cannot ensue, as in every known case of the kind the cotyloid cavity was proportionably enlarged also. As to the causes of *real shortening* of the limb without dislocation, M. Parise, on repeating Fricke's experiments, obtained results directly the reverse of those of that author, as artificial diminution of the head of the femur constantly produced shortening of the limb. The most important case, however, of true shortening without dislocation, is atrophy of the bones of the lower extremity, arising from arrest of development. This occurs very frequently, and, if overlooked, may occasion serious errors of diagnosis and prognosis; but error is avoided by measuring each limb comparatively from the great trochanter to the condyle of the femur, and from the head of the tibia to the outer ankle.

However clearly the causes of apparent alterations of the length of limb may be ascertained, still much difficulty often exists in determining the question practically. In the case of apparent elongation a satisfactory conclusion can indeed be obtained by placing the lower extremities symmetrically, and adopting the well-known method recommended by Sanson and A. Bérard; but, in apparent shortening, as the diseased limb is adducted, the limbs cannot be similarly posited, for though the sound limb may be brought to the same degree of adduction as the other, yet to do so it must be either more flexed or extended; and, if more flexed, it would seem shorter; if more extended, longer by measurement than it really is. M. Parise, therefore, concludes that we can only approximate to the truth in this case by taking the mean between measurements of the limb in those two positions; while M. Bonnet maintains that the difficulty can only be overcome by restoring the affected limb to its natural position, which often requires some time and the aid of suitable apparatus. As to the symptomatic value of alterations of the length of the limb, M. Parise concludes that *real shortening* from *atrophy of the bones* is a bad symptom, not merely because the limb must remain permanently shortened, but because it indicates a bad form of disease; that *apparent lengthening to the eye*, resulting from abduction and flexion, indicates a large collection of liquid in the joint, which may, by protruding the head of the femur, also occasion *real* lengthening; and that dislocation is imminent when this elongation, after having been decided, diminishes slightly, but motion continues as difficult and painful, and the great trochanter, previously prominent, becomes still more so. When *apparent shortening*,

arising from adduction and flexion, exists at the commencement of morbus coxæ, the quantity of liquid in the joint is small, and the disease has probably commenced in the hard structures; when it succeeds apparent elongation, the capsule of the joint is probably perforated. Shortening *both* to the eye and by measurement, with prominence of the great trochanter, indicates dislocation; or, if the great trochanter is not prominent, erosion of the cartilages, or more or less destruction of the auricular surfaces, according to the amount of shortening present.

M. Bonnet absolutely rejects resections in any of the large joints, even of the upper extremities. "They are," he says, "more fatal than amputations; and, after having been long considered one of the most brilliant conquests of modern surgery, they have now fallen into merited oblivion. I cannot even imagine any case in which resection of a large joint could be indicated." (Vol. i., p. 165.) We shall not stop to dispute or refute this sweeping condemnation, to which, indeed, we should not have adverted, were it not that, in addition to the resection of those joints, now pretty generally considered admissible, M. Bonino theoretically, and Mr. Fergusson practically (Med. Chirurg. for 1845, vol. xxviii.), have recently inculcated the propriety of resecting the head of the femur in certain cases.

We believe that all the known cases of resection of the head of the femur are referred to in M. Bonino's and Mr. Fergusson's papers. M. Bonino enumerates twelve, including one doubtful case (Kluge's); and to these we may add Mr. Fergusson's own case, and one which he has been informed, by Mr. C. Hawkins, occurred in the practice of Sir B. Brodie. Excluding Kluge's doubtful case, thirteen remain, which may be thus classed. In two cases (Oppenheim, Seutin) the operation was performed in consequence of fracture of the upper extremity of the femur by gunshot wounds. Ten were cases of disease of the hip-joint. And in one (Textor), though probably, if not certainly, the patient, a child aged 7½ years, in the first instance laboured under disease of the joint, it was discovered after death that a fall at an early period of the affection had caused fracture of the neck of the femur, of the horizontal ramus of the pelvis, and of the ascending ramus of the ischium; and that those fractures, the existence of which was not suspected during life, had occasioned suppuration of the surrounding soft parts.

In appreciating the results of resection of the head of the femur in disease of the hip, the last case must clearly be left out of consideration. The two cases in which the operation was performed in consequence of gunshot wounds, do not directly bear on our present purpose, but we may say thus much respecting them, that, although they both terminated fatally, we are yet of opinion that in any similar case where the great vessels and nerves were intact and the soft parts not extensively injured, resection would be decidedly preferable to amputation at the hip, though the contrary opinion is advocated by M. Lailard (*Relation Chirurg. du Siège de la citadelle d'Anvers*). Of the ten patients on whom the operation was performed because of disease of the hip-joint, six recovered (White, Schlichting, Schmaltz, Heine, Vogel, Fergusson). All those patients were young, the two oldest, those of Schlichting and Mr. Fergusson, being both aged 14. The remaining four patients died (Hewson, Brodie,

Textor, two cases); they were all older than those in whom the operation succeeded, the youngest being, so far as we can make out, one of Textor's patients, aged 18.

The cause of death in the fatal cases seems to have been directly referrible to the operation but twice, that is to say, in one half of the fatal cases, and in one fifth of the whole number operated on for disease of the joint. Mr. Hewson's patient survived three months, and died from profuse suppuration, coexisting with, if not caused by, caries of the pelvis. One of Textor's patients survived fifty-four days, and died from sloughing over the sacrum, when the wound was nearly cicatrized and the upper extremity of the femur surrounded by newly deposited bone. A second of Textor's patients died in four days, and, though there was tubercular disease of the mesenteric glands, which, if known, would have contraindicated interference, yet death must be attributed to the operation, as was also, we are told, apparently the case with Sir B. Brodie's patient; but no particulars are given of this case, save that the patient was an adult; that the head of the bone was in the acetabulum at the time of the operation, and that the patient died a few days after it was performed.

The result of the operation in the successful cases was more favorable than might have been *a priori* anticipated. With respect to the patients of Schmaltz and Heine we are merely told that they recovered. A false joint formed and an useful limb was enjoyed by White's, Schlichting's, and Vogel's patients; and, in Mr. Fergusson's case, where  $4\frac{1}{2}$  inches of the bone, measuring round the curve of the neck, and shaft were removed, the limb, six months after the operation, was about  $2\frac{1}{2}$  inches shorter than its fellow, and a false articulation was evidently in process of formation at the hip.

The indication for the operation in disease of the hip-joint is the difficult part of the question. Most surgeons reject the operation, on the ground that we cannot tell when the cotyloid cavity is implicated in the disease, in which case the operation must generally be useless. We believe that the disease most frequently, though by no means constantly, commences in the head of the femur, and that the bones of the pelvis become subsequently implicated; but, granting this, it only raises the presumption that the cotyloid cavity would be found healthy during the early period of the malady, when resection of the head of the femur would be utterly inadmissible. In all the cases of which we have any tolerably accurate account, in which the operation was performed, abscesses had opened externally, and the head of the femur could be felt denuded and isolate from the surrounding soft parts, the very stage of the disease in which the cotyloid cavity is usually implicated. But we may here mention that, though all Textor's patients died, yet the cotyloid cavity was found healthy in two of them; in the third it was diseased, and the actual cautery was applied after the surface of the bone was cut away. It is easy, however, to imagine a condition of things (we have seen it more than once) in which the diseased head of the femur, lying at the bottom of an ulcer or of a fistulous abscess, excites all the mischief of a foreign body, and in which its removal would seem to hold out the only chance of recovery. Under such circumstances, which existed in Mr. Fergusson's case, the operation may be justifiable, but it is one which, in the present state

of our knowledge, cannot be indicated or contraindicated by fixed rules; every thing must depend on the discretion and judgment of the surgeon, after carefully weighing all the circumstances of the particular case before him.

We scarcely feel it necessary to express any summary opinion as to the merits of M. Bonnet's work, after the very ample analysis we have given of its contents, the more especially as we have, for the most part, coupled that analysis with an expression of opinion respecting the author's views and practice. We shall therefore conclude this rather lengthened notice for the present; we say, for the present, inasmuch as we shall soon have occasion to return to the subject of diseases of the joints, when many matters not adverted to in this and the preceding article shall receive fitting consideration.

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#### ART. VII.

*Lectures on Pulmonary Phthisis, delivered in Jervis-street Hospital; comprehending the Pathology, Diagnosis, and Treatment of the Disease, with an Appendix.* By J. T. EVANS, M.D., &c.—Dublin, 1844. 8vo., pp. 196.

THIS is a curious book in more respects than one. It is written by a member of the profession who formerly "spent the greater part of his time for some years in assisting St. John Long in his daily frictions;" and who now proclaims that, "after twelve years of regular medical study, he does not think the time that he was thus so strongly engaged *by any means misspent*." (p. 131.) What this author may have gained either in an intellectual or moral point of view during the period he refers to, neither appears on the face of things, nor is stated in his work. On the other hand, he, in the page following that which contains the above declaration (p. 132), characterises his past conduct as "*a reckless trifling with human health and life*;" whence it should follow, but we believe it does not follow, that, in the estimation of Dr. J. T. Evans, time passed in a reckless trifling with human health and life is not by any means misspent. It is highly probable that St. John Long held the same creed, until the death he had been so active in dealing unto others paralysed his arm for ever.

The author is perfectly aware that multiplication of books is an evil; consequently, "if the present work did not contain some things which he believed to be both novel and useful, it should never have been offered to the public." As the "best apology" for bringing it before the profession, the writer alludes in the preface to "*some of the most important and original*" views discussed in his pages. We proceed to attend him in his references to these views.

The reader is first informed that "the presence of tubercles appears, in the opinion of authors, to constitute the essence of the disease,"—a notion which Dr. Evans is earnest in proving to be hollow and erroneous. If Dr. Evans will produce evidence that one single author of repute teaches the doctrine he here ascribes to the class, we will acknowledge there is here something more than a rather worn-out artifice of gentlemen desirous



of notoriety as reformers. Does Louis teach the monstrous absurdity which he is here stigmatized as promulgating? No; he simply tells us that tubercle constitutes the anatomical character of phthisis. Does Carswell's voice give justification to that error? Far from this; it announces the blood to be diseased from the first in phthisical subjects, and this before the development of local tuberculization. Does Clark, in his practical volume, give any colour of justice to the accusation of Dr. J. T. Evans? Less than all others even; for in the state which he styles "tuberculous cachexia," a state fully evolved before the generation of pulmonary tubercle, he recognizes the physical substratum of the disease. But if Dr. J. T. Evans has less of the character of an original teacher here than he would desire to persuade the pupils of the Jervis-street Hospital, we willingly concede him the merit of perfect unconditional originality in the announcement that "tubercles exert little or no influence on the progress of phthisis!" This is something new with a vengeance. What! tubercles which lead to inflammation of the pulmonary substance, pleuritis, perforation of the pleura, pneumo-thorax, pleuritis sometimes accompanied with agonizing pain, in itself capable of shortening existence,—tubercles which block up and render useless or actually destroy (it may be) three quarters of the entire pulmonary substance,—which cause a reduction *pro tanto* of the oxygenating surface for the vivification of the blood,—which are followed by the obliteration of much of the old, and the generation of a new, vascular system in the lung,—which lead to the development of similar tubercles in the meninges, a development that may cut off individuals in a few days,—which cause ulceration of the intestinal surfaces, ulceration that by its very cure sometimes destroys life, and by its propagation renders existence torture,—what! tubercles, which do all this, exert no influence on the progress of phthisis! Verily, there is novelty and genius here; and when Dr. Evans proves his thesis, none but persons, in whom jealousy knows no limit, will dream of contesting his right to an excessively capacious niche in the Temple of Fame.

The next discovery made by the author is that "morbid anatomy is not pathology,"—a novelty absolutely of the first water, and which is about of the same character as the following:—healthy anatomy is not physiology: whereof we make Dr. Evans free gift for his next course of lectures in Jervis-street.

Dr. Evans objects to the division of the disease into two stages, as totally insufficient; and has divided the disease into several stages, corresponding to what he believes to be the progress of the *lesions*,—a word, by the way, not English in the Gallic sense in which it is here employed. The proceeding seems somewhat inconsistent for a person who regards the main of these *lesions*, namely, tuberculous matter, as a matter of no consequence. But *aliquando bonus Homerus*—and why should not Dr. J. T. Evans be entitled to his occasional snooze?

The next novelty is the reference of several of the ordinary symptoms of phthisis, "especially *those of impeded circulation*, to the condition of induration, the result, as I believe, of chronic pneumonia, which so frequently takes place in the neighbourhood of tubercular masses or of cavities." Heaven help our ignorance; we had fancied that, since Dr. Carswell demonstrated the connexion of the said impeded circulation and



chronic pneumonia, the point was common property. But here Dr. Evans lays rude hands on the bantling, and affirms he is its father. We simply demur until he shall have proved his paternity.

Dr. Evans holds, that in M. Fournet's book, there are "many *old observations* put forward with an imposing air of novelty, and many *new assertions* destitute of foundation,"—an opinion we should not have thought it necessary to notice, except from its furnishing, we would humbly suggest, a singularly appropriate epigraph for Dr. Evans's own second edition. This stern pathologist and modest lecturer considers M. Louis's "etiology illogical, his diagnosis rather meagre, and his treatment inadequate;" but, probably fearful of blasting the French author's reputation utterly, Dr. Evans condescends to observe, "*I cannot but thank M. Louis for the candour and truthfulness of his observations.*" (p. 192.) Happy the man who receives thanks from so eminent a quarter: it is truly comforting to know that, after one may have learned to think highly of having enjoyed the highest European reputation for some dozen years, and felt that even that earthly good is tainted with evil, a vote of thanks may come from Dr. Evans to renovate the delightful feelings of early success.

Turn we next to the author's lectures on treatment,—a chapter which might be expected, in consideration of his early occupations, to be particularly instructive. These lectures open with various flourishes concerning "fond and devoted parents,"—rescuing victims from "the dark recesses of the lone and silent tomb," &c. &c., and next proceed to show that the profession has not advanced a step towards improved treatment of the disease since the days of Celsus. Anon the lecturer displays in learned phrase the folly of persons who treat symptoms of the same name always by the same remedies, and illustrates the said folly by sundry examples. Take one of these, and *ab uno disce*—"The *night-sweats* may depend on the rapid circulation of the fever of emaciation occurring in the relaxed condition of deficient growth; or they may be critical to the repeated forms of intercurrent pneumonias, tuberculization, and supuration already referred to; or they may be vicarious to the natural discharges from other surfaces, as when they are accompanied by constipation." (p. 97.) Now, we very candidly confess our inability to comprehend what is meant by "the rapid circulation of the fever of emaciation occurring in the relaxed condition of deficient growth,"—so that not for worlds would we dispute the reality of the influence which Dr. Evans ascribes to the thing, whatever it is. Secondly, we deny that any human being has ever proved, or even in any remote degree made it probable as a matter of fact, that the night-sweats of phthisis are "critical" to intercurrent pneumonias or other of the states mentioned. Thirdly, it has clearly been shown by Louis,\* that the notion of the vicariousness of sweats in the disease is a mere fallacy, the offspring of fancy and not of observation.

Dr. Evans proceeds to inquire, "what, in fact, are the lesions in which phthisis pulmonalis necessarily consists?" The response informs us that among these "lesions" rank, for example, "diminished force of growth,"

\* *De la Phthisis*, p. 215, ed. 2. Paris, 1843,

and "an excessive nervous excitability." Very remarkable "*lesions*;"—let that pass, however. But we cannot let pass the following piece of inductive philosophy, designed for the instruction of youths about, sooner or later, to have on their heads the dreary responsibility of actually treating phthisis :

"We have likewise previously seen, that a diminution in the waste or decomposition of the tissues is equivalent to an increase of supply or growth; and it is a fact, independent of all theory, that a diminution in the amount of respiration will produce this effect. It was on the principle of neutralizing the operation of the inspired oxygen that Dr. Beddoes recommended inhalation of carburetted hydrogen in this disease; and there is no doubt that persons who had been weak and wasted, grew fat and strong under his treatment. Naphtha, creosote, and the non-nitrogenized articles of food, such as starch, gum, and sugar, will act in a similar manner. *So that, you perceive, there are many ways by which we can oppose this first and predisposing lesion of phthisical disease.*" (p. 100.)

Blessed man, says the student to himself; thrice blessed therapist! never shall emaciation (or, as my venerated master hath it, "diminished force of growth or reparation") exhibit its withered form in *my* path! with starch, gum, and sugar (no bad things, by the way), a dust of this, and a shake of that, along with a little creosote, I shall drive emaciation before me,—and, please heaven, make my fortune. Now, it is plain that such is the interpretation any reflective student must put on Dr. Evans's doctrine; for a *student* can hardly be expected to see through the loophole of escape which Dr. Evans has so very neatly provided for himself by the simple use of the word "*oppose*:" we may "*oppose*" emaciation, says the innocent teacher, with the remedies I enumerate. No: had his young Irish auditors caught fully the sense of this little word, they would assuredly have reminded the learned man of the various attempts that have been made from time to time to keep out (or "*oppose*") the tide with a pitchfork, together with the provoking result, that the tide has hitherto invariably beaten the pitchfork in the long run. After this precious specimen of therapeutics, it seems sufficiently flat to turn to a point of pathology. Dr. Evans calls emaciation "*this first and predisposing lesion of phthisical disease*:" but what says clinical observation in the hands of Louis? Why, that in *one half* only of the patients observed by him\* did loss of flesh appear among the first symptoms,—in *none* is it spoken of as their harbinger.

But Dr. Evans hasn't half done "*opposing*" yet: hear him in the second lecture again :

"After this rapid review of the lesions which constitute pulmonary consumption, and the means which we possess of baffling them, I think you will already be inclined to agree with me, that there is nothing necessarily fatal or incurable in this disease; *but that, on the contrary, it is just as tractable as other chronic diseases.*" (p. 102.)

Why, this is next door to Professor Halloway's pill and the wafer advertisements of the sham Dr. Locock: the wafers ensure perfect cure of consumption in ten minutes, we believe; so that Dr. Evans may be considered so far to be outdone. We are very sorry for him: but from

\* Op. et ed. citat. p. 369.

many strange things we see in this strange book, we suppose the defeat is not quite irreparable.

It will probably be expected that we should say something of St. John Long's "treatment" and "nostrum." Dr. Evans tells us concerning the first,—“the principal, or, indeed, to speak more correctly, the sole element in St. John's treatment was counter-irritation.” As respects the second (that is, the nostrum), it appears there was none; for, though the turpentine and strong acetic acid liniment was the composition this man-slayer chiefly employed, “we sometimes,” says Dr. Evans, “made an alteration in the liniments.” It is reported (and, as we believe, correctly) that an individual among the aristocracy, eminent among the eminent for the soundness of scientific judgment characteristic of that branch of the community, gave St. John Long £3000 for his secret! Poor woman, she got the vinegar and turpentine receipt; and may she be happy with it,—the very worst and most uncertain counter-irritant that still holds a place among the long list of such.

Dr. Evans indulges no less in singular words than in singular therapeutical ideas: where did he find the terms “lympthy,” “lungy,” “enterital, &c. &c.,” which occur *passim* in his pages? Where, too (and these examples are jotted down as being the first that catch our eye in turning over the leaves), did he become acquainted with his method of spelling the words “Aritæus” (p. 27); “Cyrrosis” (p. 12, twice); “gum-ammonia” (p. 105); “albuminaria” (p. 121), &c. &c.? But if English orthography fares badly with Dr. Evans, it has no right to complain,—for the French is ten times worse off. The book closes with a professed transcript from Andral's Clinique Médicale, which throughout contains on an average about two errors of spelling *per line*;—there is not a single accent from beginning to end; and we will venture to affirm that there is not a Frenchman from Calais to Toulon who could read the extract without real difficulty.

#### ART. VIII.

*Delle Alterazioni Patologiche delle Arterie per la legatura e la torsione Esperienze ed Osservazioni di LUIGI PORTA, Professore di Clinica Chirurgica nell' I. R. Università di Pavia. Con tredici Tavole in Rame.—Milano, 1845.*

*Experiments and Observations on the Pathological Alterations of Arteries by ligature and torsion. By LUIGI PORTA, Professor of Clinical Surgery in the Royal University of Pavia. With thirteen copper-plates.—Milan, 1845. Folio, pp. 439.*

BELIEVING that the alterations induced in the arterial system by the ligature had never been scientifically demonstrated, Dr. Porta took advantage of his situation in the University of Pavia to apply himself to the task, by experimenting on animals, and by gathering from his own practice and that of others, facts in human pathology illustrating the question. He commenced his experiments in 1835, and has continued them during nine

years, making upwards of 600 upon 270 animals. Most of them have been made upon dogs, as these animals are more easily obtained, and offer a closer analogy in physical organization with man than others; but sheep, goats, horses, asses, oxen, and rabbits, have been also used. The greater number were instituted in the Clinical School with the assistance of students, and preparations in the museum are further proof of authenticity. The object of the author has been to solve the following problems:

1. What becomes of a ligature abandoned upon an artery; does the material of which it is formed influence the results of the operation, and, if so, what material is the most suitable?

2. What alteration does the artery undergo in the different methods of ligature and torsion, and which of the two methods merits the preference?

3. What change in the circle of the arteries succeeds obliteration of the principal trunks, and, consequently, what is the power of the arterial system in opening the channels necessary for the preservation of the limbs?

These inquiries are preceded by certain anatomical considerations upon the arteries. It appears that the arteries of the larger animals are much more tenacious than those of man, and those of dogs are one degree, and of goats and sheep two degrees, more fragile and weak. This, of course, varies with the age of the animal. We pass over a long description of the various coats of arteries, as we find nothing here peculiar to the author, until he comes to treat of the vascularity of the internal coat, when he argues that what is generally considered to be stellated injection of the capillaries which permeate this tunic proceeds from injection of the vessels of the middle coat being seen through the transparent internal one.

“Examining the larger arteries of the chest and neck in horses, dead of acute inflammation after ligature of the carotids, I have seen vessels morbidly injected on the external surface appear on the internal surface of the inner coat. This is more easily observed in the principal arteries of large animals, on account of the greater caliber of their *vasa vasorum*. In all these cases, however, if the artery be split, and the last fine layer of the internal tunic be removed by fine forceps in the situation of the injection, this layer is found to be without vessels.” (p. 12.)

“Still vessels that do not exist, or that cannot be demonstrated in the normal texture, appear and may be shown after inflammation. I have already said that in dissecting large arteries, inflamed by the action of the ligature, we not unfrequently discern around them a network of small vessels, which pass from the cellular sheath, permeate the strata of the middle coat, and appear on the external surface of the inner coat. Then if this coat be thickened, or if the coagula and false membranes occupy the channel of the truncated artery, vessels are sometimes observed passing from the seat of the ligature, or traversing the walls of the divided vessel, and by a plastic power regenerating themselves in the bosom of the new formations. At times, also, within the clot of blood which plugs the artery, as I shall show hereafter, conspicuous vessels are prolonged which communicate with the external vessels. This phenomenon is much more rare and more difficult to demonstrate than is commonly supposed; but it offers a perfect analogy to what is observed in the nerves and mucous membranes, the ultimate layer of which never presents vessels in its texture, after injection of the finest coloured liquids;

while the products of inflammation formed on the surface, lymph, cellular tissue, or false membrane, present vessels in large quantity, which spring from beneath, and are easily seen and injected." (p. 13.)

I. After these preliminary observations, the author passes on to describe his experiments made to determine the effects of ligature, and the influence which a diversity in its material exercises upon the artery to which it is applied. In his experiments upon the material of ligatures he has wisely confined himself to such as are most applicable in practice—catgut, silk, prepared and crude, flax and hempen threads, and hair—and as the results have shown no palpable difference between crude and prepared silk, or flax and hemp, the species of ligature are reduced to four—three of animal matter, catgut, silk, and hair; and one vegetable, flax or hemp. These substances were applied as simple threads upon the principal arteries of animals, by different processes, and the ligature left for various periods from a few days to three years. Among the number of experiments we shall state the result of the more important as briefly as possible.

Several experiments with catgut upon the carotids and femoral arteries of dogs and sheep are first related. They are of considerable interest, but we find it would be impossible, without the plates, to give any correct idea of the changes produced; and the conclusions of the author being apparently justly founded, probably this is all that most of our readers would require. These conclusions are thus stated:

"Among the substances applicable to the ligature of arteries, catgut is doubtless the most strong and homogeneous. A series of eighty experiments proves that even of small diameter it has sufficient strength to compress an artery, divide its proper coats, and maintain them in mutual contact until obliteration or division is perfect; but it differs from the other ligatures, as it does not always divide the internal wall at the moment when the knot is formed, but becoming lax by a process of softening, it sometimes allows the artery to reopen. This, however, does not interfere with the success of the operation, when the relaxation is gradual and the internal plug is formed in time.

"Still, from the experiments instituted, and partly related above, it appears that catgut a day, or even some hours, after its application, excites adhesive inflammation in the cellular sheath of the artery, and becomes covered by a stratum of plastic lymph, olivary in form, which surrounds and buries it. This lymph at first becomes modelled into a soft, gelatinous, adherent tissue, which gradually becomes atrophied, and generally leaves a simple cellular web; or a ligamentous substance more close and opaque, designed to isolate the ligature and the artery; or more rarely suppurative inflammation is set up in the seat of ligature, and a circumscribed abscess is formed when the ligature remains, until it is thrown off. The catgut relaxes, softens, dissolves, and becomes confounded with the surrounding cellular tissue; or the artery upon which it rested being consumed, it becomes dry and stiff, and lays bare in the layers of the cellular tissue; or becomes concealed in the peculiar tissue of the cord remaining between the truncated extremities of the vessel; or confined in a cyst, or coating of the plastic lymph which at first formed around it. The new cyst is at first simple and cellular, but afterwards it acquires consistence, and offers in its walls an external layer, strong and coriaceous, and a species of epithelium, or internal rosy, soft, mucous layer, containing the ligature and some secreted matter. This cyst is generally small, resembling a grain of rice or millet, and when thus small it is inordinarily attached and closely adherent to the ligature, following all its irregularities, thus partly assisting in the process of solution. But sometimes, by exorbitant exhalation, it becomes en-

larged to the size of a pigeon's egg, or more, then resembling a true encysted tumour. In its last changes, if not discharged by the fistula remaining after the operation, it withers and remains undisturbed among the tissues where it lies, or terminates by being with its cyst completely absorbed." (pp. 21, 22.)

The author took the necessary precautions to prevent the escape of the ligature, so that the fact of absorption was in many instances demonstrated. The plates contain several illustrations of each of these various effects of the catgut ligature. In thirty-three of eighty cases the catgut had completely disappeared, and in several others it was found so much softened that little doubt could be entertained of its progressive absorption.

More experiments were made with silk than with any other material, both in the manufactured and crude state, untwisting the threads of commerce and applying them not thicker than a human hair. The only difference in the results was, that manufactured silk left on an artery lost its artificial colour, while the unprepared maintained the white or yellowish colour natural to it.

When compared with catgut, silk ligatures produced much more irritation, and excited suppuration more frequently, although in many cases simple adhesive inflammation alone was produced. Like the catgut, they are first covered with plastic lymph, then connected with the cord remaining between the two extremities of the artery, or they lie bare between the layers of the common cellular tissue, or become encysted. If not thrown off, the silk may remain inert during life; yet in some cases the author says he has succeeded in demonstrating that after a long time it becomes softened, and may be absorbed. In one hundred and twenty experiments the ligature was discovered in one hundred and one cases, in nineteen (a sixth) it disappeared; in some of them it may have escaped observation, but as in most of those discovered the silk had undergone a gradual change, and great care was taken in the examination, the possibility of the absorption of silk cannot be denied.

No difference was found between slender ligatures of linen or hempen thread. They were not at all more irritating than silk. Of sixty cases, in three or four only did an abscess form, in all the others adhesion followed; and when they were applied merely as a loose knot, frequently, after the course of some months, they were found lying innocuous upon the uninjured and pervious artery. After an interval of two years, like catgut and silk, they are found either in a state of perfect integrity, or macerated, or completely removed. Fifty were found, ten had disappeared, and most of those found were in the stage of metamorphosis.

Forty experiments were made with black horse-hair, single and double, and the observations have been prolonged to three years. The only difference between the hair ligatures and the others is, that they preserve their physical properties unaltered, and still remain in the body innocuous. Of the forty ligatures thirty-eight were found. Of the two which disappeared, one was applied upon a sheep, and could not be found after seven months; whilst others of silk and thread, upon other arteries of the same animal, were found. The other was upon the carotid of a dog. It is much more probable that the ligatures escape unobserved than that they are absorbed, and thus regarded, they tend to confirm the correctness of



the former observations. Human hair has been tied upon the arteries of rabbits, and, like horse-hair, has always been found unaltered.

During fifteen years the author has applied ligatures upon the principal arteries after amputation, wounds, or in cases of aneurism, and disease, in upwards of one hundred cases, and the comparative results in men and animals, with regard to the alterations in the ligature and its effects, show that suppuration is much more common in the human subject; that the ligatures, if left, almost always escape, sooner or later, either from the original wound or a secondary opening; but that in some cases of amputation, followed by union by first intention, when both ends of the ligature had been cut close off, they had remained in the limb and never appeared. Hennen, Veitch, and Lawrence have observed the same fact. Certain cases recorded by Porta and other authors tend to prove that when the ligature remains in the human body it undergoes the same series of changes as in the lower animals. The chapter is concluded by a table, which we give entire, as the results are practical and important:

LIGATURES APPLIED, 300.

1. Quality.					Brachial					14		
					Iliac					19		
Catgut	.	.	.	80	Femoral					80		
Silk	.	.	.	190								
Thread	.	.	.	60								
Hair	.	.	.	40								
2. Methods.					3. Time left.							
					1 to 30 days					44		
					1 to 3 months					61		
					3 to 6 months					48		
					6 to 9 months					54		
					9 to 12 months					46		
					1 to 2 years					29		
					2 to 3 years					18		
Circular ligature					.	.	.	.	.	140		
Loose ditto					.	.	.	.	.	70		
Double ditto*					.	.	.	.	.	45		
3. Animals—several experiments made on same animal.					6. Result.							
Dogs					.	.	.	.	.	65		
Sheep					.	.	.	.	.	24		
Goats					.	.	.	.	.	12		
Calves					.	.	.	.	.	3		
Horses					.	.	.	.	.	13		
Asses					.	.	.	.	.	5		
Rabbits					.	.	.	.	.	8		
4. Arteries (on 45 the double ligature was applied).					Catgut 80					disappeared	33	
										remained	47	
					Silk 190					disappeared	19	
										remained	101	
					Thread 60					disappeared	10	
										remained	50	
					Hair 40					disappeared	9	
										remained	38	
Carotids					.	.	.	.	.	110		
Subclavians					.	.	.	.	.	33		
										64	236	

This table is alone sufficient to show the care and industry of Dr. Porta ; but we must pass without further comment to the succeeding chapter of his work, on the methods of ligature and torsion, and of the pathological conditions induced by them in arteries.

II. There are four methods, of employing the ligature in the continuity of the trunk of an artery : 1, tying it round and leaving the ligature ; 2, removing the ligature immediately after applying it ; 3, interposing a foreign body beneath the ligature ; 4, applying the ligatures and dividing the artery between them.

The common method of circular ligature is first treated, and a number of experiments are related, which were instituted for the purpose of determining the consecutive changes at different periods. Without the plates we could not convey any correct or clear idea of these changes in individual cases, and we must therefore be content, as before, with an analysis of the conclusions of the author, upon the result of one hundred and forty experiments :

“The circular ligature applied upon an artery determines the following mechanical alterations: division of the proper coats, strangulation of the cellular coat, and their approximation internally at each side of the ligature with the formation of a clot. In this condition the artery, in consequence of the operation, is subject to two kinds of inflammation, the adhesive and the suppurative. The experiments just related demonstrate the first kind, or obliteration by adhesion.

“The process of coalition of the tied artery has four periods: the first, external arteritis and organization of the *oliva cellulare* ; [this implies a quantity of coagulable lymph exuded, covering the ligature, and taking the shape of an olive ;] the second, closure of the vessel ; the third, isolation of the closed extremities, and detachment of the ligature from the intermediate cord ; the fourth, assimilation of all these parts. . . . .

“The rapidity of the organization of lymph is extraordinary, beyond belief. At the close of the second to the third day the *oliva* is changed into a gelatinous tissue ; and generally in less than a week the new cellular sheath is perfectly formed, and presents a most beautiful and beneficial provision of nature for the obliteration of the artery, and protection against the blood which threatens its future destruction.” (pp. 53, 54.)

We pass over a minute account, more curious than important, of the other stages of the process, but extract a passage on the formation of the internal plug, or clot :

“The *trombo*, or internal coagulum, is among the first phenomena of the ligature of an artery, and arises, as it is known, from the stoppage of the blood between the ligature and the first lateral vessels. This purely physical phenomenon is general in tied arteries, but not so constant as to be observed in all cases. Of one hundred and forty experiments with the circular ligature the clot was not formed in twenty-nine, sometimes on neither side, and more frequently only on the side next the heart, or *vice versa*, and this without neighbouring lateral vessels: several times the clot has not formed, and the artery has remained permanently empty, notwithstanding an interval of some *millimeters* from these vessels. When the clot forms it may take place in a few hours, or in the first day ; or the blood may not be completely coagulated on the third or fourth day. The larger a vessel is, the nearer the heart, subordinate to the action of this organ, and rich in lateral branches, the longer is the clot in forming, it is weaker, or is altogether wanting. It is also erroneous to suppose that the clot is always formed in proportion to the capacity of the tied trunk, and to the distance of the first lateral branches ; not

unfrequently the clot is excessive, and passes beyond small and large lateral emissaries one or two *millimeters* in diameter; while in other cases it remains short, or slender and thread-like, and only fills a part of the channel. When largest, it scarcely forms a plug, or completely fills the closed end of the artery, exactly uniting its walls.....Once formed the clot remains a long time unabsorbed, prolongs the process of assimilation, and it appears that it sometimes becomes permanent, traces being discovered several years after an operation; but when the artery goes through its last changes, the coagulum becomes white, hard, firm, almost calcareous, attenuated, forms part of the ligamentous walls of the vessel, and then entirely disappears." (pp. 56, 57.)

Travers and Jones believed that a division of the proper coats of the artery was necessary to the success of the operation, but our author states that it is not indispensable. In many of his experiments, particularly with catgut, he found that he had not divided these coats; yet the channel of the artery was closed, and the result the same as in case of division. The obliteration then occurs in two modes: either by coalition and simultaneous assimilation of all three coats at the spot tied; or by a slow and insensible contraction, which, after the obstruction of the clot, without destruction of the walls, renders the arterial tube impervious, and converts it in time into a ligamentous cord. But more frequently the ligature which does not at first mechanically divide the arterial tube, does so afterwards by the inflammatory process.

Besides obliteration by coalition, an artery may be closed also by suppuration. This is uncommon, however, in the lower animals, and is generally attributed to violence in the operation, motion, the introduction of foreign bodies, &c. Most commonly a little pus is formed in the centre of the lymph effused around the ligature, and this pus is reabsorbed. Complete suppuration without lymph is much more rare. When it occurs the tied ends of the artery lie in the middle of an abscess, separated from each other, with their coats soft and yellowish, and the opening of the canal closed only by the clot, which is the sole protection against hemorrhage for one or two weeks. As the suppurative process ceases, granulations spring up from the cellular coat and cellular tissue around the ends of the artery, and by their contraction the coats of the artery are brought together, and the opening is permanently closed. The artery, when thus cicatrized by the second intention, afterwards presents the same conditions as if it had united by primary coalition. A number of cases from the author's experience, and that of others, are given to show that the same changes occur in the human subject. But—

"The two kinds of inflammation above described have an inverse ratio of frequency in man and in brutes. It is very rare, and only from extraordinary circumstances that a complete suppuration with total separation of the ends of the tied artery occurs in brutes; and it is rare that a perfect coalition is effected in our arteries. And as this is the most simple, ready, and prompt mode of closing vessels, we see how much more fortunate animals are than man in this respect.

"Operating in whatever manner, we can never expect in surgery the fortunate results from the ligature of arteries that are obtained in the lower animals; but, judging from analogy and from known data, I believe that when ligature of arteries shall be performed in man with greater delicacy than has been hitherto employed, selecting the most homogeneous material and the most simple method, respecting the cellular matrix of the vessel, cutting off the ends of the ligature, reuniting the wound, avoiding proximity to the aneurism, and enjoining the most perfect quiet

to the patient, we shall obtain adhesion more frequently; and when, notwithstanding our care, suppuration comes on, it will be circumscribed, and not always prevent a plastic exudation which can close the mouths of the artery, and prevent those fearful accidents which, very rare in brutes, have so often put the life of man in danger." (p. 74.)

We now pass to a consideration of the other methods of tying arteries: 1, temporary ligature; 2, mediate ligature; and, 3, double ligature, with division of the vessel.

1. The method of *ablation* may be defined thus: the removal of a circular ligature immediately after its application upon an artery, and closure of the wound. In order to determine if this method was sufficient to excite inflammation of the coats of the artery, and thereby obliterate its canal, the author made a number of experiments, which he records. They were fifty in number, and made upon the principal arteries of various animals, dividing in every case the proper coats of the vessel with the ligature, and removing it at various periods from a minute to some hours. Division of the proper coats, and strangulation of the cellular, is produced, and the latter becomes inflamed for some lines in extent, and causes a lymphatic exudation, becoming restored to its natural state about the third or fourth week; very rarely the inflammation is greater, and the same coat is thickened, hardened, and contracted, and, remaining so, compresses the other coats, and diminishes the caliber of the vessel; suppuration is rarely produced.

Immediately after the removal of the ligature the tube is free to the current of blood, and is permanently pervious, uninfluenced by the division of the proper coats of the artery; while, on the other hand, these coats do not appear to be influenced in the changes they undergo by the sanguineous current incessantly passing along. It is only when the cellular coat is thickened, as before described, that the caliber of the vessel is sensibly diminished. Still, in some rare cases, a clot forms which obliterates the vessel, or arteritis follows, and external exudation of lymph produces the same result. Of the fifty cases, in six or seven a clot formed, and in five or six, lymph, or false membrane, which occupied totally or partially the canal of the artery.

Mr. Travers had preceded the author in these experiments. He left the ligature from two to twenty-four hours; and in eleven experiments upon the carotids of horses and asses he almost always obtained obstruction of the vessel by sanguineous or albuminous coagulum. His papers in the fourth and sixth volumes of the *Medico-Chirurgical Transactions* are well worth attentive perusal. Dr. Porta, in addition to the fifty trials above noticed, made twenty-four others, removing the ligatures at two, six, and twelve hours. In one or two cases obliteration followed, but in the majority it did not occur; the clot and coagulum were wanting, and the caliber of the vessel was not diminished. Travers, Copeland, Roberts, and Crampton have adopted this method in some cases of aneurism in the human subject. In one case a popliteal aneurism remained pervious after ligature for twenty-six hours, another for six hours; one of aneurism at the elbow was cured by ligature of the brachial removed after forty hours, and another after popliteal aneurism of the femoral removed after twenty-four hours.

Dr. Porta has also performed seventy experiments to determine the value of a loose or slack ligature. The kind of animal, the material of the ligature, and, above all, the degree of tightness of the ligature, have greatly influenced the results; but the sum of the whole may be said to be that either by clot, exudation of lymph, or slow contraction from thickening of the cellular coat, the artery was closed in half the cases; in the other half it remained pervious, without any, or with only very slight, alteration in its texture; while, in a few cases, a part of the walls became ulcerated or eroded, and gave rise to fatal hemorrhage. These experiments upon the loose ligature illustrate the effects of the ligature of reserve, which was at one time the fashion, as a supposed safeguard.

2. The *mediate ligature* has for its object the closure of the artery without injury to its coats. It is termed also flat ligature, ligature with a small cylinder, Scarpa's ligature. For the ligature a small riband is generally used, formed of several simple waxed threads, and as the intermediate body a small cylinder, or small roll of adhesive plaster.

To determine the value of this method the author made one hundred and twenty experiments—thirty-five with the permanent, and eighty-five with the temporary ligature. He has also collected ninety-six cases of mediate ligature performed upon the human subject, that both in its permanent and temporary application the results in man and the lower animals are similar. The following table will show these results in the eighty-five experiments upon the temporary mediate ligature:

1. Animals.				4. Condition of the Artery.			
Dogs	.	.	44	Artery found uninjured	.	.	33
Sheep	.	.	12	a little eroded	.	.	21
Horses	.	.	15	eroded and cicatrized	.	.	9
Asses	.	.	5	divided into two portions	.	.	16
Mules	.	.	5	reduced to a cord	.	.	6
Oxen	.	.	4				
2. Arteries.				5. Condition of Canal.			
Carotids	.	.	40	Artery found pervious—			
Subclavian	.	.	20	Carotids	.	14	} 24
Brachial	.	.	11	Subclavian	.	6	
Femoral	.	.	14	Brachial	.	2	
				Femoral	.	2	
3. Period of Removal.				Artery closed by plug	.	.	34
1 day	.	.	9	by lymph	.	.	11
2 days	.	.	15	divided in two	.	.	16
3 days	.	.	43				
4 days	.	.	13				
5 days	.	.	5				

The exact result of the mediate permanent ligature is not given; but the author states that, instead of being more secure than the common circular thread, suppuration is much more common, the artery is deprived of its cellular coat, and the internal clot is the only safeguard against hemorrhage.

3. The method of double ligature, with division of the artery between, was produced in forty-five cases—thirty-five with simple incision, and ten with removal of a portion of the artery of various lengths. The *mechanical* results are the same as in the common method, with the addition of ecchymosis, retraction of the divided extremities, and impulsory movement of the cardiac extremity. The first *vital* phenomenon after the operation is inflammation of the cellular coats, with copious exudation of lymph,

which afterwards becomes modelled into a cellular cylinder, and restores the continuity of the vessel. This cylinder varies from a few lines to two inches in length, according to the degree of retraction after division of the artery. At first it is much more voluminous than the vessel, but afterwards it becomes smaller, firmer, and fibrous, and is reduced to the state of a thin ligamentous coat; or it is completely absorbed, leaving the ends of the artery permanently separated; or very rarely (it only occurred in two experiments) the closed extremities of the artery are united by a new cellular tube, in the midst of which the two ligatures are found:

In ten experiments made upon the carotids of dogs, with removal of a portion of artery between the two ligatures, the cylinder formed, as in simple division when the portion removed was very small, but when it exceeded two centimeters the cylinder was not found, and the extremities cicatrized separately without union between them.

In the forty-five experiments with the double ligature, hemorrhage and corrosion of the cardiac extremity occurred in three cases, which was never observed after the common ligature. A table of the results of six hundred ligatures in the human subject, which we shall presently relate, will be the best test of the practical value of the method. The conclusions of the author are as follows:

“The temporary and loose ligatures are simple methods, destined, from their inefficiency, to remain neglected. The permanent mediate ligature, formerly recommended from prejudice, should meet the lot which the same method in wounded arteries for a century has suffered. The temporary ligature, although at first sight tempting, after the above demonstrations, and the facts drawn from other authors, may be declared to be a difficult, complicated, and uncertain method. To this the double ligature adds violence, and a multiplicity of proceedings, frequent impossibility of execution, copious suppuration, and greater tendency to hemorrhage. Thus the circular ligature alone remains, to which good sense and practice have already accorded the preference, now approved and justified by science,” (pp. 108-9.)

The tabular view of six hundred ligatures of the large arteries in man has been prepared with so much care, and affords so much information, that we give it entire:

1. Operators.				4. Arteries.			
Anglo-American	.	.	275	Aorta	.	.	4
Italian	.	.	131	Innominate	.	.	8
French	.	.	106	Carotids	.	.	132
German	.	.	64	Subclavian	.	.	74
Russian	.	.	22	Brachials	.	.	68
				Common iliac	.	.	11
				Internal iliac	.	.	12
				External iliac	.	.	96
				Common femoral	.	.	16
				External femoral	.	.	180
2. Sex.				5. Number of Arteries tied in the same Individual.			
Male—				Two common carotids, right and left	.	.	5
Adults	545	}	551	Right carotid and subclavian (method of	.	.	
Children	6	}		Brasdor)	.	.	4
Female—				Right and left external iliacs	.	.	1
Adults	48	}	49	Right and left external femoral	.	.	7
Child	1	}		Right external iliac and left external fe-	.	.	
				moral	.	.	1
				Two common carotids, uncertain cases of	.	.	
				Bünger and Cross	.	.	(2)
				Two external femorals, uncertain cases of	.	.	
				Hodgson	.	.	(2)
3. Indications.							
Aneurisms	.	.	418				
Aneurismal varices	.	.	30				
Nævi	.	.	34				
Various tumours	.	.	19				
Wounds	.	.	84				
Ulcers	.	.	8				
Hemorrhage	.	.	4				
Neuralgia	.	.	1				



6. *Methods.*

Hunter's method (more than one ligature)	93
Brasdor's	31
Double ligature	67
Mediate ligature	96
Circular ligature	356
Unknown	27

7. *Progress.*

Regular, in cases	381
Anomalous, in	219

8. *Accidents.*A. *Local Accidents.*

Hemorrhage from the tied vessel	75
Hemorrhage, suppuration, and gangrene from the tumour or wound	40
Return of the aneurism	25
Reproduction of aneurism in other parts	11
Gangrene of the limb	49
Palsy and atrophy of the limb	14
Phlegmon and local abscess	13

B. *General Accidents.*

Gastrico-bilious and typhoid fevers	10
Inflammation of—	
Head	18
Chest	22
Abdomen	6
Neuroses—	
Apoplexy	2
Tetanus	3
Convulsions	1
Syncope	1
Hemiplegia	4
Neuralgia	7
Cachexia—	
Hydrothorax	1
Tabes purulenta	6

9. *Result.*

Cures	433
Deaths	167
General mortality, 27 and a fraction per cent.	

A. *Mortality in Sex.*

In males	551
Deaths	153
In females	49
Deaths	14

B. *Mortality in indication.*

In aneurisms	418
Deaths, 117.	
Aneurismal varices	30
Deaths, 5.	
Nevi	34
Deaths, 3.	
Various tumours	29
Deaths, 8.	

Wounds	Deaths, 28.	64
Ulcers	Deaths, 3.	6
Hemorrhage	Deaths, 3.	4
Neuralgia	Deaths, 0.	1

C. *Mortality from Artery.*

Aorta, cases	Deaths, 4.	4
Innominate	Deaths, 8.	8
Carotids	Deaths, 35.	132
Subclavian	Deaths, 27.	74
Brachial	Deaths, 10.	68
Common iliac	Deaths, 5.	11
Internal iliac	Deaths, 4.	12
External iliac	Deaths, 22.	96
Common femoral	Deaths, 8.	16
External femoral	Deaths, 45.	180

D. *Mortality in different Methods.*

Hunter's cases	Deaths, 10.	23
Brasdor's	Deaths, 19.	31
Double ligature	Deaths, 11.	67
Mediate ligature	Deaths, 23.	96
Circular ligature	Deaths, 102.	356

10. *Cause of Death.*

Hemorrhage from the tied artery, in cases	30
Hemorrhage, suppuration, and gangrene from the tumour or wound	17
Bursting of internal aneurisms	8
Severe inflammation of the limb operated upon*	30
Progress of the disease indicating the ligature, tumours, wounds, ulcers, hemorrhage	12
Adynamia and vital exhaustion from severity of the operation	13
Fevers of different character, not evidently local in their origin	6
Internal inflammations of—	
Head	12
Chest	20
Abdomen	6
Neuroses—	
Apoplexy	2
Tetanus	3
Convulsions	1
Syncope	1
Cachexia—	
Hydrothorax	1
Tabes purulenta, from suppuration of the wound made in the operation	6

We now pass to the article upon what the author calls the accidents of the ligature, meaning the unusual circumstances which may arise and

\* These thirty patients died from the direct effects of gangrene, and not from the amputation required by this occurrence.

disturb the course of the operation, or of the subsequent cure. The most important of these are arteritis and hemorrhage.

1. *Arteritis*, as ordinarily excited by the ligature, is moderate and defined in its extent; but it may be violent and diffuse, and cause great danger. In this case the three coats are found intimately united, as it were fused together; the cellular coat is thick and pulpy, rich in small vessels, and infiltrated with lymph or serosity. The proper coats are rarely swollen, but they become soft, tender, fragile, with very slight cohesion, and so easily torn that their separation is almost impossible. They maintain their colour, or sometimes put on a rosy tint, independent of vessels, which are only found in the false membrane on their surface. The internal surface of the vessel does not always preserve its natural polish, but becomes unequal and scabrous or undulated and flaky, from evident hypertrophy of the membrane and its abnormal productions. It is in these cases that blood-vessels have been discovered in it. The artery may retain its natural caliber, or become contracted, or may become dilated or enlarged in consequence of the simultaneous action of three causes—softening, hypertrophy of the walls, and impetus of the blood.

This arteritis may terminate in adhesion, more rarely in suppuration, and ulceration to varying extent. In the spots where the inflammation has been most intense, ulcers, on the walls, are seen of various size and shape, and a third or half of the tube may be implicated, and often departing from the point of ligature the ulceration destroys the artery to the extent of two, three, or four inches.

The best means of avoiding this accident is care in the operation, and quiet after it.

2. *Hemorrhage* after ligature arises from three principal sources—secondary opening in the tied vessel, an injured collateral branch, and from the aneurism, or wound, which has led to the operation.

In animals the danger from hemorrhage is trifling. In upwards of four hundred ligatures, followed by obliteration of the larger arteries, only three cases of secondary hemorrhage occurred, and five others in cases where the mediate ligature had been employed, with them being only two per cent.; while in man the table of six hundred cases shows that the proportion exceeds twelve per cent.

The condition of the artery is almost always the same—suppuration of the cellular sheath, perforation or division from corrosion of the proper coats, and partial or total deficiency of the internal clot.

The following table presents some important information. Of the six hundred cases, hemorrhage occurred in more than one hundred; but in upwards of forty the blood appeared from the wound, or aneurism, that had called for their operation, and the cases in which the artery gave way at the seat of the ligature amounted to seventy-five, or one-eighth of the whole.

1. Frequency.		Innominate	
Ligatures	000	Carotid	8
Hemorrhage in Cases, 75.		Subclavian	132
2. Arteries.		Brachial	73
Aorta	4		68
Cases, 0.			

Common iliac	Cases, 3.	11	16 days	.	.	.	.	.	3
Internal iliac	Cases, 2.	12	17 days	.	.	.	.	.	1
External iliac	Cases, 6.	96	18 days	.	.	.	.	.	2
Common femoral	Cases, 9.	16	20 days	.	.	.	.	.	2
External femoral	Cases, 27.	180	21 days	.	.	.	.	.	1
			22 days	.	.	.	.	.	2
			24 days	.	.	.	.	.	3
			26 days	.	.	.	.	.	3
			28 days	.	.	.	.	.	1
			30 days	.	.	.	.	.	1
			32 days	.	.	.	.	.	1
			33 days	.	.	.	.	.	1
			37 days	.	.	.	.	.	2
			42 days	.	.	.	.	.	1
Hunter's	Cases, 5.	23	45 days	.	.	.	.	.	2
Brasdor's	Cases, 7.	31	55 days	.	.	.	.	.	1
Double ligature	Cases, 9.	67	5 months	.	.	.	.	.	1
Mediate ligature	Cases, 15.	96	Time unknown	.	.	.	.	.	3
Circular ligature	Cases, 39.	356							
3. Methods.			5. Result.						
4. Period.			Cases	.	.	.	.	.	75
1 day	.	1	Cured	.	.	.	.	.	31
2 days	.	2	Death from hemorrhage	.	.	.	.	.	30
3 days	.	3	Death from other causes	.	.	.	.	.	14
4 days	.	1	Mortality of those in whom hemorrhage occurred, 55 per cent.						
5 days	.	4	Mortality strictly caused by hemorrhage, 37½ per cent.						
6 days	.	6	6. Means of Cure.						
7 days	.	1	Cures	.	.	.	.	.	31
8 days	.	6	With plug	.	.	.	.	.	13
9 days	.	2	With torsion	.	.	.	.	.	4
10 days	.	5	By second and third ligature of the artery	.	.	.	.	.	9
11 days	.	1	By bandage and cold fomentation	.	.	.	.	.	3
12 days	.	2	By amputation of the limb	.	.	.	.	.	2
14 days	.	3							
15 days	.	5							

III. The second section of the work is on the torsion of arteries. This is no modern invention; it was known to Galen and the Arabians, and was then practised almost as is done at the present day upon the smaller arteries, with a small hook or forceps. The instrument used by the author is a common pair of dissecting forceps, rather strong, with conical extremities, obtuse at the apex, the internal surfaces scored, and exactly joining along their whole length. On the large animals he employed large polypus forceps. No other instrument is necessary, and this is a principal recommendation of the method; as, if complicated instruments or proceedings were required, the operation not being an indispensable one, it could not be proposed in place of the simple, well known, and safe method of ligature.

The method is only applicable to wounded or divided arteries; and either simple torsion, or torsion after isolation of the vessel, may be employed. Simple torsion is adapted to small arteries of the fourth and fifth order; they are seized with small forceps, the instrument closed and turned in the fingers three, four, or six times, always in the same direction, and then the vessel is abandoned. In this way, six, eight, or ten arteries on the surface of a large wound may be stopped in a very few moments. It may be necessary to repeat the torsion two or three times. The author frequently adopts this plan after amputations, upon the radial, ulnar, tibial and interosseal arteries. Previous isolation, and from seven to ten turns of the

forceps, are necessary for the larger arteries. The errors to avoid in the operation are the seizing one wall of the vessel only, or not taking a sufficiently firm hold, seizing part of the surrounding textures with the artery, and twisting too little, so that the proper coats are not ruptured, or too much so that the cellular coat is also broken.

The author made sixty experiments on animals, and many observations in man, to determine the anatomical effects of torsion, and the subsequent alterations in the artery. They are closely analogous to those produced by the ligature, viz., circular laceration, and sometimes introflexion of the proper coats, strangulation of the cellular coat, and an internal clot, as mechanical effects; and adhesion or suppurative inflammation, as vital.

Several interesting cases, taken from the author's clinical practice, are given to illustrate the practical value of this method in the human subject. In the first, it was successfully employed in the brachial after amputation. No hemorrhage ensued, but the man died in three days from pneumonia; vessel closed by clot and adhesion. Same results in the second case, in a man who died four days after torsion of the tibial; and in the third, who died nine days after torsion of the brachial. In the fourth case, torsion was applied to the femoral after amputation of the thigh; the patient died of pulmonary abscess seventeen days afterwards; no hemorrhage occurred, and the artery was found perfectly closed. The same result occurred in a case in which torsion was applied to the popliteal after amputation; the patient died in ten days, also, from pulmonary abscess; and the popliteal was also closed in the sixth case, the patient dying of apoplexy fourteen days after the operation. In the seventh case, torsion was applied to the femoral after amputation; the patient died in twenty-one days of phthisis, and the artery was found exactly in the same condition as if a ligature had been applied. The eighth and ninth cases are instances of the successful application of torsion to the arteries of the leg and fore-arm after amputation. In the tenth case torsion was applied to the femoral; but as bleeding did not cease, a ligature was applied. A ligature was also necessary in the following case, in which torsion was used and repeated upon the femoral unsuccessfully. In the twelfth case the leg of a boy was amputated below the knee, the popliteal was divided above its bifurcation, and twisted six times upon itself. When the compression was removed, the artery was seen to pulsate violently, turn twice round, and throw a jet of blood from its centre. It was again twisted three times, and the bleeding ceased. The wound cicatrized without any other accident. The other cases are three of successful application and termination of the femoral, and two to the popliteal after amputations. A case is also given in which a large varicocele had produced neuralgia and atrophy of the testis, and a cluster of the veins was removed of the length of two centimeters. Hemorrhage to a large amount followed from the mouths of five or six large veins in the cord. Each of them was seized and twisted five or six times, and a common bandage applied. No hemorrhage followed, and the cure was complete in a month.

The concluding paragraphs of this section are important, and we accordingly extract them.

"Since I introduced torsion into this school, I have almost exclusively practised

it for the obliteration of wounded arteries. From the beginning I kept an exact register to the number of four hundred; then I neglected to record the daily cases in hospital and private practice, in which torsion was applied, except in cases of amputation. It was applied to the occipitals, temporals, maxillary, superior thyroids, some branches of the subclavian, the thoracic, external pudic, spermatic, &c., and to the digitals, so that there is no artery of the fourth or fifth order upon the surface of the human body that has not been repeatedly twisted, after accidental wounds or cutting operations, and the result of so much practice during nine years has been fortunate beyond every expectation. I well remember in some cases to have been obliged to repeat the twisting two or three times; but I do not remember that the ligature was ever necessary on account of torsion having failed. Thus I have ceased to employ the ligature for wounded arteries of moderate size, and now employ torsion only, as to the advantages of simplicity and celerity it unites others, as it may be done without assistance, and does not leave foreign bodies in the wound.

“With regard to the large arteries of the extremities, I have already applied torsion in sixty-five cases after amputations and disarticulations, principally performed in the Clinique, and some in private practice. Although the operation in these cases has not succeeded so fully and securely as in the preceding, yet in the majority it has answered. For the arteries of the fore-arm and leg, radial, ulnar, tibial, interosseal, torsion, either simple or after separation of the end of the artery, has constantly succeeded, except in two cases; in one of which the anterior tibial, and in the other, posterior tibial, gave way, and the ligature was required. On the brachial it has always succeeded; and of twenty-three operations on the femoral and popliteal it has failed in four cases, two already mentioned, the twisted portion having given way, and ligature being necessary. I know that with some surgeons of my acquaintance laceration and hemorrhage have been more frequent, but that may depend upon error in method or management. Fricke has enjoyed very good fortune, having only one case of hemorrhage in thirteen torsions of the brachial, femoral, popliteal and tibial arteries. He, however, has no bounds to his enthusiasm, and prefers the new operation in all cases, and twists with success even ossified arteries, which are not susceptible of torsion. On the other hand, the cases of Delpech and Textor are inconclusive, and of no more weight against this operation than the first unfortunate trials of the Hunterian operation in France by Desault and Deschamps. (pp. 161-2.)

The author goes on to argue that ill success is more frequently to be attributed to incapacity in the operator than to the condition of the vessel. If not twisted enough, the current of blood will reopen it; if too much, the coats give way. He continues:

“The results of practical surgery correspond with, and are even more favorable than, the results of zootomy. In my series of experiments upon the carotids, subclavians, and femorals of dogs, sheep, goats, and horses, torsion in general succeeded; but in eleven cases out of sixty secondary hemorrhage occurred, requiring either a repetition of the torsion, or the ligature, and destroyed more than one animal. Experiments on brutes, then, do not give a secure criterion to argue the same results in man; because here torsion of the mouths of vessels upon the surface of the wound is a ready and simple proceeding, much more likely to be successful than torsion after exposing, isolating, and dividing the large arteries in animals.” (p. 163.)

The conclusions are, that in wounds of arteries only, can torsion take the place of the ligature; and as it is perfectly safe and secure when practised on the smaller arteries, it ought to be performed when those arteries are divided. The question with regard to the larger arteries is not yet settled; but it would appear that although torsion answers in the majority

of cases, still the ligature affords greater security against secondary hemorrhage.

IV. Having examined the changes of the ligature, and its effects upon the tied artery, the author commenced his investigations on the collateral circulation established after the obliteration of the principal arterial trunks. The collateral circulation, or anastomosis, he treats as direct and indirect; the first carried on by some vessels running directly between the extremities of the obstructed trunk; the second by muscular and subcutaneous branches. A number of very interesting plates exhibit the direct anastomosis at various periods, from a week to two years and a half. From them, and the narrative of the experiments and dissection, we learn that a direct communication between the extremities of a tied artery is carried on by means of branches arising from these extremities, and these branches are of two kinds, primitive and new. The first merely become enlarged; the second are generated in consequence of the application of the ligature.

“If an artery which has been operated upon be incorporated with another healthy one, in the first the roots of the *vasa vasorum* are found more capacious, numerous, and crowded than in the second. Clusters of vessels arise from the large lateral branches; and from the large clusters others smaller and smaller arise, which is not the case in healthy spots. The minute branches spring in preference from the small vessels, on account of the tenuity of their walls, the easy permeability of their pores, and the pressure of the blood that deviates from the tied trunk. Then some small vessel runs from the proper coat of the trunk, and runs isolated, or ramifies on the surface; this may be natural. But in some parts this vessel suddenly, at its origin, forms a shoot of small twigs, or presents a species of bud, or small bulb, from which small vessels spring in considerable number; this is doubtless preternatural. The effused lymph which forms a covering to the ligature is soon found very rich in vessels; and this abundance in a tissue newly formed around an artery could only be explained by new propagations from the turgid and dilated *vasa vasorum* of the cellular coat. After some time, upon the very apex of the closed extremity of the trunk, direct anastomotic arches are discovered; and to these anastomotic arches, in many cases, we cannot deny priority to the ligature, and their appearance in this place by simple enlargement. Those vessels that opened from the coats at some distance, by absorption of the obliterated extremities of the trunk, afterwards appear to run from the apex, being prolonged in a ratio with the retraction of the extremity. But the apex of this extremity is sometimes covered by tender buds: the closed end is cribriform and reopened by new mouths; and the vessels that these open into traverse from one extremity of the artery to the other, and are seen passing through the ring of the ligature that has strangulated and divided it. Lastly, within the cavity of the operated artery vessels are generated, to which the sanguineous plug serves as matrix, and these communicating with the external branches, enter into the system of direct anastomosis between the two extremities. I have not as yet discovered internal vessels, except in the carotids of dogs, some weeks or months after the ligature, because I have limited my observations to these arteries only; but I believe that the same thing may be verified in others. The phenomenon is more likely to occur in the carotids and external iliacs, because these arteries not dividing for a considerable distance evolve long coagula. However, I have seen new vessels formed in coagula only a few lines in length, which scarcely covered the blind extremity; and of the two ends of the carotid the inferior has much more frequently presented examples of internal vessels than the superior. At first I believed that the body become vascular in the cavity of the artery was always lymphatic or cellular; but reiterated observations have convinced me that it is generally the fibrinous



clot, formed from the blood within the obliterated vessels, which is rendered vascular. Struck with the novelty of the fact, of which I had no previous idea, and not well assisted by injection, which succeeded imperfectly, I have maintained that the internal vessels advanced from the point, or were an appendix of the external anastomoses that arise from the arterial extremity at the base of the plug. [Instances are shown in plates.] By ulterior inquiries I have become certain that the internal vessels arise primitively within the coagulum or clot, and afterwards run to the apex of the closed artery, and become continuous with the external branches." (pp. 181-2.)

This direct anastomosis is much more easily demonstrated in the ruminating animals than in the carnivora, as in the former the collateral branches are less numerous, and therefore, there are greater obstacles to a free re-establishment of the circulation. The human subject, from the number of lateral branches, resembles the carnivora; but still cases and plates are given, proving the occasional occurrence of direct communication.

Maunoir, Parry, Mayer, and Müller have preceded the author in this demonstration, but he deserves the credit of establishing the authenticity of the fact by the number of his observations, and of having made more exact preliminary studies of the normal state of the vessels, and of the pre-existing anastomoses; and by analysis of the whole process proved the new formation of vascular arches between the obliterated extremities of a tied artery. Many of the plates given in illustration are very beautiful.

The *indirect lateral* circulation is carried on by the anastomosis of secondary vessels pre-existing around the central obliterated trunk. These are deep or muscular, and superficial or subcutaneous. The author wished to determine exactly by what vessels the circulation was carried on in each region, and the changes they undergo. He demonstrates first the effect of the ligature of the femoral in different animals, as the anastomoses about the hip are so well defined, and the opposite uninjured limb offers so decided a contrast to the one operated upon; and then treats of the other principal arteries and their anastomoses. This division of the work occupies upwards of two hundred pages; but fortunately for us, as it consists in a great measure of details of the dissection of different animals, at various periods after ligature of the principal arteries, we need not follow the author very closely, as all that we wish is to put our readers in possession of the result of his labours in general terms. We shall not even descend to particulars, except with regard to the aorta, even in the human subject, as this part of the subject has been so well worked in this country, and the results are known to every surgical student.

The changes which the principal lateral branches undergo after ligature of their parent trunk consist in enlargement. It is clear that all the blood passed by the trunk is very soon, after the ligature, carried through the lateral branches. If a sensible quantity of blood were detained, diminished vitality of the limb would afford evidence of such detention. Now, such diminished vitality Dr. Porta has never observed in any of his very numerous experiments on animals. Yet, in opposition to this argument, the most careful examination of the dead body, during more than a week after the operation, does not show the least dilatation of the lateral branches; and when the dilatation does commence, it proceeds very slowly,

and does not equal the caliber of the tied trunk for some months. This apparent contradiction is explained by the natural elasticity of arteries, allowing an instantaneous dilatation during life, which cannot be detected after death. That such a vital dilatation does take place is proved by the following experiments :

"1. When the two carotids are laid bare in a living animal and one is tied, the other is very soon seen to become dilated and turgid, and to pulsate more strongly than before. In the same way the superior branches of the suprascapular and mammary, when the subclavian is closed below by forceps or ligature, at once become dilated, and are seen to be enlarged, while after death no difference can be detected.

"2. When in dogs, sheep, or goats, a day or two after the ligature of the subclavian or superficial femoral, the superior branches of the operated trunk are laid bare, they are observed turgid and capacious, but this disappears, and cannot be shown after death.

"3. When in any of these animals, or in the horse, the two carotids are tied in the middle of the neck, and one is opened above the ligature, at first nothing occurs; but after fifteen or twenty seconds a jet of blood commences, which rapidly increases, and continues as if it were from the cardiac extremity. It is true that this experiment is not sufficient proof of the sudden enlargement of the lateral anastomosis, because the communications of the carotids within and around the cranium with the vertebrales are naturally large, short, and direct; but in the subclavian, external iliac, and superficial femoral, where the natural anastomoses are much more slender, the same experiment affords the same result, and after ligature the blood, from an incision of the artery below it, jets forth in quantity until the animal faints and dies.

"4. When the ventral aorta is tied in an animal, and the femorals are opened, hemorrhage does not take place at first, but after a few moments the blood issues from the wounded arteries in the same manner in which it comes from open veins; doubtless on account of the sudden dilatation which it has produced in the lateral branches, although these vessels, both in this and the preceding experiment, appear natural in the dead body." (pp. 217-18.)

"In animals with transparent arteries, as frogs, I have frequently seen under the microscope, when an arterial branch has been closed with forceps, that the adjacent branches suddenly enlarge, and the blood is urged on with greater velocity. From these facts we argue that in the first moments after the ligature of the principal arteries of the limbs, the blood is accelerated in the lateral vessels, and by this acceleration partly overcomes their temporary want of size." (p. 219.)

But it is difficult to suppose that an acceleration of the circulation and some mechanical distension of the lateral branches is sufficient to maintain the integrity of a part or organ, when the innominate, or the two carotids and a vertebral, or the common iliac are tied, or at any rate to supply the same quantity of blood as before the ligature. It is more reasonable to suppose that a limb, left motionless, may for a time be deprived, without injury, of a portion of the blood with which it was supplied in the healthy state in quantity sufficient for the performance of all its functions.

The more minute branches, or anastomotic arches, are shown to be enlarged from the first day, not only from the easier passage of injected matters, but also by the eye and the scalpel. The enlargement is thus first detected in the "median arches, then in the secondary ramifications, and, lastly, in the lateral trunks, which are inserted above and below the

obliterated extremities of the artery." This may be explained by the greater thickness and resisting power of the larger branches.

When the circulation is established sufficiently to supply the necessities of the part, the vessels remain without further alteration; and this occurs at a period varying from some months to a year after the ligature. In dissections made from twelve to thirty-six months afterwards, no further alteration was found in the anastomotic system.

We find some important observations in the section on the effects of ligature of the carotids. In upwards of one hundred cases of ligature of one or both carotids in dogs, sheep, and goats, disturbances of the nervo-muscular system, indicating a deficient supply of blood, were never observed. In thirty-two experiments upon horses, asses, mules, and calves, alarming phenomena occurred in five cases. In three horses the thread securing the second artery was scarcely tied, than extreme agitation, spasm of the jaws, great anxiety in the chest, tremors and convulsive motions of the limbs came on, and death soon followed. Two mules, whose carotids were tied at the same time, suffered from paralysis of the extremities, particularly the posterior; they were obliged to be raised and kept from the ground by cords, on account of actual defect of muscular power. Their heads fell on the manger, they refused food and drink, and died in eighteen and twenty-four hours, while no recognizable alteration or injury to the nerves could be detected after death. The other nineteen cases were without any ill consequence. In calves, the operation was as safe as in dogs or sheep; the animal walks about and grazes immediately afterwards.

With regard to ligature of the vertebrae, Dr. Porta is of opinion that the weakness of the extremities, dyspnoea, and nervous symptoms, ascribed by Sir Astley Cooper to a defective supply of blood to the brain, are merely the results of apathy and moral depression, which follow every severe operation, and not attributable to cerebral anæmia. Sir Astley fancied that the phrenic nerves, especially, suffered deprivation of blood. The author argues that the anastomoses are so free that no want of blood is possible from simple ligature of the vertebrae. He has never seen the instant death, as described by Cooper, after compression of the two vertebrae; but he has seen animals suffer greatly, and this was to be attributed to their delicacy, or the extreme difficulty and long duration of the operation; and he believes that the vertebral arteries have no greater influence than the carotids upon the functions of the brain.

Eleven experiments were performed to determine the collateral circulation of the innominata after the ligature of the roots of the carotids and subclavian at each side; but the operations were very disastrous, six deaths occurring from hemorrhage, thoracic inflammation, or violent convulsions. Of the five survivors, two had no bad symptoms; in the other three, orthopnoea, from spasms or convulsions of the diaphragm, gradually disappeared in the space of a few days.

The common iliac was tied in sixteen dogs. Half died from acute inflammation around the spot, or from peritonitis; one or two from adynamia; but the mortality was increased by the animals biting away the dressings, and laying the parts bare.

The section on ligature of the aorta is an interesting one. Experiments were made on sixty animals—twenty-seven dogs, nine sheep, five goats, and nineteen rabbits; fifty-three died; that is, all the rabbits, sheep, and goats, and twenty dogs. Seven dogs recovered, but two afterwards died from causes independent of the ligature. The deaths from the “circular” ligature took place from a few hours to three days; from the loose ligature, not for a month. The causes of death in nine cases were abdominal inflammation, the direct result of the operation; the rabbits and several dogs died from the depressing effects of the operation upon the general system; the other animals from paraplegia, or general paralysis from a defective supply of blood to the parts below the ligature. Some experiments have shown that when an animal suffers greatly from the ligature, if he does not sink under the abdominal disease, he may be saved by removing the ligature, and allowing the artery to reopen. One of them is worthy of extraction. After the ligature in a dog, six months old—

“The pulsation in the iliacs and femorals instantly ceased; the limbs became sensibly colder, and in less than ten hours incapable of motion or supporting the body; so that, notwithstanding the efforts of the animal, he was compelled, against his will, to lie down. Twenty-four hours of obliteration of the aorta and paraplegia, with evident sinking of the general powers, having passed on, I laid bare the wound, divided the ligature, and removed it. Immediately afterwards the arteries resumed their natural pulsation, and the animal, rapidly recovering, was able to walk with security upon the four limbs. There were no abdominal symptoms, but the wound of the operation supplicated, and was not cicatrised until the end of three months. The animal was then killed, and the aorta was found entire and pervious, without rupture or corrosion of its coats, and girdled externally by a stratum of adherent cellular tissue, and internally by a small clot of lymph adherent to its walls, near the orifice of the left external iliac.” (pp. 349-50.)

The mediate ligature was used in this experiment, a piece of cork being placed between the thread and the artery.

“Opening the femoral arteries suddenly after obliteration of the aorta demonstrates that the circulation below is maintained, and arrives through the small branches in the central trunk of the limb. Yet the hemorrhage from the wound in this trunk only expresses a fraction of the circulatory power, because, with the closure of the aorta, the femoral having ceased to be the centre of the circulatory system of the thigh, does not receive from the new centre of the small collateral anastomoses more than a fraction of blood, and a small impulse on account of its great caliber, and the remote and indirect action of the heart. It is certain that if the gastrotomy, indispensable in exposing the aorta, was not so frequently fatal, the ligature of this artery would more often succeed, the circulatory power of the lateral anastomoses being frequently sufficient to support life. But it is equally certain that in many animals this power is not sufficient to maintain life, and hence paraplegia, general paralysis, depression of the powers, and death.” (p. 355.)

In the anastomoses the mammary, intercostals, colic, mesenteric, and first lumbar, have not been found to take the part their anatomical disposition would lead one to anticipate; the principal anastomotic chain being always formed by the four pairs of lumbar and posterior sacral nearest to the obliterated spot.

Although the deaths have exceeded three-fourths of the number operated on, still the possibility of recovery in a certain proportion has been de-

monstrated. Dangerous and uncertain operations may be hazarded in desperate or fatal cases. New operations seldom succeed at first; the ill success of the few cases of ligature of the aorta in the human subject is, therefore, not a sufficient reason to reject the operation altogether; and, "perhaps in future, operating better in the left ileo-epigastric region, with simple displacement, and without tearing the peritoneum, in the same way that we tie the common and internal iliac, the dangers will be diminished, and the saving some patients will authorize us to admit in surgery a just indication for ligature of the aorta. I have composed the present article, with the express desire that this operation may hereafter prove useful, and at least afford in man the same results obtained in the lower animals." (p. 368.)

The last chapter of the work is on the "accidents of the lateral circulation." It may be deficient or excessive. Excess is not observed in the lower animals; but in sick men, plethora, inflammation, or a return of the aneurism, are often the result of a too active collateral circulation. Plethora and inflammation may appear in the part operated on, or elsewhere. In almost all limbs, some days after operation, heat, throbbing, pulsation of the superficial arteries, and local fever occur; but this is temporary and unimportant. But in the head, congestion and threatened encephalitis may, in a certain degree, be attributed to undue reaction of the anastomotic and capillary systems.

"Of seven cases of ligature of the carotid recorded, and four operated on by me, in five it was necessary to abstract blood repeatedly during the first days, to relieve the head from plethora; and this is singular, because in the one hundred and thirty-two cases of this operation, set forth in the table, mention of encephalitis is scarcely ever made." (p. 371.)

Dr. Porta then quotes cases by Abernethy, Langenbeck, Dohloff, and Wattman, in which evident marks of inflammation of the brain and its membranes were found after death, succeeding ligature of the carotid; and others by Landa, Wardrop, Magendie, and Barovero of Turin, in which the symptoms evinced a similar condition. But more frequently reaction in the head is owing to "reverberation" induced by obliteration of a distant artery. Many cases are quoted, in which cerebral inflammation has been found after death, or evinced by symptoms during life, after ligature of the subclavian, iliacs, and femorals. Ligature of the large external arteries has frequently occasioned inflammation of the viscera of the chest and abdomen, or of their investing membranes, probably proceeding in part from the "hydraulic disorder of the blood in the limb operated on," but more often from local inflammation and constitutional irritation. Still, of six hundred cases, in forty-six only has severe inflammation, either local or general, been set up.

A too free collateral circulation, and undue dilatation of the anastomotic branches, may induce a return of the aneurism. It will be seen by the table that this occurred in twenty-five of the four-hundred and forty-eight cases in which ligature was applied for the cure of aneurism or aneurismal varix.

Paralysis, atrophy, or gangrene, may be the result of defective collateral circulation, and these results are much more frequent in man than in the lower animals. They frequently precede the operation, being effects

of the compression exerted by the aneurismal tumour on the principal nerves of the limb; but either atrophy or palsy may occur separately, and as an immediate sequence of the ligature; or the atrophy may be one of the sequels of paralysis; or again, may exist alone, without alteration of the nervous system, being a simple effect of defective nutrition, from insufficient supply of blood. Several instances of each of these results are recorded in the text.

It will be seen by the table that gangrene was observed in forty-nine of the six hundred cases; more than half of these were attacked from the second to the third day, others from the second to the third week. When circumscribed, it was limited to the fingers, hand, foot, or leg, and advanced slowly; when diffuse, its course was rapid and fatal. It is seldom the result of the ligature alone, but depends on local and general causes in combination—as ossification of arteries, severe wounds, great size or diffusion of the aneurism, hemorrhage, ligature of nerves, or bad operating, general debility, or the presence of disease in the system.

We here close our analysis of this work, and need say but little as to its merits, as our readers can form their own opinion from a perusal of the preceding pages. Still we cannot leave our author without giving expression to the high opinion of his industry and perseverance impressed upon us by his work. He laid down certain objects for investigation, and proceeded in his task in a philosophical manner, making experiments wherever they appeared necessary, and collecting from the works and experience of others such facts and observations as were calculated to assist him in his researches. Although no results of any striking novelty have been thus obtained, yet much light has been thrown on the formation of direct anastomosis, and fair grounds have been afforded for regarding torsion more favorably in practice than hitherto. With regard, also, to the materials of ligatures, their form, application, permanent or temporary stay upon the artery, and their various ill effects, although the author's deductions from his experiments do not differ materially from generally received opinions in this country, still, by the number of his observations, he has afforded a far more solid basis than previously existed on which to found those opinions. It is not the least of his merits that he appears to have operated with so much care, so publicly, recorded his experiments with such good faith, and reasoned so soundly upon them, that many of the questions he has investigated may be regarded as finally determined, and consequently, those who might have been disposed to follow in the same path may employ their time and energies in other departments of science. The plates are excellent specimens of art, and the letterpress is carefully got up. Altogether, the book is both a very beautiful and a very valuable one.

We trust this work may be received as a token of the regeneration of the Italian school of medicine, and that at length observation and experience will take the place of the fanciful hypotheses which have so long fascinated and misled our southern brethren. We cannot convey our opinion of the merits of this volume better or more briefly, than by saying, that its author is worthy the place he holds, as the successor of the great Scarpa.



## ART. IX.

*A Memoir on Amputation of the Thigh at the Hip-Joint (with a successful Case).* By WILLIAM SANDS COX, F. R. S.—*London*, 1846. Folio, pp. 47.

THE present work answers very well to its title; it is indeed a very complete account of the operation of amputation at the hip-joint. The history of the operation from the period when it was first proposed, down to the latest date, is minutely drawn up from the materials collected by Hedenus and Velpeau, which are added to by Mr. Cox's research; and it is right to add that Mr. Cox has evidently in many, indeed, we believe most instances, consulted the original authorities referred to by Hedenus and Velpeau, and in one or two instances corrected the statements of the former writer. In the account of the various methods of operating which have been either proposed or executed, Mr. Cox does not, indeed, go into needless details respecting the minute steps of many of the modifications which have been contrived of the three leading methods, the circular, the flap, and the oval operations; but he describes each of those three classes of operations with all necessary explicitness and clearness. The method which Mr. Cox seems to consider as in itself the best, and which he adopted in the case that gave occasion for the publication of the present memoir, is the following:

"The patient is to be supported in a recumbent posture, and so placed that the tuberosities of the ischia project a little beyond the margin of the table. The compressor (Segnoroni's) is to be applied over the external iliac artery as it passes over the body of the os pubis, and the thigh must be partially flexed. The operator is to stand on the other side; the point of a narrow double-edged knife, about twelve inches in length, is to be boldly and steadily introduced at an inch below the anterior-superior spinous process of the ilium, and made to glide across the neck of the femur, parallel with, and a little below Poupert's ligament, so as to pass beneath the muscles of the anterior and internal region of the thigh, and beneath the superficial and deep femoral vessels, and be brought out about an inch below the margin of the anus. The knife is now to be carried downwards in a line with the anterior surface of the femur, from three to three inches and a half, in proportion to the bulk of the limb, and then brought obliquely downwards and forwards through the integuments, by which means the anterior flap will be formed. The flap is immediately to be thrown back, and its vessels, viz. the deep and superficial femoral arteries, at once compressed.

"The limb is now to be depressed and rotated outwards, when the capsule, being fairly exposed, is to be divided close to the edge of the acetabulum, and next the round ligament. The head of the bone will then be made to slip from the socket by a slight rotatory movement outwards, and the knife is to be carried through the joint. The posterior layer of the capsule, the extensor muscles of the hip attached to the trochanter major, and the small rotators inserted into the digital fossa of the same process, are then to be divided; the knife is to be passed downwards on a line with, and close to the posterior surface of the femur for about three inches or three inches and a half, and afterwards downwards and backwards through the integuments—thus completing the operation. The posterior vessels are first to be secured, and afterwards the vessels of the anterior region. The majority of surgeons recommend that the synovial membrane and cartilage of the acetabulum shall be scraped off; but I believe this to be a bad practice, and often productive of suppuration and disease of the bone. The cartilage and synovial membrane will take on adhesive inflammation in the same manner as the

other soft parts, and the secretion will be suspended. The nerves should be cut off as high as possible; for, if they should become engaged in the cicatrix of the wound, they would cause the most violent pain, not only during the cure, but after the wound was healed." (pp. 27-9.)

This operation, Mr. Cox says, "is a modification of Bécclard's and Lisfranc's plan," and it combines some features of both methods, improved perhaps by their fusion. As a general method it is, we have no doubt, a good one, and it answered exceedingly well in Mr. Cox's case; the limb, we are told, having been removed within thirty-five seconds. Of course Mr. Cox does not mean to recommend this method in every case; for, though we do not perceive that he has adverted to the point, in some instances the plan of operation must be governed by the direction in which the injury or disease for which it is performed has extended; and a method not advisable in itself may be the only one left for the operator to adopt. "The great objections," Mr. Cox observes, "to amputation at the hip-joint have been the difficulty of restraining the hemorrhage, and the shock given to the nervous system by the operation itself." (p. 25.) The rapidity with which the operation he proposes may be performed, tends greatly to obviate the latter objection, and the former he thinks is overcome by "the invention of the arterial compressor by Dr. Segnoroni, of Padua." We confess that we would feel more confidence in having the artery commanded by the hands of a good assistant than in trusting to the compressor; it would be absurd to question that the latter may be most effectual, but we would prefer the intelligent machine to the merely mechanical apparatus. Mr. Cox in describing Larrey's operation mentions his practice of tying the main artery of the limb as closely as possible to Poupert's ligament, but does not think it necessary to occupy space in condemning the proceeding. We may, however, mention that if the surgeon trusts to this method as a certain guarantee against hemorrhage, he may find it perfectly illusory. Thus in the operations performed by Peliken and Korseniewsko the vessel was tied, but nevertheless the hemorrhage was so profuse as apparently to have materially contributed to the fatal result of those operations.

Mr. Cox gives two statistical tables of the result of the operations in which amputation at the hip-joint has been performed, one exhibiting the successful, the other the unsuccessful cases. The whole number of cases contained in both tables amounts to 84, of which 26 were successful and 58 unsuccessful. These tables are not only most diligently and faithfully compiled, but, as far as our knowledge extends, almost complete. We are indeed aware of but one omission, which is a case, mentioned by Velpeau as having been seen by Delaunay at Moscow, in which the patient had perfectly recovered after disarticulation of the thigh, performed in consequence of gangrene of the limb. It might perhaps be objected that this case rests on hearsay, but it is in this respect on a parity with the case of the English sailor operated on after the battle of Aboukir. This case increases the number of successful operations to 27, and the entire number of operations recorded to 59. We think too that the case of M. Baffos, which Mr. Cox ranks among the unsuccessful cases, might perhaps be fairly placed among the successful ones, so far as the operation was concerned, for, to give Mr. Cox's summary of the result, "the wound healed healthy; on the sixty-third day pain came on, the cicatrix ulcerated and

opened, and he died at the end of the third month." Death resulted in this case from constitutional causes not from the results of the operation itself, and, therefore, in appreciating merely the results of the operation we should be rather inclined to place it among those in which the *operation* succeeded. Mr. Cox, however, states the facts accurately so that every one can form their own opinion, and is probably right in rejecting from his table of successful cases a case open to any objection.

We availed ourselves of the materials collected by Mr. Cox to examine whether any connexion could be traced between the success or failure of the operation and the cause for which it was performed, but the latter particular is unspecified in so many cases that no satisfactory conclusion can be deduced. It may, however, be mentioned that of the successful operations 14 were performed for traumatic causes; 7 (including the case mentioned by Delaunay) in consequence of disease; while in 6 the nature of the affection under which the patient laboured is unknown. Of the unsuccessful operations 20 were performed for injuries, 18 for disease, and 20 for causes not mentioned.

The disease under which Mr. Cox's patient laboured was one of an unusual character. A young woman aged 23, had undergone amputation of the thigh, at the age of 14, in consequence of disease of the knee-joint. This, therefore, we may observe, is the third case in which disarticulation of the hip has been performed after previous amputation of the limb above the knee, and all three operations were successful. About three months after this first operation ulcers formed round the cicatrix, and a very painful "substance" formed at the back of the stump; the ulcers healed partially but never completely, from the centre towards the circumference. About six years after the operation the integuments became hard and thick, and at length fungous growths formed on the surface, and the parts became affected with gradually increasing pain. A variety of treatment was employed during the course of several years, but the disease made progress and on July 1st, 1844—

"The integuments extending upwards, anteriorly for about three inches, and posteriorly for about four inches and a half, are of a dull white colour, and of a cartilaginous hardness; and above this, for a limited extent, anteriorly, posteriorly, and laterally, the same parts have a glazed, corrugated appearance, like that presented by an old cicatrix. Patches of fungous growth, of a livid colour, protrude from one-half to one-third of an inch from the general surface, at intervals. From these excrescences blood occasionally exudes and at times a sanious fluid. These excrescences are extremely tender and bleed on the least touch. The integuments of the upper part of the stump are of a perfectly healthy character and feeling. There is no enlargement of the cutaneous veins, or of the inguinal or femoral glands. The stump generally is tender to the touch; the pain nearly constant, sometimes of a dull aching character, sometimes throbbing." (pp. 30-1.)

The character and general appearance, and health of the patient were perfectly satisfactory. On examining the parts after the operation, the integuments were found to be converted into a mass of cartilaginous hardness, pearly white, and from three-eighths to five-eighths of an inch thick. Under the microscope this structure appeared cellular and vascular, with myriads of minute globules, interspersed with spindle-shaped bodies, as observed by Müller in many tumours. The adipose tissue was very dense and intersected with fibrous bands, as were also the muscles, which had

a granular appearance, were softened, and had undergone fatty degeneration, with the exception of those inserted into the trochanters. The arteries were not enlarged, and the muscular and perforating branches when injected, were found to terminate in a vascular network distributed to the integuments and fungoid growth. The nerves terminated in bulbous enlargements, that of the great sciatic as large as a walnut, grayish, solid, vascular, and sending off fibrous bands which were lost in the adipose tissue. The bone was sound. It would be tedious to trace the progress of the case after the operation, suffice it to say that after three months the patient was discharged cured.

There are one or two things respecting Mr. Cox's memoir of which we must complain. One is the cumbrous and expensive form in which it is published. Surely, if Mr. Cox did not wish it to appear in the transactions of some society, such as the Medico-Chirurgical or Provincial, he might at least have brought it out in the ordinary size, and at the ordinary expense of an octavo pamphlet. Neither can we pass without comment, the plan of publishing the work by subscription adopted by the author. The benevolent and very laudable desire of Mr. Cox "to benefit the poor patient," might have been carried into effect as well or better, without the necessity of offering to the subscribers the bonus of a *Memoir on Amputation of the Thigh*. What do the Prince Albert, the Earl Howe, and the scores of reverend and other gentry care for such an offering? The few medical men in the list might, indeed, have been properly presented with copies; but all the others could and would very well have gone without, while the money expended on the copies provided for so large a number of non-professional men, would have added not a little to the fund "for the benefit of the poor patient;" and Mr. Cox would have escaped the imputation to which we know he has rendered himself (we believe innocently) obnoxious, of having, in this proceeding, contemplated a novel and most effective method of puffing and glorifying himself, in the eyes of his non-professional friends. It is clearly for the non-professionals that the full-length portrait of pretty Elizabeth Powis has been prefixed as a frontispiece; as it is calculated to convey no tittle of information to surgeons; while the engraving, and especially the colouring of it, in red, pink, blue, green, and yellow, must have made a large hole in the treasure-trove of the "poor patient." All this is certainly in the worst possible taste: it more resembles the proceeding of one of our vain neighbours across the channel, than that of an English surgeon of high standing in his profession, and is totally unworthy of Mr. Cox's acknowledged reputation as a man of science and surgical authority.

In its present garb comparatively few are likely to become acquainted with this memoir. Either of the mediums we have suggested, particularly the first, would have given it a greatly more extended circulation; and we wish, for the profession's sake as well as the author's, that it had been rendered more generally accessible, as we know nowhere else that the mere English reader could find satisfactory information respecting the results of this particular operation. Indeed, we are not acquainted with any work on this subject in any language which contains that information so fully.

## ART. X.

*Sui Pregi e Doveri del Medico.* Del Professore ROBERTO SAVA, &c. &c.—  
Milano, 1845.

*On the Deserts and Duties of the Physician.* By Professor ROBERT SAVA.  
—Milan, 1845. 8vo., pp. 214.

WE have gone through the work before us with considerable interest, as it is calculated to afford information upon the condition and sentiments of our professional brethren in Italy, a subject well deserving attentive examination, and one which the standing and position of the author have enabled him to treat satisfactorily. The work is divided into a number of chapters, and we think we cannot do better than follow the author's arrangement, extracting the parts most worthy of notice, and making our own comments upon them.

The work is commenced by a short introduction on the rank of the profession, which the following extract will prove that Dr. Sava is not at all disposed to depreciate: it is thoroughly Italian:

"A great physician is the *first of men*: by the improvements with which he perfects the art of healing he becomes the benefactor of humanity; and by the empire he exercises over death he is in some degree the image of the divinity upon earth." (p. 11.)

It is stated a little further on that Molière by his sarcasms has done a great service to the profession, by correcting the defects of physicians, and rendering them, "what in reality they now are, the most intelligent and agreeable men in society," and that all agree in treating them as such. If this be true with regard to Italy, we can cordially congratulate our brethren on the position they have acquired; but some personal observation and many remarks of the author in other chapters lead us to fear that "the wish was father to the thought."

The first chapter is on the "preparatory studies of persons destined for the profession, and on their conduct at college," insisting on selecting such youths only for a medical career who have a natural disposition for it, giving them a sound general education, and then pointing out much such a course of study as is adopted in our medical schools.

The "entrance of a young physician into the world" is the subject of the second chapter. It appears that many of the evils recognized here are in full operation in Italy—the same boldness and confidence of ignorance, the same poverty tempting to charlatanry, the same supposed neglect of patient merit. Let us hear our author:

"The qualities essential to the physician to succeed in the world are not transcendent merit, great devotion to study, and profound judgment; but rather an exorbitant accumulation of charlatanry, untiring chattering, and an audacity that nothing can ever disconcert." (p. 29.)

Not a very flattering portrait of the "first of men!"

We pass over an unimportant chapter on the "estimation of the physician," and arrive at the fourth, on the "physician in society." To prevent



repetition we shall connect our remarks on this chapter with others on the ninth and sixteenth, "the exterior of the physician," and "*saper-fare*," or knowledge of the world. The author commences with some just observations :

" Medicine is not a science incompatible with indulgence in social amusement, nor does it exclude those who exercise it from the politeness, amenity, and graces that form the pleasure of society. One may be at the same time physician and man of the world ; and, if some melancholy doctors declaim against the study of the art of pleasing, in doing so they have less regard to the dignity of their profession than to the impossibility of correcting the pedantry of their character and the absurdity of their manners. The imperturbable gravity they carry into society, as well as to the bedside, is a veil under which they frequently conceal profound ignorance ; and the silly sarcasms they launch against those of their colleagues who join a clear intellect and agreeable manners to knowledge, are nothing more than a confession of their secret jealousy. There are close connexions between the arts of pleasing and healing. . . . Some young physicians, too much engaged by study, live with their books, and withdraw themselves from society to dedicate themselves to their learned researches. This constant occupation gives them an embarrassed aspect and a timid reserve, of which they can never correct themselves, and which frequently diminish the success to which the multiplicity and profundity of their acquirements entitle them." (pp. 38-40.)

Perhaps the young medical man, in his entrance into the world, is never so likely to err as in taking the happy medium between pedantry on the one hand, and fashionable frivolity on the other ; he is apt to run into either of these extremes, so injurious to his prospects. There is a powerful passage in Lord Brougham's dissertation upon the character of Sir Humphrey Davy we cannot forbear quoting. He says, " He was led away by the plaudits of fashion, and must needs join in its frothy, feeble current. For a while he is remarked to have shown the incongruous combination of science and fashion, which form a most imperfect union, and produce a compound of no valuable qualities, somewhat resembling the nitrous gas on which he experimented earlier in life, having an intoxicating effect on the party tasting it, and a ludicrous one on all beholders." Still *omnibus ornatum excellere rebus*, should be the motto of the medical man striving for excellence ; for if sound solid learning be not accompanied by the knowledge of a man of the world, and the habits and manners of a gentleman, it will become tainted with pedantry, and its use be confined principally to the closet ; and thus be far less likely to enhance the success of its possessor. The social pleasures of the evening need never interfere with the business and study of the day, and, enjoyed in moderation, are useful in affording relaxation to the mind, and in forming manners which give a lustre to knowledge, adorn it, and prepare a path for its progress. Lord Chesterfield, in his Letters to his Son, has observed, with great truth, that " the exercise of the mind in the morning whets the appetite for the pleasures of the evening, as much as the exercise of the body whets the appetite for dinner. Business and pleasure, rightly understood, mutually assist each other, instead of being enemies, as silly or dull people often think them. No man tastes pleasures truly who does not earn them by previous business ; and *few people do business well who do nothing else*." Most people must have seen genuine merit obscured by the want of a



little of the polish required by refined social intercourse. Men of solid acquirements, in the presence of patients much their superiors in position, are sometimes, on this very account, awkward and disconcerted, uncertain what to do, what questions to put, or what replies to give; and either becoming disagreeably forward in their efforts to overcome their shyness, or so confused that they cannot readily apply their knowledge, and the confidence of the patient is lost. This is, no doubt, often the true source of the complaints we hear of ignorant and unworthy persons being high in public estimation, while merit is neglected, unrewarded, or oppressed. We believe that transcendent merit must naturally triumph over all obstacles; but unless it be of this commanding order, it will not serve in the choice between a nervous, timid, or ill-bred man, and his collected, easy, and agreeable, though perhaps less learned, rival.

In the chapter on the "exterior of the physician," the subject of dress is treated at some length; but the end of the author's arguments is simply that every pretension to singularity should be avoided—negligence and the extreme of fashion with equal care. Carelessness in dress is offensive to most persons, as it shows an apparent indifference to please, and thus becomes an indirect offence to their self-love; and slovenliness, especially in a young man, is too often connected with a want of that extreme cleanliness which health requires, and sometimes to a degree that shocks common decency.

In the chapter on "Saper-fare," the *savoir faire* of the French, for which our language has no exact equivalent, there is a curious passage, which we quote, as it may illustrate the position of the profession in Italy. The author is speaking of the method of making a name, and says,

"It is very difficult for a young physician to make himself known in a large and spacious city. Here are accumulated a prodigious quantity of doctors of every class: officers of public health, army surgeons, surgeons of the administration, obstetricians, titled physicians, physicians without title and without name, midwives, &c. Here flourish charlatans of every species, from the herbalist, the homœopathist, the orthopedist, to the truss-maker and the curer of venereal diseases; even the druggists, with syringe or pestle in hand, mutilate prescriptions and give advice. What troubles, therefore, what labours, what circumspection are necessary to emerge from the crowd! How can the modest physician elevate alone the edifice of his renown? What time will be necessary to complete it?" (p. 113.)

We continue our extracts from this chapter, pointing out the trickery—which, though not actually recommended, is said to be necessary,—for marked reprobation, although it is to be feared that the progress of education is not so far advanced, even in our own country, as to render all that is said by any means untrue.

"The *saper-fare* of a physician may have for its object either glory or fortune. Few direct themselves towards the former; the crowd precipitates itself towards the latter. It is too difficult, even with great intrigue, to create a literary fame; but sure and ready means of becoming opulent offer themselves freely to the able and impudent man, who has prepared his success by subverting every sentiment of modesty and polite delicacy..... Some physicians, at the bedside of a patient to whom they wish to give a lofty idea of their knowledge, listen to him with great gravity, affect a profound comprehension, pronounce a few words in a

and does not equal the caliber of the tied trunk for some months. This apparent contradiction is explained by the natural elasticity of arteries, allowing an instantaneous dilatation during life, which cannot be detected after death. That such a vital dilatation does take place is proved by the following experiments :

"1. When the two carotids are laid bare in a living animal and one is tied, the other is very soon seen to become dilated and turgid, and to pulsate more strongly than before. In the same way the superior branches of the suprascapular and mammary, when the subclavian is closed below by forceps or ligature, at once become dilated, and are seen to be enlarged, while after death no difference can be detected.

"2. When in dogs, sheep, or goats, a day or two after the ligature of the subclavian or superficial femoral, the superior branches of the operated trunk are laid bare, they are observed turgid and capacious, but this disappears, and cannot be shown after death.

"3. When in any of these animals, or in the horse, the two carotids are tied in the middle of the neck, and one is opened above the ligature, at first nothing occurs; but after fifteen or twenty seconds a jet of blood commences, which rapidly increases, and continues as if it were from the cardiac extremity. It is true that this experiment is not sufficient proof of the sudden enlargement of the lateral anastomosis, because the communications of the carotids within and around the cranium with the vertebrals are naturally large, short, and direct; but in the subclavian, external iliac, and superficial femoral, where the natural anastomoses are much more slender, the same experiment affords the same result, and after ligature the blood, from an incision of the artery below it, jets forth in quantity until the animal faints and dies.

"4. When the ventral aorta is tied in an animal, and the femorals are opened, hemorrhage does not take place at first, but after a few moments the blood issues from the wounded arteries in the same manner in which it comes from open veins; doubtless on account of the sudden dilatation which it has produced in the lateral branches, although these vessels, both in this and the preceding experiment, appear natural in the dead body." (pp. 217-18.)

"In animals with transparent arteries, as frogs, I have frequently seen under the microscope, when an arterial branch has been closed with forceps, that the adjacent branches suddenly enlarge, and the blood is urged on with greater velocity. From these facts we argue that in the first moments after the ligature of the principal arteries of the limbs, the blood is accelerated in the lateral vessels, and by this acceleration partly overcomes their temporary want of size." (p. 219.)

But it is difficult to suppose that an acceleration of the circulation and some mechanical distension of the lateral branches is sufficient to maintain the integrity of a part or organ, when the innominate, or the two carotids and a vertebral, or the common iliac are tied, or at any rate to supply the same quantity of blood as before the ligature. It is more reasonable to suppose that a limb, left motionless, may for a time be deprived, without injury, of a portion of the blood with which it was supplied in the healthy state in quantity sufficient for the performance of all its functions.

The more minute branches, or anastomotic arches, are shown to be enlarged from the first day, not only from the easier passage of injected matters, but also by the eye and the scalpel. The enlargement is thus first detected in the "median arches, then in the secondary ramifications, and, lastly, in the lateral trunks, which are inserted above and below the

obliterated extremities of the artery." This may be explained by the greater thickness and resisting power of the larger branches.

When the circulation is established sufficiently to supply the necessities of the part, the vessels remain without further alteration; and this occurs at a period varying from some months to a year after the ligature. In dissections made from twelve to thirty-six months afterwards, no further alteration was found in the anastomotic system.

We find some important observations in the section on the effects of ligature of the carotids. In upwards of one hundred cases of ligature of one or both carotids in dogs, sheep, and goats, disturbances of the nervo-muscular system, indicating a deficient supply of blood, were never observed. In thirty-two experiments upon horses, asses, mules, and calves, alarming phenomena occurred in five cases. In three horses the thread securing the second artery was scarcely tied, than extreme agitation, spasm of the jaws, great anxiety in the chest, tremors and convulsive motions of the limbs came on, and death soon followed. Two mules, whose carotids were tied at the same time, suffered from paralysis of the extremities, particularly the posterior; they were obliged to be raised and kept from the ground by cords, on account of actual defect of muscular power. Their heads fell on the manger, they refused food and drink, and died in eighteen and twenty-four hours, while no recognizable alteration or injury to the nerves could be detected after death. The other nineteen cases were without any ill consequence. In calves, the operation was as safe as in dogs or sheep; the animal walks about and grazes immediately afterwards.

With regard to ligature of the vertebrae, Dr. Porta is of opinion that the weakness of the extremities, dyspnoea, and nervous symptoms, ascribed by Sir Astley Cooper to a defective supply of blood to the brain, are merely the results of apathy and moral depression, which follow every severe operation, and not attributable to cerebral anæmia. Sir Astley fancied that the phrenic nerves, especially, suffered deprivation of blood. The author argues that the anastomoses are so free that no want of blood is possible from simple ligature of the vertebrae. He has never seen the instant death, as described by Cooper, after compression of the two vertebrae; but he has seen animals suffer greatly, and this was to be attributed to their delicacy, or the extreme difficulty and long duration of the operation; and he believes that the vertebral arteries have no greater influence than the carotids upon the functions of the brain.

Eleven experiments were performed to determine the collateral circulation of the innominate after the ligature of the roots of the carotids and subclavian at each side; but the operations were very disastrous, six deaths occurring from hemorrhage, thoracic inflammation, or violent convulsions. Of the five survivors, two had no bad symptoms; in the other three, orthopnoea, from spasms or convulsions of the diaphragm, gradually disappeared in the space of a few days.

The common iliac was tied in sixteen dogs. Half died from acute inflammation around the spot, or from peritonitis; one or two from adynamia; but the mortality was increased by the animals biting away the dressings, and laying the parts bare.

magnify them to be. Flattery injures the giver, without benefiting the receiver. Certain of our physicians are qualified as excellent writers, while it is plain from their works, that they are ignorant of the elementary rules of the art of writing. . . . . Chénier has exactly defined the qualities that a good critic ought to possess. The ignorant person, he says, does not see what is beautiful; the detractor will not see it; the critic sees it, and puts it in evidence. He speaks of great authors that have passed away with respect, but not with idolatry. He is just to the dead, just and benevolent to the living. He does not limit himself to the admiration of grand works, but pays a tribute of esteem to useful labours. Criticism is the science of taste illuminated by justice. . . . . The end of criticism is to instruct. To fulfil it a journalist should possess profound and varied knowledge, in order to judge soundly of the relations to the medical sciences. A vast erudition must be accompanied by the art of writing, and especially by good taste; without which his criticism would disgust his readers. And in giving an account of a new book, he should avoid every kind of digression, follow the path of the author, and bring forward his principal ideas, either to approve them, or compare them with others analogous, put forth by contemporaries or by the older writers—endeavouring always to vary his style according to the theme, in order to sustain the attention. The nature of the materials submitted to his criticism does not prevent him from writing with the necessary elegance.” (pp. 48-53.)

We pass without further comment to a chapter on the “age of the physician.” It is written to show that talent and not years is the true test of the practitioner; that knowledge makes a young man old, ignorance an old man a tyro.

In a following chapter on “literary habits of medical men,” it is justly argued that he who devotes his whole time to the study of medicine does well, but that he who studies with equal ardour, and yet employs some time in general literature, does much better. The *belles lettres* have the same effect upon the intellect that an excellent diet has upon the body. Haller and others are quoted as proofs that physicians may be good poets; but the author strongly dissuades them from publishing the verses they compose as a pastime. He thinks the ridicule attached to an indifferent poet compromises the dignity not only of the professor but of his art. The study of moral philosophy is recommended from its numerous relations with medicine. We thus trace the formation of ideas, the rules that ought to direct life, the paths to happiness, the influence that different climates exercise over the physical powers of man and on social institutions, the power of regimen on the passions and intellectual faculties, and that of disease upon the operations of the intellect. We advance even to the operations that constitute the function of intelligence and determine the will, and learn to decipher the various characters of the passions. Nothing can ever separate the study of the physical man from the study of the moral man.

After a long chapter successfully combating the charge of atheism and irreligion brought against medical men in general, the author treats on the relations of physicians with each other. We are sorry to see it stated that “in general true friends are rarely found among individuals of the same profession; envy and interest are in opposition.” (p. 98.) It says little for our Italian friends. The medical philosopher should be superior to the petty calculations of sordid interest; it is only base minds that envy affects. This the author fully allows, and urges the most scrupulous

delicacy in all our dealings with each other. One of his hints will not be understood in Italy only.

"Nothing can be more shameful than the transactions which take place between some physicians and unscrupulous druggists. Every physician who feels the dignity of his profession will reject these illicit gains and vile associations." (p. 139.)

It is not every day that we can look over a series of portraits of the professional men of any nation, drawn by one of themselves; but here, in a chapter "on some defects which should be avoided in practice," there is quite a little picture gallery. We shall follow the guidance of Dr. Sava, and act as interpreter to our readers.

"1. *On routine, or mechanical practice.* The routine physician practises an art, the philosophical principles of which are unknown to him. Without good foundations, without medical genius, he reasons solely upon the perceptions of the senses. Aged in his ideas, indifferent to the progress of science, he obstinately confines himself within the limited circle of certain actions; and all his knowledge, all his ability, consist in seizing the first appearances of things, and in prescribing certain formulæ. He resembles the pilot in a tempestuous sea covered with rocks, who is ignorant of the existence of the compass as a guide in navigation. Very ignorant, therefore stubborn and obstinate, he is incapable of supporting the arduous labour and profound meditation that the difficult art of recognizing and curing diseases require from the practitioner. Like a machine, the wheels of which always determine the same effects, the routine physician always repeats the same acts. His indolent, dull, obtuse intellect is never applied to reflection, and abhors every thing that has an appearance of labour: observation to him is dumb, and its brilliant light cannot clear away the dense cloud which closely covers his eyes.

"Many varieties of routine physicians may be distinguished—some, servile imitators of the ancients, are far from supposing that twenty centuries of experience have wrought any progress in medicine. Hippocrates was a great physician, yet he was not acquainted with tartar emetic and quinine—therefore in their opinion quinine and tartar emetic are useless remedies. Such physicians declaim against vaccination, and in general against all the discoveries of genius. Others unite to the most profound ignorance—to the absolute incapacity of appreciating the merit of the ancients—a stupid pride which will not allow them to recognize any thing valuable in their contemporaries. No principle guides them, and they might paint themselves approaching blindfold the bedside of a patient, close to which death stands watchful and prompt, raising an axe impelled by chance . . .

"It is difficult to visit a great number of patients and defend oneself from the tendency to blind medication induced in man by the natural slothfulness of his intellect, and on this account routine physicians especially abound in hospitals.

"These persons recognize a disease at a single glance—that which presents the most difficult diagnosis is characterized most easily by them—nothing embarrasses them. After a few questions made for form's sake to the patient, they mechanically prescribe a formula, which the student, after having heard the indicating word, writes at full length in the visiting-book. This is all their art—this is their conduct, always the same. But these practitioners, the number of whom is fortunately inconsiderable, scarcely know the faces of their patients.

"Some physicians become machines as they grow old; age does not allow them to follow the progress of science, or subject themselves to new studies; obstinately attached to their old doctrines, they will vary nothing; every thing new they treat with disgust and disdain, and thus they never read. After fifty years of medical practice it is impossible for them to adopt other principles, different from those they have acquired, and been accustomed so long to follow.



"2. *On presumption.* Do not ask this doctor what he knows, but rather what he is ignorant of. He has read every thing, has seen every thing; the most delicate surgical operations to him are mere pastime; nothing confounds him: his genius foresees every thing, undertakes every thing. He speaks in magnificent periods, and considers it dishonorable to appear ignorant of any thing. What diseases has he not cured? Confirmed cancer and hydrophobia in his hands have ceased to be incurable; without vanity he believes himself to possess all the knowledge that he can have, or can even acquire: the first aphorism of Hippocrates has no signification for him: in him he believes that the power and genius of Esculapius' self are infused.

"3. *On timidity.* This physician has great talents, profound and vast knowledge, yet he does not succeed, and will never occupy a part in the hierarchy of his profession; and with the most extensive acquirements, he has the appearance of ignorance. Question him—his replies are most confused and inexact. The most simple cases alarm him—he detests acting, and always does it with fear.\* In vain nature announces a favourable result—still trembling he dares not second her. He has never felt those sudden unlooked-for inspirations which reveal to the man of genius the character of a disease complicated in its progress and diagnosis, and make him discover in untrodden paths the means of overcoming its violence and obstinate resistance. Thus he loses in deliberation the favorable opportunity and the moment of risking with advantage. Such a physician does not kill his patients, but he lets them die.

"4. *On false judgment.* Some physicians puff themselves up with pride, boasting scepticism in their science. Free from every prejudice, they regard the precepts of the oracle of Cos as idle nonsense. Immovable in their opinions, they look upon the most authentic facts as fables, and the art of knowing and treating disease is, in their eyes, quackery founded upon the ignorance and credulity of the vulgar. But how are we not imposed upon by persons initiated into all the secrets of medicine? How suspect them of unfairness, when in truth they make the sacrifice of so many years of study of such painful toil? In this manner many ignorant persons argue. Still the impartial man soon discovers in these pyrrhonians, these empirics, that, disgusted by an unfortunate practice, they impudently accuse medicine of the errors exclusively imputable to their own ignorance. Without learning or talent, ignorant of the elementary principles of science, with judgment essentially false, in order to appear free-thinkers in the profession, they slander that of which they are ignorant, condemn every thing they are incapable of comprehending, and become a mark of the public contempt for daring to exercise a calling they themselves condemn as useless to society.

"Other physicians perceive nothing obscure in the science of humanity. Nature has no secret they cannot discover—no veil hides from their penetrating regard the mysteries of our organization. There are no diseases they cannot perfectly explain and cure. They regulate themselves blindly by all the observations contained in books, and all the axioms of Hippocrates appear to them immutable truths. Experience would accuse their doctrine in vain—the teacher has said it,—they reply in his defence that he is never deceived. Thus for them all new discoveries in science are valueless, indeed they will not suppose them true. All the phenomena, all the changes that a disease presents during its course, depend in their eyes, not upon the efforts of nature, but upon the drugs they have administered, however inactive and useless these may have been, and with their lofty idea of the powers of medicine. They imagine that none of the evils which

\* We once heard an old and experienced physician of this stamp declare, that though he had practised medicine fifty years, he had scarcely ever left a patient's house without feeling a desire to go back and alter his prescription! And yet our friend was a man of great intellectual power and much learning. He was only singularly deficient in force of character, or, as the phrenologists would say, in *combattiveness*.—ED:



afflict the human species can resist them; and indiscriminate dispensers of tonics, bloodletting, emetics, and the more active remedies, they still think that they must act, and act with all energy.

"These are fanatics in medicine: such are the enthusiastic partisans of any doctrine. Let every one beware of censuring their venerated idol; if he should have such temerity, the abuse vomited from their mouth would equal in quantity the words that Homer makes the aged Nestor deliver in public discourse, comparing them to sheets of snow impetuously falling. They are filled by exclusive admiration, and despise even what is judicious in other schools." (pp. 140-7.)

We have given this chapter at length as a good specimen of the author's style—perhaps the portrait painting is not altogether free from caricature, but if it be true that men are more readily laughed out of their errors than preached out of them, it may not be without its good effect. The succeeding chapters "on the mode of questioning patients," "on the patience, prudence, charity, discretion, chastity, philanthropy, and magnanimity of the physician," "on the peculiar qualities of the surgeon," "on the duties of physicians to the dying," "on mental medicine," and "on fees," are sound and sensible, but rather too commonplace to be adapted to our pages. That on mental medicine is perhaps the worst in the book, being merely a superficial glance at a most important and interesting subject. We felt almost tempted to preach a sermon on the author's text, but our space is already exceeded, and we must close with thanks to Dr. Sava for a well-written and interesting book, full of sound, sensible, and acute observation. The only fault we find with it, is that self-love and self-interest are too much worked upon as main-springs of conduct, to the neglect of those high feelings of honour which should guide every one in his course through life.

On looking over some of our remarks on the manners of the physician, it seems to us possible that some of our readers may be led to believe that we attach undue importance to superficial accomplishments. We wish, therefore, to observe here, that, while we consider a deep and thorough knowledge of his profession to be the real claim of every medical man to distinction and reward, we are desirous that these claims should not be obscured by inattention to minor matters. The grand difference between the well-bred and ill-bred man, lies in the degree of attention paid to the feelings of others. A man of common sense, with a moderate supply of good nature, just sufficient to make himself cheerfully undergo a little self-denial for the sake of others, cannot be guilty of any real offence to good breeding. It requires a certain amount of observation and knowledge of the world to acquire unembarrassed, easy, and graceful manners; but the more the power of such manners is insisted upon in covering the want of merit, the more clearly do we see their influence in adorning it.

## ART. XI.

*Scrofula ; its Nature, its Causes, its Prevalence, and the Principles of Treatment.* By BENJAMIN PHILLIPS, F.R.S. Assistant Surgeon to the Westminster Hospital.—London, 1846. 8vo. pp. 379.

THIS is one of the few books which may be said to be after the critic's own heart. It is a book that was wanted ; the object of it is excellent ; it has been carefully planned ; the investigations required by the plan have been conducted with the utmost energy, industry, and carefulness, in a sufficiently extensive field, and over a sufficiently long period of time ; the immense mass of materials thus obtained has been examined and weighed with the greatest attention and impartiality ; the inferences and results have been honestly and cautiously deduced and elaborated, and condensed into the smallest possible space consistent with perspicuity ; while the whole style and manner of the composition is such as ought to characterize the production of a well-educated, a learned, and an experienced surgeon. In saying thus much, we are far from implying that the work is a perfect one ; and we suspect that the author is himself the last person who would claim such a character for it. It is, however, but doing simple justice to Mr. Phillips to state, that we believe the work to be as complete as it could well be made under all the circumstances of the case. Though generally agreeing with the author in his views and conclusions, we occasionally differ with him in both ; and in presenting to our readers as fair and complete an analysis of his book, as we can, we shall not hesitate to display what we may regard as its errors with the same freedom as we hold up its manifold excellences to the admiration and imitation of his brethren. It would be paying the author of such a book a compliment most unworthy of his acceptance, to hesitate to express our honest and candid opinion on any of the questions discussed in it, whether that opinion was in unison with his own or in opposition to it. His object, like ours, is to reach the truth ; and he will not, we are sure, quarrel with us or any one else who can aid him, even in the least degree, in attaining it.

We cannot do better than introduce our review of this work in the words of its author.

“ In the course of my professional practice, and especially in the discharge of the duties entrusted to me as surgeon to a large metropolitan infirmary, the treatment of scrofulous affections formed a subject of frequent and anxious consideration. I often felt desirous to relieve myself of anxiety, by consulting the experience of others, as recorded in their published works, but I found that something was wanting in all those to which I referred—too much was assumed, too little carefully examined ; and although many of the works on scrofula which we possess have deservedly acquired a high reputation, at an early stage of my investigations I was led to the conclusion, that much was yet wanting to complete our knowledge of the disease . . . . Fully satisfied that a more extended collection of facts, and a more accurate classification of the phenomena than had yet been attempted, were necessary and practicable, I entered upon the task which has now been brought to a close, and men who have been engaged in such extensive and minute inquiries can alone appreciate the patient labour which has been devoted to the work.

“ The materials which I have obtained and used, are probably the most ample

that have ever been collected and brought to bear on a single medical subject; and if I had contemplated the difficulties I have encountered in procuring information, or the extent to which the inquiry would have proceeded, I should probably have been deterred from prosecuting it to a conclusion. But as is the case in all such undertakings, the obstacles and discouragements were only gradually unfolded, and the labour was lightened by the interest which the subject itself excited." (p. 4.)

Mr. Phillips commenced his more extended collection of facts with his own work-circle at the Marylebone Infirmary. Finding this too limited, he visited, to use his own words, a large number of the national schools, the parochial establishments, and the charitable institutions which are found in every part of the metropolis. He was thus enabled to determine whether the healthier and wealthier localities of St. George's, Hanover Square, St. Marylebone, and Kensington, were more lightly visited by the disease than the comparatively poor and unhealthy districts of Shoreditch, Bethnal Green, St. Giles's, and the lower part of Westminster. Having ascertained, as far as was practicable, the condition of the population of the metropolitan districts, where the effects of close packing may be expected to be keenly felt, he pursued the inquiry through districts where the population was widely spread; and he was thus enabled to compare the condition of the people living under very different circumstances, and to ascertain the influence of crowding upon human life, in so far as concerns scrofula. He extended the investigation to the mining and factory districts, with a view to determine how far particular operations tended to develop the disease. He caused children to be examined in the north and the south, the east and the west of our own country, so as to be able to estimate the influence of particular localities in our own island to determine the affection; and he extended his researches throughout Ireland and Scotland, so as to be enabled to estimate the influence of particular articles of food; and the correctness of the evidence so obtained, has been to a certain extent tested by the condition of recruits, and of persons committed to prisons; by the mortuary tables of the Registrar-General, so ably analyzed and arranged by Mr. Farr; by those of the Irish census, so well digested by Mr. Wilde—in so far as they could be made available in an inquiry like the present; and by a large number of hospital and dispensary returns.

Mr. Phillips's ambition grew by what it fed on, and he now found that the sphere of the British isles formed too limited a boundary for observation. He therefore pushed his statistical inquiries into the four great continents. Through the kindness of Lord Aberdeen, he has obtained most valuable reports from Russia, Austria, Prussia, Bavaria, Portugal, and Holland. He has acquired important information from the northernmost portions of Europe, as well as from France and Belgium, and in Bavaria, Belgium, Tyrol, and Switzerland, he has been enabled himself to verify the accuracy of the information with which he had been furnished. He has also obtained returns of the condition of children in so much of the United States of America as is found on its eastern shores, between Boston and New Orleans; from a certain portion of the Bengal presidency; from some districts in China; and from portions of Egypt, Syria, and Greece, abutting on the Mediterranean Sea, and from Madeira.

Mr. Phillips drew up forms of inquiry which he procured to be transmitted to all such persons as were likely to assist in his researches.

Early in his progress Mr. Phillips found the definition of scrofula and of the scrofulous temperament to be rather a knotty difficulty. He was very much in the predicament of Richard Wiseman, who had to select the persons to be placed before Charles the Second, to be touched for “the evil,” and he came to a conclusion very similar to that at which Wiseman arrived. Wiseman observed that none of the definitions which limited struma to an enlargement of the cervical or inguinal glands, sufficiently expressed the disease termed in England the king’s evil. “For I appeal,” he says, “to the practitioners of this nation, whether physicians or surgeons, desiring them to say whether there be not many tumours and ulcers commonly judged to be the evil, which are contained in no cystis at all, neither do they concrete into a glandule. I instance in the upper lips chopt, divers tumours by congestion in the muscous parts of the body, others in the bones, &c.; so that either we must exclude all these from the king’s touch, or alter our definition.” In his diagnostics for the court, he, however, takes the enlarged cervical glands for a pathognomonic sign. He says, “These which we present to his majesty are chiefly such as have this sort of tumour about the musculus mastoides, or neck, with whatever other circumstances they are accompanied; nor are we difficult in admitting the thick-chapped upper lips and eyes affected with a lippitudo; in other cases we give our judgment warily.” Mr. Phillips had to define cases of scrofula for statistical tables, and not for “the king’s touch.” He limited the pathognomonic signs to enlarged cervical glands discoverable by the touch or the sight; sinuses, or ulcerations resulting from them; and ordinary scrofulous bones or joints. We subjoin the form which Mr. Phillips employed for the collection of the extensive statistical data to be subsequently noticed, together with his explanatory remarks:

1. Number of children examined between 6 and 16 years.	BOYS.		4. Number exhibiting the evidences of scro- fula described in the third column, and possessing the charac- ters described in the second column.
	2. Number of such chil- dren who have de- cidedly fair hair, and light blue or light gray eyes, and a fair, soft skin.	3. Number of children exhibiting any of the following marks of scrofula: enlarged cervical glands disco- verable by the touch; sinuses or ulcers, suc- ceeding to such glands; scrofulous bones or joints, or the conse- quences of them.	
	GIRLS.		
	DIET.		

"The object of the second question is to ascertain how large a proportion of cases is found among those children who have what may be fairly called a light complexion. With reference to the third question, it is not pretended that every mark of scrofula is included; but as opinions as to what is and what is not scrofula are very different, it seemed important to fix upon certain points which did not admit of much controversy. In a healthy child, the lymphatic glands about the jaw and the neck are not perceptible on passing the finger over them; whenever, therefore, they can be detected, they are to that extent in an abnormal condition, though not necessarily scrofulous. All that is required for the purposes of the present inquiry is to pass the fingers along the spaces under the lower jaw, and parallel to and in front of the mastoid muscle; and if sensible enlargements can be detected, the case would be enumerated in the third column, which includes all cases in which such enlargement can be perceived." (p. 10.)

The mere signs of scrofula do not teach us much as to its nature. To this question Mr. Phillips devotes the second chapter of his book, previously appropriating a chapter to the exposition, examination, and demolition of antecedent theories. He plunges at once into the middle of his epic; thus:

"I conceive, then, that scrofula is a disease of the constitution, and that it is most clearly manifested by certain external signs, of which swelling of the subcutaneous lymphatic ganglia is the most conclusive. But *tumid glands*, however, wherever they may be situated, are not always a proof that a constitution is scrofulous; they may be the result of local irritation, in an apparently untainted constitution. The glands in the groin may swell from a sore on the foot; a mesenteric gland may swell under the influence of an ulcer in the intestine; a cervical gland may enlarge under the irritation of teething, or of scalp disease. A *tumid gland*, even in the neck, is then no proof that the constitution of the individual in whom it is found is scrofulous. But supposing one, or several cervical glands to become tumid, in the apparent absence of any obvious local irritation, this would constitute a strong ground for suspicion that the constitution was suffering under the taint of scrofula. It would not, however, amount to more than suspicion, and the suspicion could scarcely receive absolute confirmation, unless we have the opportunity of observing the contents of the tumour itself. Unless the swelling of the gland be accompanied by the deposit of a product, hereafter to be described, known as *scrofulous matter*, the proof of a scrofulous constitution is, in my judgment wanting.

"In so far then as the local manifestation is concerned, the question, 'What is scrofula?' admits of a tolerable satisfactory solution; but as I regard the local affection, wherever seated, as clear evidence of constitutional disease, it is most important to inquire what that state of the constitution is which causes the local affection; and whether, in the absence of the local affection, there be any certain means of determining whether a constitution be scrofulous or not. Because it may be that the means at our disposal may be powerless to remove the matter when once deposited." (p. 27.)

Mr. Phillips is well aware of the objection that may be advanced against this view of the case. There are scrofulous diseases in which no scrofulous matter can be demonstrated; scrofulous ophthalmia, for instance.

"Supposing such an objection to be made, my answer is this: that the ophthalmia in these cases is a simple catarrhal inflammation, set up in a scrofulous constitution, and acquiring its particular characters, not in virtue of anything specific in its nature, but in consequence of the state of the constitution upon which it is grafted. The particular character to which I allude is a peculiar irritability, which is a prominent feature in inflammation set up in debilitated constitutions, and there is certainly nothing uncommon in such inflammation in persons whose health

has been broken by other causes than scrofulous disease. At the time I visited the Whitechapel workhouse, 'scrofulous ophthalmia' was epidemic among the girls. Of eighty-nine present, seventy were suffering from the disease; but of the eighty-nine, only fourteen exhibited any of the ordinary marks of scrofula; and the proportion of girls who presented marks of scrofula, found among those suffering from ophthalmia, was not greater than among those who were free from it.

"I believe that diseases regarded as scrofulous, but in which no scrofulous matter is present, are not scrofulous at all, but simply the result of such low inflammatory action as is often set up in a debilitated state of the constitution." (p. 28.)

We think this answer to the objections is inconclusive, for sundry reasons. Firstly, we are told nothing of that "peculiar irritability," which is the pivot of the answer. Secondly, we doubt very much whether all the cases of so-called scrofulous ophthalmia, noted in the Whitechapel workhouse, were true examples of that disease; that is to say, were genuine examples of that well-known variety of ophthalmia, which occurs, from time to time, among strumous children, and with the character of which none is better acquainted than Mr. Phillips. Did it not rather come into the category of those epidemic ophthalmias which, in their more formidable forms, have desolated barracks and prison-houses, more especially on the Continent, and have been made the subject of so much discussion, and the cause of so much authorship? We think it beyond question that such ophthalmia prevails occasionally in a mild and modified form amongst the pauper children maintained in the workhouses. Thirdly, it is certain that there may be extensive scrofulous disease of the joints (one of Mr. Phillips's pathognomonic signs, and one of those on which his statistics are founded), and yet the cervical glands be unaffected. In such cases we must adopt Mr. Phillips's own conclusion, that "those diseases regarded as scrofulous, but in which no scrofulous matter is present, are not scrofulous at all." "The consequences of scrofulous bones, or joints," may also exist without any trace of scrofulous matter; so that, after all, we fall back to Wiseman's principle of selection for his majesty's touch.

*The signs of a scrofulous constitution*, as generally stated by authors, are fairly open to the objections advanced by Mr. Phillips. They are too indefinite; in a large proportion of cases they are not present; in another large proportion they exist, but without signs of scrofula. The following is Mr. Phillips's account—and it is a masterly one—of the qualities most commonly met with in those who are tainted with the disease:

"In the form of the body there is usually observable a want of muscular development; but even this is often absent. There is often an appearance of plumpness, or roundness, which is the result not of muscular development, but simply of an hypertrophied, or infiltrated condition of the cellular tissue, and which rapidly disappears under fatiguing exercise, privation, or disease. Commonly there is a general paleness and coldness of the surface of the body, which is owing to a feeble circulating apparatus; but in a large number of cases, about one-fifth of the whole, that paleness does not extend to the face. The colour of the hair is very variable, but for the most part it inclines to a dark tint. Of nearly nine thousand scrofulous children I have myself examined, a little over thirty-two per cent. had light hair and eyes. The *alæ nasi* may be broad, but for the most part they are not so. There is not, as some persons have supposed, anything constant in the



shape of the lower jaw, or in the appearance of the teeth. The abdomen is commonly tumid. The whole of the mucous surfaces are especially liable to derangement; discharges from the nose, the eye, and the ear are common. The digestive mucous membrane affords early indications of suffering; the tongue has commonly a dirty whitish coating; the tonsils are usually enlarged, and they are often so tumid as to impress a disagreeable and frequently husky character upon the voice, and to cause snoring when the patient is asleep. A still more deleterious influence is exercised by these tumid bodies; they lessen so much the channel for the passage of the air in respiration, that the sufficient development of the chest may be interfered with. The stomach and bowels are frequently disordered, and digestion is ill performed; acrid eructations are common; flatulence is often very troublesome; and the action of the bowels is very irregular, sometimes relaxed, at others constipated. Sometimes the evacuations are clay-coloured, very offensive, and of varying consistency; at others having a redundancy of bile. Similar evidences of derangement are observed in the air-passages, commencing at the nose (which exhibits increased secretions upon the occurrence of very slight variations in the temperature), and passing through their whole length. Similar phenomena are observed in the mucous tissue of the genito-urinary system; the bladder often shows an impatience of the presence of the urine, and the desire to void it is often frequent. The skin, though often dry and hard, is sometimes the seat of a considerable greasy exhalation; sometimes it is found to be fetid and sour. The acidity of the exhalation may be so decided as to determine a reaction upon litmus paper. In many of the cases observed by Mr. Kaye, on the Mediterranean coast, it was so. The scalp and other parts of the cutaneous integuments are often the seat of eruptive affections. The absence of vascular and muscular energy often causes the child to lie and sit about much, and indisposes him to enter into the energetic games of his playfellows. As to the intellectual development claimed for scrofulous persons, I am bound to say that it is usually wanting. That many scrofulous children present that character is quite true; but the result of very careful observation has convinced me that the overwhelming majority are without those superior intellectual qualities which have been pointed out as their ordinary character. Among the better classes the feebleness of a scrofulous child attaches to him an interest which, without it, he might not have enjoyed. To compensate for his physical inferiority, the anxious parent seeks to make him mentally superior to his bodily stronger fellows, and frequently succeeds; but often the limit of healthy action is passed, the nervous and intellectual systems have the vital action concentrated on them too intensely. The sufferer loses flesh, the general health languishes, and the intellectual faculties may give way, destroyed by an opposite, but not less sure, method than that which breaks down the poor man's child." (pp. 30-2.)

We have not yet got to the author's ideas as to the nature of scrofula: we have the pathognomonic signs, and we have the constitutional signs. But the scrofulous constitution is not scrofula; there is a wide divergence from the state of health—the constitution is fitted for the development of scrofula—but the scrofulous constitution is only a reality when the deposit occurs. Mr. Phillips observes:

"That condition of the economy, then, which is favorable to the formation of scrofulous matter is not scrofula, but a diathesis, or disposition; it may exist long, it may cause the tumefaction of many glands, but, under favorable circumstances, it may disappear without the deposit of a single particle of that product, whose presence in the glandular structures is, I conceive, necessary to constitute scrofula. Again, I say, the condition of the system favorable to this deposit is marked by no certain external signs, up to the moment when the glands become tumid. The child may be fair or dark, pallid or ruddy, well fed, clad and lodged, or all these may be the worst possible; he may be the child of wealth or of poverty; he may

live in town or country; he may be the child of old or young, of healthy or sickly parents; he may be born and live within the tropics, or in the arctic circle; under all these circumstances, as we shall see hereafter, scrofula may be developed." (p. 37.)

*The scrofulous deposit: its physical characters.* This is the subject of the third chapter. At its commencement we have at last a real definition of scrofula. "We," observes Mr. Phillips (meaning *I*, for only Kings and the Vehmgericht of the critics claim the dignity of the *Wæ*), "define scrofula to be a disease manifested by a peculiar deposit in the subcutaneous lymphatic glands." The qualities of this peculiar deposit are thus described by Professor Albers, Mr. Dalrymple, and Mr. Gulliver: the two latter made their observations by the special request of the author.

First, M. Albers:

"The tubercle presents, under the microscope, separate minute tubes, which under a linear enlargement of five hundred and fifty times, prove to be cells. This is not the case with scrofulous matter, which is granular." (p. 40.)

Secondly, Mr. Dalrymple:

"The whole material is composed of disintegrated tissue, granular molecules, irregular exudation-corpuscles, and in which the nucleolus is seldom to be recognized, and a considerable quantity of oil-globules, which may be abstracted by boiling in ether, and recovered by evaporation on a plate of glass.

"In acute or chronic inflammation of the glands, in otherwise healthy subjects, in whom no particular morbid disposition exists, the exudation-corpuscles, by what appears to be a law of vitality, proceed to the development of a cyst around the nucleus, or cytoblast; and this nucleus even splits into two or more, and hence a pus-globule is formed. At this point, however, the process stops, and the pus-globule subsequently disintegrates, and is resolved into granular and fluid matter. During the development of the cell and fissure of the nucleus, a pus-globule may be said to be an organic and vitalized body, deriving its means of increase from the blastema around.

"The exudation-corpuscle, however, is capable of a much higher degree of organization, and, under favorable circumstances, the cell-germ produces its cell, the cell elongates, and either fibre or filament is produced, as in the healing of a wound.

"In this scrofulous matter it appears that the exudation-corpuscles do not possess even that feeble vital power, which induces the further change into pus, and therefore it passes from the nucleolated cytoblast into an irregular granular body (disintegrated), the elements of which, by some further chemico-vital process, resolve partially into oil, or fat-globules." (p. 40.)

Thirdly, Mr. Gulliver—his observations upon tubercular and scrofulous matter have been very extensive:

"In the human subject, it appears to me that crude tubercular matter, from whatever organ obtained, differs as little in its microscopical as in its general and chemical characters. When examined by the aid of the microscope, crude tubercular matter can scarcely be said to present any regular structure, as it is merely made up of minutely granular matter, oily spherules, some shapeless albuminous flakes or shreds, and a few irregular corpuscles; the latter are probably nothing but effete, or shrunken primary cells." (p. 41.)

The chemical characters of scrofulous matters are, not very clearly made out. It is certain, however, that a large proportion of albumen enters into their composition, and that they may vary considerably in the proportion of chemical elements.

*State of the organ in which scrofulous matter is about to be deposited.* It is well known that there is considerable hypertrophy of tissue and increased vascular action in the glandular structures previously to the deposit of the scrofulous matter. Mr. Phillips raises the question, whether this morbid state is dependent upon the blood circulating through the structures, or whether it is of local origin. Mr. Dalrymple thinks the glands are in an inflammatory condition. He thus describes the microscopic appearance of a scrofulous cervical gland.

"This enlarged gland appears to consist of a general parenchyma in a state of chronic inflammation, surrounding irregular masses of yellowish white matter more immediately the subject of examination. In direct proximity to the edges of this white material, the blood-vessels are seen to be more enlarged and congested than elsewhere, and in some parts, the capillaries are occluded with coagulated blood. The parenchyma, which at first sight appears healthy, is, on examination with high powers, found to be infiltrated with exudation-corpuscles, resembling lymph-globules. The natural texture of the gland consists of its proper corpuscles, filamentous tissue, blood-vessels, lymphatics, and nerves. In this morbid specimen, everywhere is the filamentous tissue infiltrated, and its fibres separated by innumerable exudation-corpuscles, and the proper corpuscles of the gland are similarly surrounded and imbedded. As the parenchyma is nearer to the white matter, so proportionally do the proper corpuscles of the gland become more indistinct, the filamentous tissue more obscure, the blood-vessels irregularly dilated and filled with red globules, and they at last disappear insensibly. The exudation-corpuscles are more numerous, but irregular in size and shape, and interspersed with minutely granular matter." (pp. 45-6.)

Mr. Phillips agrees with Mr. Dalrymple, and probably with the great majority of pathologists in the opinion that the glands are inflamed.

"This congested, inflamed, or hypertrophied condition of the subcutaneous glands is of very frequent occurrence. It can be detected in one-fifth to one-sixth of the juvenile population of this country. If we examine the necks of delicate children, we very commonly observe that the lymphatic glands of this region are enlarged. In the state of health, the eye and the finger fail to detect them at all, and when they are thus cognizable, the proof is complete that they have undergone the change of structure to which I have referred. A tumour of considerable size may result from the aggregation of a cluster, or a chain of such glands; that tumour may completely subside, so as to leave no trace of its existence behind it, and this is the result in probably nine cases out of ten where such swellings have been observed. This is proved by the fact that they are enlarged so as to be felt in more than 20 per cent. of the juvenile population in England and Wales, and that they are not found to proceed to suppuration in more than 2 per cent. of that population; and I regard this fact as satisfactory proof of two circumstances—the hypertrophied or inflamed condition, on the one hand, and its entire subsidence on the other. And that subsidence I assume to be a proof that no scrofulous matter was deposited, because I have no reason to think that when scrofulous matter is deposited in any structure, it can be removed by any process of absorption; therefore, when the deposit has once taken place, the swelling it occasions must remain until the matter is ejected, because complete recession does not take place while the deposit is present in the part." (p. 46.)

It is generally in the largest and reddest gland, that the scrofulous deposit is found, but this is not invariably the case.

*Whence is the scrofulous matter derived?* Mr. Phillips investigates the theory of infarction of the glands by inspissated lymph, or chyle (in the case of the mesenteric glands). He sets it aside, however, for the

more modern theory that morbid products are derived directly from the blood, and raises these questions: "Has the blood previously undergone a change in its elements, the part in which the deposit is made remaining unchanged? Or, does the blood remain in a healthy condition, the part in which the deposit is made exercising a specific influence upon the blood, and determining the character of that deposit? Or, are both removed from a healthy condition before the product is deposited?"

In answering the first question in the affirmative, Mr. Phillips displays, as indeed he does everywhere, great research both literary and experimental. The two following sentences contain the final results of his own investigations:

"I have examined the blood in sixty-seven instances of scrofula, and although I have almost always observed a considerable deviation from the condition of healthy blood, the changes have not presented sufficient uniformity to induce me to regard any particular condition as specially characteristic of scrofula; the changes are such as seem to belong to a tolerably extensive group of affections, all, it is true, being connected with disordered nutrition and debility . . . . I am not, therefore, in a condition to point out any particular state of the blood which is certainly characteristic of scrofula; but it is certain that if we examine the blood when the marks of scrofula are evident, its altered condition is also evident; but I know no particular condition of the blood which clearly marks the existence of scrofula. However, the occurrence of similar changes, at the same time, in different regions of the body, seems to point to some other than a local agent, as producing the change in the organ in which the deposit is made, and that agent the blood." (pp. 57-8.)

The "summing up" is given more decidedly than is usual with Mr. Phillips, for, like many other philosophical inquirers, he has oftentimes kept us oscillating between two conflicting opinions, and left us at last in possession of neither.

"So much may, I think, be fairly assumed, that the blood is changed before the deposit is made, that the accumulation of certain morbid materials in the blood constitutes what is known as the scrofulous diathesis or constitution, and that their deposition in the subcutaneous lymphatic glands constitutes what we know as scrofula." (p. 89.)

*Are pulmonary tubercles, or phthisis, and scrofula in their nature identical?* This is the question Mr. Phillips has placed at the head of his sixth chapter. He answers it in the negative. In doing this, we are quite certain that he virtually opposes the opinion usually current amongst practitioners; but it is due to Mr. Phillips to say, that he has entered fully into the subject, that he has diligently sought for precise and numerical evidence, and that if he fail to convince his readers (as he has certainly failed to convince us) it is not for want of industry and research. We will let Mr. Phillips speak as much for himself as possible. His plan of inquiry is thus laid down:

"Our attention will first be directed to the character of the product deposited in phthisis and scrofula, and the condition of the part in which the deposit is made: we will then proceed to ascertain whether the circumstances under which the deposit is made are identical; whether it occurs at the same period of life, and whether it fall with equal severity on both sexes; whether, where one of these affections is largely prevalent, the other keeps pace with it; and whether those who die of phthisis bear in their bodies the marks of scrofula; because if in most

of these respects a kind of antagonism between these affections can be demonstrated, a conviction must then be induced, that how strong soever may be the resemblance in the general characters of those diseases, the points of difference are certainly not unimportant. And even if we should come to the conclusion, that in their nature these affections are identical, it might still be imprudent to lose sight of *scrofula*, as a term well known, to indicate a particular form of tubercular disease.

"First, we will consider the physical and chemical characters of the two products; and we will take tubercular matter, such as is found in the lungs, on the one hand, and *scrofulous* matter, such as is found in the subcutaneous glands, on the other, for the purpose of the comparison. I do this because we are considering whether *phthisis*, which I prefer to regard as a tubercular disease of the lung itself, and *scrofula*, which I define to be a disease of subcutaneous lymphatic glands, be in their nature identical. I exclude, therefore, from the present inquiry, disease of the mesenteric, the bronchial, and other glands existing in the thoracic or abdominal cavities, because I wish to consider *phthisis* and *scrofula* under their most decided characters." (pp. 61-2.)

We must really put in an initial *caveat* here. We can fairly require that words and terms be used in a precise and definite meaning. "I define *scrofula* to be a disease of subcutaneous lymphatic glands;" such is the precise meaning of the term *scrofula* as used in this chapter by Mr. Phillips. But is that the meaning in which it is used by pathologists and practitioners, or even by himself? We may most aptly quote Richard Wiseman, and appeal to the practitioners of this nation, whether physicians or surgeons, desiring them to say, whether there be not many tumours and ulcers commonly judged to be the evil, which are contained in no cystis at all; neither do they concrete into a glandule. We may also appeal to Mr. Phillips himself with the most perfect certainty of agreement, or else why has he placed "*scrofulous* bones or joints" amongst "*the marks of scrofula*"? More than two thirds of the cases of *scrofulous* disease of joints we have examined presented no enlargement of the subcutaneous lymphatic glands, so that if we adopt Mr. Phillips's definition, these cases so generally understood as *scrofula* ought not to be considered *scrofulous*.

We do not make these observations for the purpose of mere cavilling; they are necessary to the proper estimate of Mr. Phillips's statements and facts. In reality he has had it in intention to discuss the question generally on the wider basis, and we shall consider his facts accordingly; but in discussing the details, he first takes the more narrow definition and then the wider. For example, the pathological anatomy of *scrofula* as compared with that of tubercular *phthisis*, is limited to the morbid state of the subcutaneous cervical glands and the morbid product deposited in their tissue; there is no reference made to the pathological anatomy of other glands ordinarily decided to be the recipients of *scrofulous* deposit, as the bronchial, Peyerian, or mesenteric, nor of the inguinal and other subcutaneous glands. Neither have we any notice of the anatomy of *scrofulous* joints, when the *scrofulous* product is compared with tubercular deposit in the lungs. But in discussing the general pathology of the two diseases, Mr. Phillips adopts the wider definition of *scrofula*—that wider definition, indeed, on which the public and hospital statistics of the disease are founded.

It is but justice to Mr. Phillips, however, to state that he is himself aware of some discrepancy in his views in this respect, since he observes



that "in its narrowest shape, the question is this, are tubercular disease of the lung and scrofulous disease of the subcutaneous glands identical in all other respects than in the seat of the deposit?" Yet even this is indefinite, and cannot be allowed to be an accurate exposition of the question. We must distinguish between the morbid change and the resulting symptoms; unless such distinction be rigidly made confusion is sure to result. The general symptoms of scrofula, taken rigidly in the meaning given to the term by Mr. Phillips, can only be those which result from softening and suppuration of the subcutaneous cervical glands in which there is a deposit of scrofulous matter; the general symptoms of tubercular phthisis can only be those of suppuration of the lungs in which there is a deposit of tubercular matter. In the latter case we know what these symptoms are; in the former we do not know, neither has Mr. Phillips informed us. The question that heads this sixth chapter is a significant example of this inaccuracy; "are pulmonary tubercle or phthisis and scrofula in their nature identical?" We understand from the text, as well as from the context, that the term pulmonary tubercle and phthisis are used convertibly; but it is known quite well that pulmonary tubercle may exist without the series of phenomena generalized under the term phthisis pulmonalis; phthisis may be latent, just as the scrofulous deposit may be present in the subcutaneous gland, unaccompanied by marasmus and hectic fever, or, in short, by any other appreciable pathological change in the organism. In following Mr. Phillips through his argument we shall endeavour to act upon the distinctions we have thought it necessary to make, and thus more clearly elucidate the subject itself.

*Is there any difference between the scrofulous deposit in the subcutaneous cervical glands and the tubercular deposit in the lungs?* No very remarkable difference is perceptible by the naked eye, except such as may be dependent upon the difference of structure of the two organs. There is no essential difference in their chemical elements. Sandras and Albers believed that they detected a microscopic difference between the two products, but Mr. Gulliver controverts their statements. He says:

"In the human subject, it appears to me, that crude tubercular matter, from whatever organ obtained, differs as little in its microscopic as in its general and chemical characters. The drawing [a plate is appended to the work] shows how nearly the microscopic elements composing crude tubercle of the lungs and of the lymphatic glands agree. All the objects are magnified about six hundred and eighty times, linear admeasurement. In the upper part, *a*, is depicted tubercle from the lung of a man, aged twenty-four, who died of the disease; in the lower part, *b*, tubercle from a subcutaneous gland of the neck of a boy, aged nine, who died of scrofula affecting the superficial glands, membranes of the brain, and the spleen; his lungs contained only a few small tubercles." (p. 64.)

The result of Mr. Phillips's own judgment on the facts and statements investigated by him is, that in so far as concerns the physical and chemical qualities of scrofulous and tubercular matter, we can, at present, draw no distinct line of difference between those products.

*Is there any difference in the part in which the deposit is made?* Mr. Phillips says there is. The scrofulous deposit is preceded by inflammatory action; tubercular deposit is not preceded by inflammatory action. In this respect he insists there is a marked difference between the two, and



that it is an important difference. The proposition is open to considerable doubt, but if it be granted, it can only be granted as a specific difference dependent upon the difference in structure between the lymphatic glands and the lungs; the deposit itself and the diathesis in which that morbid product originates may be identical. Mr. Phillips decides in the negative as to any difference as to vascularity of the morbid products: neither can be injected.

*Difference as to the period when scrofula and phthisis destroy life.* Here Mr. Phillips uses the term scrofula in the wider sense, and adduces the mortality tables to prove "that the generally received opinion is correct, that the ravages of scrofula, where it destroys life, are most severely felt before, those of consumption after, the period of puberty." In the metropolis during the year 1840-1, 21 per cent. of the persons who died of consumption were under fifteen years of age, while 60 per cent. of the deaths from scrofula were under that age. A comparison of other districts in England exhibits similar results, and induces Mr. Phillips to state:

"The proof, then, seems to me ample, that the period of life when the deaths from phthisis are most numerous, is not that when the ravages of scrofula are most keenly felt; and this constitutes another distinction between these diseases. I know that it is said that the registered deaths from scrofula are no proof of the extent to which it prevails, but such reasoning would also apply to phthisis. Complete evidence they may not furnish; but as approximative evidence, they are unexceptionable. If the deaths from scrofula in a given district be 100, and in another district of similar population 50, I conceive it would be abundant proof of the greater prevalence of the disease in the former district than in the latter; though it might not show how far enlarged glands could be detected in either case." (p. 71.)

We have considerable doubts as to the scientific value of the facts on which Mr. Phillips founds his numerical estimate. By "deaths from scrofula," he cannot mean deaths from scrofulous disease of the subcutaneous lymphatic glands, for it is not probable any person has ever died from that disease; nor can he mean deaths from diseases in which enlargement of the gland is a pathognomonic sign. We know positively that the term scrofula is used by medical practitioners in a much wider sense than this; and as defined in the 'Statistical Nosology,' published by the Registrar-General, it includes chronic abscesses of all kinds, as psoas abscess, abscesses of the skin not glandular, and suppurating joints of every kind. If we adopt these latter affections into the definition of scrofula, then scrofula is "a disease of the constitution," and Mr. Phillips has stated no solid grounds in proof of his proposition, that a deposit of scrofulous product in the cervical glands is the only pathognomonic sign of this affection. The old maxim of *ubi irritatio ibi fluxus* is substantially true, and we can have no difficulty in conceiving that the scrofulous matter already circulating with the blood may be deposited in other glandular and parenchymatous structures than those of the superficial cervical glands. Indeed, seeing that the fluid from whence the product is derived pervades the whole system, it seems altogether unreasonable to limit its deposit to such comparatively unimportant structures as the subcutaneous glands of the neck. On the contrary, we may fairly assume from a consideration of all the laws regulating the deposits of nutritive, excretive, or

morbid matters from the blood, that such exclusive deposit does not take place.\*

Assuming, then, that the true nature of scrofula is a dyscrasy, or morbid condition of the blood, and that the scrofulous product is deposited from the latter, we shall have a family of scrofulous diseases differing in their pathological anatomy and their semeiology, according to the sex, the age, and the organ or structure in which the scrofulous product is deposited. Considering the question in this way, we are decidedly of opinion that the mortality tables cannot afford data for a scientific inquiry into the pathology of the two forms of disease in question. We shall, however, subsequently show that the deductions they do afford when properly estimated are altogether opposed to those of Mr. Phillips; we think they prove the identity of the two diseases, rather than their distinctness. We may here state, that the difference as to the sex of the victims of phthisis and scrofula is also in favour of our views. The identity of the two diseases by no means demands that they should affect both sexes with equal severity. The sexes are under different predisposing circumstances. In early childhood and youth, the greater activity of the boy renders the joints more liable to those injuries which so frequently induce scrofulous disease in them. On the other hand, in the pubescent female, the energy of development is expended rather in the pelvis than the thorax; being the converse of what happens in the male, and hence we should anticipate more cases of pulmonary deposit in the former sex than in the latter. Now this is precisely the result of Mr. Phillips's statistical inquiries. By data obtained from the Registrar-General's Reports for 1839-42, he shows that there was a preponderance of female mortality from phthisis in England and Wales of 15 per cent., but that the mortality of males from scrofula (that is, *scrofulous diseases*) exceeded that of females by about 24 per cent. In Ireland, the deaths of females from phthisis exceeded those of males by 13 per cent., but the deaths from scrofula (*scrofulous diseases*) were fewer by 35 per cent.

The difference in the relative prevalence of the two diseases is illustrated in a very striking manner by Mr. Phillips. But here again we must caution our readers as to the meaning attached to the terms "scrofula" and "phthisis" or "consumption" in the statistical nosology; the former we again assert comprises a *class* of diseases and the latter includes other forms of chronic pulmonary disease than tubercular phthisis. With this *caveat* we subjoin Mr. Phillip's facts in his own concise way.

\* M. Louis expresses an opinion of this kind so forcibly, respecting the deposit of tubercular matter, that we are induced to quote it: "Whenever these products present themselves, I have never observed them in any viscus without their being likewise present in the lungs. I speak, be it remembered, of observations made on individuals who had passed their fifteenth year. Whence the apparent conclusion that their existence in the lungs forms a necessary condition for their development in other parts. There is another fact which appears further to demonstrate the reality of this connexion between the lungs and other organs, in respect of tuberculous development, or at least the agency of some one single cause acting at the same time on a great number of parts of the body. It is that, with the exception of a solitary case, I have always found the tuberculous deposit in a state of greater advancement in the lungs than elsewhere, and that, when tubercles existed in various other parts of the system besides the lungs, they had in all those others attained the same amount of development. Now, it would be difficult to conceive this uniformity in parts the most remote, and the most different in structure from each other, without admitting the action of a single cause, acting simultaneously on a great number of parts, and quite independently of the accidental influences which we may in some cases be induced to recognize." (*Researches on Phthisis*. Sydenham Society's edition, p. 153.)

"The Registrar-General's Tables for England and Wales, if we take a period of four years, show that the deaths from consumption amount annually, on an average, in round numbers, to 59,500; those from scrofula to 1200; the proportion which the one bears to the other is as 1 to 50; and the proportion they severally bear to the gross population as 1 to 265, and 1 to 13,255 for England and Wales. If the causes of the two diseases be the same, that proportion should obtain in each district; let us see whether it does. In the north-west district, the deaths from consumption are 9976, those from scrofula are 116; the proportion which they bear to each other is as 1 to 86, and to the gross population as 1 to 206, and 1 to 17,782. Here, then, we see that, at the same time, the causes of consumption are in action with more, whilst those of scrofula exhibit 20 per cent. less, intensity than the average of England and Wales. If we take the town population selected by Mr. Farr, in page 198 of his fifth Annual Report, including four years, ending in 1842, we find a population of 3,759,186. The deaths from consumption are to the population as 1 to 235, while those from scrofula are as 1 to 20,000. If we take his counties, in the same page, we get a population of 3,446,501. The deaths from consumption amount to 1 to 286, those from scrofula to 1 per 10,000, or 100 per million. The deaths from consumption being 19 per cent. greater in the town than in the country districts, while those from scrofula are 100 per cent. less.

"The evidence to be obtained from the mortuary tables of our own country proves, therefore, that if the causes of the two affections are the same, the effects differ most widely; that where there is a large mortality from consumption, there is a small mortality from scrofula, and *vice versa*." (pp. 72-3.)

The antagonism as to the prevalence of these diseases (or rather two forms of the same general disease) is further demonstrated by continental statistics and facts. In India consumption is rare; scrofula rife; so too in Russia. In the latter country, and also in Austria and Bavaria, those who bear about their necks ugly badges constituted by scrofulous ulceration are supposed to be exempt from consumption. This fact is remarkable, and Mr. Phillips is of opinion, that it shows that no necessary connexion is thought to exist between the two diseases; but may it not point out an intimate connexion? May not in fact it be, that the scrofulous diathesis once passed safely through will not readily occur again? Is it possible that one attack of scrofula may, like an attack of smallpox or scarlatina, afford a greater or less immunity from another? There is nothing at least outrageously improbable in the supposition.

It is remarkable that some facts stated by Mr. Phillips on this point are in favour of this view. In opposition to the objection that the registries do not show the amount of scrofula in a district because scrofula does not commonly destroy life, Mr. Phillips observes that if the sufferer from scrofula usually dies of phthisis there should be the marks of the former in the bodies of those who die of the latter. Now, we think this is by no means a requisite, so far at least as regards the traces of previous suppuration of the cervical glands, for we hold that (uncomplicated with any other form) to be the simplest and most harmless species of scrofulous disease. The facts, however, though collected to elucidate an opinion which we cannot well admit, are not the less valuable, and they show that a previous attack of scrofula in so far as it is indicated by marks, does not predispose to a second attack. Of 332 cases of phthisis examined by Mr. Phillips at the St. Marylebone Infirmary, only 7 presented scars resulting from scrofula!

In seven of the 332 cases just mentioned there was tumefaction of the

cervical glands, but in only two instances did they contain scrofulous matter. On this point of the pathology of phthisis and scrofula Mr. Phillips quotes M. Louis, who, in 350 phthisical bodies carefully examined by him, discovered a tuberculous condition of the cervical glands in only 35. From these and similar facts, Mr. Phillips draws another argument against the identity of scrofula and phthisis. We cannot, however, admit its conclusiveness. In scrofulous diseases of the joints we have enlarged cervical glands in probably not more than 30 per cent., yet Mr. Phillips would not we think argue that no diseased joint can be pronounced scrofulous unless the cervical glands also are affected with the scrofulous deposit. If he do, the *whole* of his statistical returns are invalidated, and of no worth.

We should be better able to say why the lymphatic glands in cases of tubercular phthisis and scrofulous diseases of the joints are not usually the seat of scrofulous product, if we were better acquainted with the circumstances which determine its deposition. In the class of diseases alluded to, it is not improbable that the morbid condition of the organs implicated determines a vicarious action in them, which prevents deposition in adjoining structures. Such an action is seen in the metastasis of arthritic and rheumatic diseases, and may occur also in scrofula. Thus, according to Hasse, whose deductions are made from 245 examples of phthisis, the bronchial glands are tuberculous in only 26 per cent., while the mucous membrane of the larynx and trachea are the seat of *tuberculous* (not "scrofulous") deposit in only the same proportion as the cervical glands, namely, 5 per cent.! The *class* of tissues may also exercise an influence in determining the deposit.

It appears then that the tubercular deposit in the lungs and the scrofulous deposit in the subcutaneous glands do not differ either chemically or physically; that the difference in the semeiology of tubercular phthisis and scrofula is solely dependent on the difference in the class of organs attacked; and that the diversities as to age, sex, and character of the population attacked are caused by a difference in the *predisposing* or *exciting* causes; the *proximate* cause being the same in both, and consisting in the deposit of a similar morbid product from the blood.

*Prevalence of scrofula in Great Britain.* An opinion is very generally entertained that scrofula is pre-eminently an English disease. Mr. Phillips addressed himself to the task of determining how much of truth the opinion might contain. He had a large number of children examined in schools, union houses, factories, and elsewhere. Of 133,721 examined, in many districts 24½ per cent. presented the marks of scrofula (scrofulous disease) indicated in Mr. Phillips's form of return. But the gross number of cases in which the disease so marked was obvious to the eye amounted to not quite 8 per cent. In union houses the proportion was rather greater, in charity schools rather less. All things considered, Mr. Phillips thinks "that the proportion of children between the ages of five and sixteen who present marks of scrofula evident upon a simple inspection amount to as near as may be, but rather under 8½ per cent." The proportion of adults is much less. Of 1521 examined, only 23 had such marks. Of cases of diseased children treated at dispensaries and hospitals, Mr. Phillips estimates 4 per cent. to be cases of scrofula, and of the gross number of

persons treated about  $1\frac{1}{2}$  per cent. are scrofula. Of 95,586 recruits examined 1 in 119 were rejected for marks of scrofula. We subjoin Mr. Phillips's summary on this point; but we observe, by way of caution, that the number of persons exhibiting external and visible signs of scrofula is not an index of the prevalence of *scrofulous diseases*, but of the prevalence of those forms only which affect external organs. We may add too that all the children *under* five years of age are taken no note of.

"It is thus seen, that though derived from so many and such different sources, there is a striking concurrence in the results of the evidence I have collected, and that agreement constitutes a strong reason for believing that my data do very nearly represent the actual prevalence of the disease. We see that the returns of cases of scrofula, found among our ordinary population, are singularly confirmed, not only by the returns of hospitals and dispensaries, but also by the examination of recruits and convicts; and I think we are thus justified in regarding as near the truth our estimate of the prevalence of scrofula, such as we have defined the disease. That is to say, that scars are apparent in about  $1\frac{1}{2}$  per cent.; that the subcutaneous glands are enlarged, so as to be perceptible on simple inspection, in less than 3 per cent.; and that the glands may be detected by the finger in  $24\frac{1}{2}$  per cent. of those of the children of the poor who are under sixteen, and in 8 per cent. of those above; or taking the whole population, in 10 per cent.; and that something less than 3 per cent. of the people are under treatment for the disease in its various forms." (p. 84.)

*Unequal prevalence of scrofula.* Mr. Phillips's extensive returns have enabled him to compare the state of the population in different portions of the British Isles as regards the prevalence of scrofula, and the result of his inquiries is, that it prevails very unequally. In some districts, 72 per cent. of the children of the poor presented marks of scrofula, while in others the proportion was so low as 11 per cent.

"If we take two agricultural districts, the Eastern, which comprises Essex, Suffolk, and Norfolk, and the South Western, which includes Wilts, Devon, Dorset, Somerset, and Cornwall, we find in the former that the deaths from scrofula are 11·8; and in the latter as 10 to every 100,000 population, being a difference of nearly 20 per cent. In the purely manufacturing districts of Lancashire and Cheshire, the proportion is only as 5·6; while in that of Yorkshire as 3·5 per 100,000; and in the mixed districts of Gloucester, Hereford, Shropshire, Worcester, Stafford, and Warwick, it is as 7·9 to 100,000.

"The Irish census returns, including as they do a period of ten years, show a mortality from scrofula of 1 in 25,252 of the population; but in the different provinces there is considerable variety. Thus in Connaught, it is 1 in 21,177; in Ulster, 1 in 31,399; in Munster, 1 in 23,363; in Leinster, 1 in 29,025; but to this mortality should be added much for marasmus. In the case of the Irish returns, the deaths from marasmus are very large, amounting to 6,665 annually; but as many of these may have been cases of scrofula, the absolute value of the returns, in so far as concerns consumption and scrofula, is much lessened.

"The infirmary and dispensary returns in Ireland show a proportion of scrofulous cases amounting to 82,746, of these 1290, or 1·5 per cent. are registered scrofula. The examination of children in Limerick gives a proportion of 5·4 per cent. having the ordinary marks of scrofula. And the total deaths comprised in Dr. Griffin's paper were 2918, those from scrofula 7.

"The recruiting returns, for the United Kingdom, show an average rejection of 8 per 1000 for marks of scrofula. In England the smallest number of rejections for scrofula occurred in London, where they amount to 5 per 1000. In Scotland, in the Edinburgh district, 16 per 1000; in Ireland, in the Dublin district, 13 per 1000." (p. 87.)



Mr. Phillips thinks these statements constitute a near approximation to the truth. The caution which preceded our last quotation applies also to this.

The prevalence of scrofula (as indicated by external marks) in other countries, as compared with England, present some interesting points of comparison. In the Orphan Asylum at Lisbon (as we learn from the returns procured by Mr. Phillips) Dr. Rosas examined 800 children, of whom 279, or 35 per cent., bore the ordinary marks of scrofula. Of the boys the proportion was 50 per cent. of the whole number examined; whilst of the girls it was only 10 per cent.; a fact remarkably in accordance with the principle we laid down (p. 136) that the identity of two diseases by no means requires that they should affect the two sexes with equal severity. In the Orphan Asylum at Amsterdam, of 495 children, 209, or 42 per cent., bore the ordinary marks of scrofula. At the Orphan Asylum, Munich, the Report shows that scrofula, at one time, affected two thirds of the children, but under improved diet and air the disease had lessened in frequency. The Vienna returns show that the number of children boarded

in the Imperial Royal Orphan Asylum was  $\frac{308 \text{ boys}}{104 \text{ girls}} = 412$ ; of these, during the year 1841,  $\frac{18 \text{ boys}}{27 \text{ girls}} = 45$ , or 11 per cent., came under treat-

ment for scrofula. The return made by the President of Police at Berlin, Von Puttkammer, of the condition of the children who were within the walls of the Frederick Orphan Asylum, shows that the number was  $\frac{230 \text{ boys}}{123 \text{ girls}} = 353$ ; of these, 125 boys and 50 girls had enlarged glands, 4

boys and four girls had scrofulous ulcers, 2 girls had scrofulous joints; making 185, or nearly 53 per cent. At St. Petersburg, of 840 children examined, 343, or nearly 41 per cent., bore evident marks of scrofula. At the Imperial Foundling Hospital, at Moscow, the number of children examined was 15,515; of whom 1294, or only 9 per cent., are reported as presenting tumid glands, ulcers, and sinuses resulting from scrofulous swellings and diseased joints. This proportion, Mr. Phillips observes, is very small; and that trifling enlargements have been passed over is rendered probable by the fact that the number presenting scrofulous ulcers and sinuses, and diseased joints, is greater than the number which has been reported to possess tumid glands, the latter being 583, and the former 711. There is, therefore, little doubt but that in the whole of the cases the disease was evident to the eye, and, if so, the proportion was very large; and even if we take the 711 cases only in which we know the disease was apparent to the eye, the proportion is large—nearly five per cent.

The returns from America varied greatly, and are evidently fallacious.

"That from the Boston House of Industry includes  $\frac{98 \text{ boys}}{48 \text{ girls}} = 146$  children; of whom 106, or 70 per cent., are returned with the specific marks of scrofula. That from Philadelphia, carefully supervised by that able physician, Dr. S. Jackson, shows a total of  $\frac{2641 \text{ boys}}{1357 \text{ girls}} = 3998$  school children examined; of whom  $\frac{10 \text{ boys}}{3 \text{ girls}} = 13$  are returned as scrofulous . . . . . Taking the population of New York, in 1840, at 345,000 in round numbers, and taking the deaths from scrofula as reported, at the



average of 35 years, at 4278, we find that the annual deaths from scrofula bear to the total population a proportion of 1 to 1241. In Philadelphia, the proportion is 1 to 453; while in London, in the same year, the proportion is, in round numbers, as 1 to 17,500." (p. 88.)

The prevalence of scrofula in Syria is less marked than in Great Britain. Dr. A. Jackson states that he thinks in India 80 per cent. of half-caste children are scrofulous, 50 per cent. of natives, 40 per cent. of English, and 10 per cent. of Mussulmen.

From the mortuary tables of Paris and Geneva, Mr. Phillips gathers that the mortality from scrofula is much greater in those cities than in our own. Taking a series of years, the deaths from scrofula in Paris amount to 1 in every 3221 of the population; in Geneva, to 1 in every 2790; in the year 1842, taking the population at 61,871, and the deaths from scrofula at 16, the proportion is 1 in 3867, while in London, taking four years ending in 1842, they amount to about 1 in 9,000.

We think that before full reliance can be placed upon these statistics, it ought to be shown that the nosology of the registrars in the two countries is similar. If tabes or marasmus of infants be entered as scrofula in Paris or Geneva, the statement will assume a very different aspect.

Taking the whole of France, the rejection of conscripts for marks of scrofula are 2 per cent., or 1754 in obtaining 86,000; our own proportion is 1 in 119. The rejections for scrofula in the Département Du Nord are 46 per 1000; in the Eastern Pyrenees, 1 per 1000. Mr. Phillips infers that for the whole of France the marks of scrofula presented by recruits are twice as many as among our own recruiting population. This conclusion, if it means that scrofula is twice as prevalent in France as in England, is certainly erroneous. The strength of the French army is kept up by a forced conscription, and not by recruiting, and, consequently, the circumstances under which the two classes of military candidates are presented for approval are totally dissimilar.

Mr. Phillips finally inquires, "Is it not, then, abundantly proved that the notion that scrofula is eminently an English disease is incorrect? and am I not warranted in stating that there is no country, so far, at least, as our information extends, in which the people are more free from the disease than in England and Wales?" We scarcely can grant it "abundantly proved," or even proved at all. There is a fair presumption, however, that the fact is so.

Mr. Phillips devotes a chapter to the question whether scrofula be now more prevalent in England than formerly. He enters into some statistical details, which must have cost him much trouble in collecting, and some of which are quite original, especially those respecting the numbers "touched" by royalty. Mr. Phillips is of opinion that scrofula prevails much less extensively now than in the 17th and 18th centuries.

*Causes of scrofula.* We now come to a much discussed question,—What are the causes of scrofula? They have been sought for in hereditary transmission, contagion, defective alimentation, imperfect aeration, deficient clothing.

*The hereditary cause.* The doctrine that scrofula (or scrofulous diseases) may be transmitted hereditarily is founded upon both theory and experience. No one can pay ordinary attention to the physiology of propagation of

animals without noticing that the most minute characteristics of parents are transmitted to their offspring. Hence it is fairly argued that if any of the structures are so constituted in the parents, that scrofulous products shall be formed on a sufficient exciting cause occurring, such a constitution may be certainly expected in the offspring, as certainly as the characteristic form or individual features.

Observation has shown, too, that it is not always the features of the parent, but often those of the grand-parent, that are reproduced in the offspring. Such facts are well known to the breeders of domestic animals, who term the process "breeding back." Hence it has been inferred that the morbid predispositions of the grand-parent, and not those of the parent, may be reproduced in the offspring. The inference that a morbid predisposition of parent, or grand-parent, may be transmitted to offspring, is founded upon the axiom that the whole is equal to the sum of all its parts; that as every line and fibre that make up the parent are represented in the offspring, so the qualities of every tissue will be represented.

It has been usually granted that those characteristics which mark the species, and are natural to it, are transmitted to the offspring; but it has been doubted whether acquired characteristics, or acquired predispositions, can be transmitted. But of this fact there can be no doubt: the pups of well-bred pointers will point like their parents, before they have received a lesson. Examples of a similar kind might be multiplied. The fact of hereditary transmission of acquired instincts, or predispositions, is certain; it only remains to determine the limits within which the law operates, and which are as yet indeterminate.

Holding it, then, as established that a predisposition in the tissues to disease may be transmitted, it remains to determine whether a disease with which one or other of the parents was affected at the time of conception of the offspring, or the mother during the period of utero-gestation, can be transmitted. This is certain; but the limits within which this law operates are also, as yet, indeterminate. We know that smallpox and syphilis may be transmitted under these circumstances; and it is not improbable that examples of transmission of other diseases may be detected on more minute observation, and particularly of those diseases which are dependent on an animal poison. But we think that it is not necessary to the production of scrofula, or of any other constitutional disease in the offspring, that one or both the parents should have been suffering from the disease at the time of conception of their progeny. There are grounds for believing that a previous impregnation at a time when one or other parent was actually diseased, will exercise an influence on subsequent conceptions. A seven-eighths Arabian mare, belonging to the Earl of Morton, had her first foal by a quagga; subsequently, she had three other foals by a black Arabian. Now, the first two foals of these three by the Arabian had a striking resemblance to the quagga in the markings of their coat and in the form of their manæ. Similar facts might be adduced.

On a careful perusal of Mr. Phillips's observations on this point, as respects scrofula, we feel inclined to believe that he has carried his numerical scepticism too far. Probably too great a stress has been laid upon the hereditary transmission of diseases, or the predisposition thereto, but the fact of such transmission is based on a much higher order of proof than

fallacious statistics. Even in reference to insanity, observes Mr. Phillips, "the case of all others in which the truth of the hereditary influence might be most easily tested, and in which the conviction of its existence is, perhaps, the strongest, I know no conclusive evidence." But has Mr. Phillips carefully looked for it? It must be observed that in an investigation of this kind positive facts must be admitted to have a most preponderating weight. "Of 191 patients admitted into Bethlehem Hospital, in 1844, an hereditary cause could only be discovered in 26 cases." Such is an example of the facts on which Mr. Phillips grounds his scepticism. At best it is only negative. But is not such a small proportion (so decidedly at variance with all experience) a warranty rather for doubting the acumen of the observer, than the known and long-cherished dogma? Of 415 insane persons admitted into the Retreat, near York, since its foundation, and connected with the Society of Friends, the disease was connected with an hereditary predisposition in 142 cases, or in the proportion of 1 in 3. And of the remaining 273 cases there were, doubtless, some in whom the hereditary predisposition existed, although not made out. Now, unless Mr. Phillips is prepared to assert that insanity is a natural and spontaneous disease in one of every three families of the Society of Friends, such a connexion between it and the hereditary predisposition must be something more than a mere accident; there must be a causal relation. With regard to scrofula, our author has instituted some important and extensive inquiries; an account of which we subjoin:

"The means which I have taken to acquire accurate data as to the extent to which hereditary causes operate, in the propagation of scrofula, are the following: I examined myself, and procured to be examined by others, in the metropolitan, the factory, and in rural districts, upwards of 2000 families, each consisting of from three to five children, and living, as nearly as may be, under similar circumstances. In one portion of the cases, both parents were apparently free from scrofulous taint; in another portion there was reason to think that both parents were tainted; in another, that the father was tainted; and in another, the mother. The number of families examined was 2023, the number of children was 7587; and the number bearing such marks of scrofula as I have already indicated was 1738, or nearly 23 per cent. In 506 instances, derived from many localities, and under the most varied circumstances, both parents were apparently untainted, and their offspring amounted to 2021. Of these, 421, or something less than 21 per cent., presented marks of scrofula. In 276 instances, there was reason to think that both parents laboured under scrofulous taint; their offspring amounted to 1092 children; of these, 271, or nearly 25 per cent., bore the ordinary marks of scrofula. In 589 instances, the father carried about him marks of having suffered from scrofula, whilst the mother was free from them; their children amounted to 2107, those having marks of scrofula to 483, or nearly 23 per cent. In 652 instances, the mother bore upon her person the marks of scrofula, whilst the father did not; their children amounted to 2367, and of these, 563, or nearly 24 per cent., presented marks of scrofula.

"In glancing over these results, it must be kept in mind that the offspring of the tainted, on the one hand, and of the untainted, on the other, are not intended to represent their relative fecundity, for means were taken to collect only such families as were represented by not less than three, nor more than five children. . . . . I do not pretend to regard these results as an accurate representation of the influence of scrofula when existing in the parent to reproduce itself in the child. I would even admit that, as the cases were seen with many eyes, the data may be more defective than if they had been the result of one person's examina-

tion; but however defective they may be, they are the only approach I know of to a reasonable amount of evidence, to enable us to judge how far it is probable that scrofula in the parent will reproduce itself in the child. And from that evidence it would seem that in children subjected after birth to similar circumstances, the hereditary influence does not appear to be exerted beyond 4 per cent. This result is in opposition to two parties—one maintaining that the disease is always hereditary, and never acquired; the other, that no diseases are hereditary, but that they are always the result of circumstances which come into action after birth." (pp. 118-20.)

We fully participate in the sentiments expressed by Mr. Phillips as to the imperfection of the data here given. The whole question is so complicated that its solution by such simple numerical results is not to be expected.

*The question of predisposition.* We judge from the context that Mr. Phillips questions the actuality of an hereditary predisposition to scrofula. We cannot say that we participate in his doubts, although he is quite correct in stating that it is difficult to test numerically. It may even be doubted if the proofs can ever be derived so clearly from statistical inquiries as from ratiocination on observed facts. We see the children of highly scrofulous parents, who are not simply of a weakly constitution, but who are truly scrofulous, and who become so, although placed under the most favorable circumstances for prophylaxis and the preservation of health. Such families come occasionally under the notice of most practitioners, and furnish extreme instances of the law of hereditary predisposition. This predisposition to scrofulous deposit has its analogue in the hereditary predisposition to arthritic deposit. As in the highly scrofulous families, so with highly gouty families, we find the hereditary predisposition develops the disease, in spite of the most careful prophylaxis. This is a matter of fact. We have ourselves carefully noted it. But in all these examples of predisposition to a particular family of diseases, whether they be the neurotic, the arthritic, or the strumous, the predisposition varies in every degree from the two extremes. The scrofulous predisposition may be so slight as to be developed into scrofula when the causes which excite it into activity are so powerful that they would develop like diseases in an untainted constitution; or the proclivity to scrofulous deposit may differ in different organs in the same individual, so that, although in childhood the lymphatic glands may escape all disease, at puberty the lungs will be inevitably involved.

*Will any other disease in the parent develop scrofula?* There can be no doubt that a delicate constitution favours the development of scrofula, whether that be derived from parents, or be a deteriorated constitution. Long continued indigestion in a parent seems to favour the development of scrofula in a child, as well as the other causes enumerated by Sir James Clark, whose opinion Mr. Phillips controverts. In doing this, he observes that the marks of scrofula do not commonly appear during the first two years of life. This is scarcely, we think, correct, even if the marks of scrofula be allowed to be none other than those which Mr. Phillips has laid down, but decidedly incorrect, if we consider disease of the mesenteric glands a mark of scrofula. We are more in accordance, however, with our author in his views as to the independence of scrofula and syphilis; indeed we fully concur with him. It appears to us that much error has arisen with regard to the relationship between the two diseases, from the fact, that persons

of scrofulous constitutions suffer severely from secondary syphilis, especially from syphilitic affections of the throat and bones. This probably results from the circumstance that the same class of tissues is the seat of both diseases.

Mr. Phillips thinks the children of old men are more likely to suffer from scrofula, but principally because they are weakly, and not because there is any power in the debility of age to transmit the scrofulous diathesis. He also thinks that too much injurious influence has been attributed to intermarriages; at least they do not tend among healthy persons to the production of scrofula.

*Can lactation by a scrofulous nurse develop scrofula in the child?* or can it be communicated by contagion? or by inoculation with pus taken from a scrofulous person? Mr. Phillips answers all these questions in the negative. It does not seem probable *a priori* that vaccination from a scrofulous child will communicate scrofula to a healthy one, but it is far from improbable; indeed we should feel inclined to think it certain that vaccinia, or variola, may exercise such powerful influence on the mucous tissues as to develop the disease, if it do not already exist; or (paradoxical as it may appear) to remove it, if it do. We know very well that many remedies display this antagonism of action, and we need not deny the same mode of action to morbid poisons. It need not, however, be granted that variola and pulmonary tubercle mutually repel each other (as Rilliet and Barthez think), because there has been a larger proportion of phthisical cases since vaccination was practised; it is a more reasonable inference that by an improved hygiene, scrofulous children have been enabled to survive until puberty, then to die of phthisis.

*General causes of scrofula.* These are specially considered under the heads of food, air, climate, humidity, temperature, occupation, &c. In discussing the question of defective nutrition as a cause of scrofula, Mr. Phillips refers extensively to the statistics of foundling hospitals, especially those of the Continent, to demonstrate that the nature of the food administered to infants has a most important influence on their health; and that the bringing up a child "by hand," as it is termed, conduces to a high infantile mortality. If the food be not suitable to the infant, or if it be taken in an unnatural manner, it acts injuriously upon nutrition. It appears, indeed, that a larger proportion of scrofulous disease is found among children who have been brought up by hand than among those who are suckled by a mother, or even a foster-mother; and that, therefore, the system of bringing up children by hand deranges nutrition, and adapts the constitution for the deposit of scrofulous matter in the child.

In order to test the influence of food on the development or prevention of scrofula, Mr. Phillips collected extensive materials for a comparison between children in union workhouses, or in certain charitable institutions, and a similar class not so well fed at their own homes. The author's summary of his results is as follows:

"To ascertain how far scrofula prevails in union workhouses, 9342 children, the inmates of 63 houses, in different districts were examined; and of these 2139, or nearly 23 per cent., presented marks of scrofula.

"In a large town I examined 784 children, in endowed and other schools, who, for the most part, had been well cared for before admission, and judiciously ma-



naged afterwards; and the 'marks of scrofula' could be detected in 127 instances, or 16 per cent.;—500 children in national schools, brought up at home, of whom the most part were not well fed and lodged, and of these 164, or 32 per cent., bore similar marks of scrofula. And 500 children examined in the workhouses yielded 126, or 25 per cent., bearing marks of scrofula.

If we compare the children in the St. Marylebone workhouse with the children of the national school in High-street, St. Marylebone, there will be no question of the physical superiority of the former; for while the children of the workhouse are affected with scrofulous swellings to the extent of 26½ per cent. only, those of the national school are affected in the proportion of 32 per cent.

In the same districts with those of the 63 union-houses, 22,704 children living at their own homes, or in institutions other than poorhouses, have been examined; of these 8,353, or 37½ per cent., had marks of scrofula.

The dietaries of union workhouses, as contained in the Poor-law circular, having been compared with the food of the agricultural labourer, and the prevalence of scrofula in each condition having been shown, we arrive at this result, namely, that the workhouse child is better fed, and less subject to scrofula than the child reared in the cottage of the peasant; and when it is considered that many pauper children are received into the workhouse when suffering and destitution have probably developed disease, and have almost certainly produced debility, the comparison is even less favorable for the child of the independent labourer, than the result which is shown by the numbers actually examined. Now, with the exception of food and clothing, the workhouse child enjoys no advantage of which the child reared in the cottage is deprived. The rooms he inhabits may be larger and better ventilated, but a greater number of persons is collected in these rooms, and the breathing space for each person may even be less than in the narrow limits of the cottage." (pp. 171-3.)

The result of Mr. Phillips's inquiries in Ireland, on this point, and amongst the schools both on the Continent and in the United States, are detailed also at length.

*An impure atmosphere as a cause of scrofula.* Mr. Phillips thinks it due to his subject to enter upon a discussion of the question how far the overcrowding of the population in large towns is detrimental to health. We do not think our author handles his statistics cleverly in this section. For example, he argues that if density of population be assigned as an exclusive cause of the excessive mortality of towns, it should be found to act uniformly, and the mortality should be in a direct ratio to the density. But it is not so. The metropolis has a denser population than Bristol or Leeds, yet the mortality is less. Now a little consideration might, we think, have led Mr. Phillips to the conviction that the term density of population is extremely vague. It is well known to practical inquirers into the sanitary condition of the people, that the indefinite use of the term has led to the greatest mistakes. A population may be crowded into narrow courts, and be as densely packed as possible, yet because of the surrounding superficies being surrounded by factories, warehouses, streets, squares, &c., the number of people *per square mile*, may be inconsiderable. The plain truth is, that the attempt to ascertain the effects of overcrowding, or of uncleanness, or of poverty, from the bare statistics of the public registers, is altogether vain. The more experience we have of statistical deductions, the more convinced we are of the multitudinous fallacies to which they give rise, except when they are thoroughly sifted, and studied in all their relations, by persons conversant with the particular details from which the



data are taken. That pure air is necessary to health we know certainly; that the air of an apartment occupied night and day by a family, situate in a confined court, surrounded by high buildings, and ventilated from an atmosphere impregnated with smoke and exhalations from sewers and graveyards, must be impure, and therefore injurious to health and life—is a proposition the truth of which cannot be doubted. To what degree it is injurious no one can possibly state numerically; the feat is simply impossible. We, therefore, think it unnecessary to follow Mr. Phillips through his long chain of statistical evidence on this head. We think the pith of the whole may be put into a nutshell. It simply amounts to this, that in those districts in which there is an excessive infantile mortality, fewer sickly children survive to die of scrofulous disease; that is, to die of scrofulous disease of the articulations, &c. So that, in fact, the greater the number of deaths from scrofula, the more healthy we may conclude the district to be. But this inference must be received with divers modifications.

*Influence of climate.* The belief that one climate is more favorable to life than another is universal, and is no doubt well founded, although no one has demonstrated (or, we believe, can demonstrate) the fact numerically. The salubrity of climates is relative. The adaptability of constitution to climate is an important element in the question. The Hindoo perishes prematurely in England; the European sickens or dies prematurely in India. Mr. Phillips can make out no facts which show that light, electricity, humidity of the atmosphere, or temperature, have any influence on the development of scrofula.

“When considering the prevalence of the disease, it was shown that we have no proof that climate, whether the temperature be high or low, variable or uniform, or the atmosphere be dry or humid, has any very obvious influence of itself, in producing or preventing scrofula. At St. Petersburg, with a mean temperature of 3·23, and a general mortality of 3·770; and Moscow, with a mean temperature of 3·6, and a general mortality of 4·010; and Iceland, where the Centigrade thermometer in winter indicates 20 minus—there appears to be less scrofula than at Lisbon, with its temperature of 71·2, or than at Amsterdam, Berlin, or Calcutta. So at Madeira, with its high mean temperature and low range, there is as much scrofula as among the juvenile convicts in Parkhurst prison. Other causes than climate must, then, in all these countries, exercise a most important influence in producing the disease; and among the causes of scrofula, we have seen that [innutritious] food holds the first place.” (p. 221.)

*Employment as a cause of scrofula.* In considering this question, Mr. Phillips devotes his principal attention to the litigated question of the influence of factory labour in developing scrofula. He has secured several valuable and extensive statistical returns from the manufacturing districts; the substance of which we subjoin in his own words:

“In Leeds, Dr. T. Smith very kindly examined, at my request, 1095 children, employed in different factories, and examined 548 children of the same class, not employed in factories. The result is, that those not employed in factories exhibited marks of scrofula in 8 per cent. more instances than those whose days are spent in such establishments. In Manchester and other great manufacturing towns, a similar result has been obtained by examinations made at my request, and on so large a scale that there is every reason to feel confident in the opinion already expressed. Again, from his Dispensary practice, Dr. Smith has made me the following report:—‘Of 916 persons, between seven and fourteen, the children

of factory operatives, but not themselves employed in factories, 365, or 39 per cent., had enlarged glands, and 75, or 8 per cent., had scars resulting from scrofula. Of 567 persons, all under twenty-one, and employed in factories, 124, or 22 per cent., had scrofulous scars.'

"Mr. Poyser, the intelligent surgeon of Winksworth, kindly examined for me the people employed in Mr. Arkwright's mills, and the following are the results which he communicated:—'Persons examined, 798; total number having marks of scrofula, 29.'

....."I have obtained, through the kindness of Messrs. Horner and Saunders, the results of the examination of 6754 factory children, from which it appears that marks of scrofula were found in only 905 instances, or only 13½ per cent. The returns of Mr. Fereday, Mr. Davis, and that of other friends, who have kindly made a comparative examination of a large number of children, exhibit similar results; and they leave no doubt on my mind that children employed in factories are more free from scrofula than the average of children in England and Wales." (pp. 231-2.)

Dr. Martin furnished returns from the Mayfield Cotton Factory, situate in the parish of Clonegan, county of Waterford. The village is within a few hundred yards of the factory, and contains a population of 3075, of which 1061 are employed at the works. The following is a tabular result furnished by Dr. Martin as to the prevalence of scrofula amongst this population :

	External population, 2014.	Factory operatives, 1061.
Hip disease . . . . .	6	2
Knee " . . . . .	7	0
Elbow " . . . . .	4	0
Wrist " . . . . .	1	1
Ankle " . . . . .	1	2
Ulcers (scrofulous) . . . . .	15	2
Phthisis . . . . .	10	8
Ophthalmia . . . . .	7	4
	<hr/> 51	<hr/> 19

In investigating the evidence furnished by the registries of deaths as to the influence of factory towns, Mr. Phillips found that, in England and Wales, the deaths from scrofula, as compared with the total population, are as 7·6 to 100,000. In rural districts, the proportion is 9; in town districts it is 5. In factory towns, having a population of 2,043,038, the proportion is 4. In non-factory towns, having a population of 2,870,416, the proportion is 5 per 100,000. The alleged greater insalubrity of the employment in the woollen factory districts, as compared with the linen and cotton, so decidedly maintained by several authors, has not been confirmed by the inquiries Mr. Phillips has entered upon. In the linen and cotton districts, included in a table contained in the appendix, and having a population of nearly 1,000,000, the gross mortality is 2·786: that from consumption, ·477, or 1 in 209; that from scrofula, ·004, or 1 in 24,872. In the woollen districts, in the table, and comprising a population of nearly three quarters of a million, the gross mortality is 2·242: that from consumption, ·396; or 1 in 252; that from scrofula, ·005, or 1 in 17,877.

We do not think much reliance can be placed on these statistical deductions, although the data on which they are founded have no doubt been collected with great care and accuracy.

The question of the influence of factory labour on the sanitary condition and strength of the population divides itself into two main branches. First, how great is the influence exercised by it on the operatives? Secondly, how great is the influence exercised on their offspring? With regard to the first question, we may observe, that the proportionate number of persons actually working in factories having the marks of scrofula, or suffering from scrofulous disease, is no evidence of the influence of factory labour on the production of the disease, because the greatest sufferers would not be able to follow their work. A very large proportion of those engaged in this kind of labour might die in consequence of their occupation, and still the results of an inquiry like that instituted by Mr. Phillips would be favorable. We might go into a well-ordered factory, and say with Mr. Hamilton, "Gad! we find the cotton factories a specific for scrofula," if we extended our inquiries no farther, just as we might have reviewed the British army on the Sulej after the late battles, and say, "Gad, the battle has been a child's play, there's nobody seriously hurt!" Now as we would go to the military hospitals for the actual casualties of the campaign, so we should go to the homes of the sick workers, or trace them to the hospitals, or to the grave. The table supplied by Dr. Martin, of Mayfield, is defective in this point; it does not tell us how many of the "external population" had been employed at the works, or how long their period of service was; and particularly, it does not tell us how many of the persons suffering from scrofulous diseases in that population had been employed there, and for what period. It is certain that the injurious influence of factory labour is slow in its operation, and consequently an extended period of time should be one of the elements of the investigation.

With respect to the second question,—what is the influence of factory labour on the health and strength of the children of the operatives (we do not mean those children employed in the factories specifically), we may again observe, that these statistics are fallacious. The prevalence of scrofula amongst children under 5 years of age is not touched upon by Mr. Phillips's statistics. Further, the mortality of children under this age is excessive in the town districts as compared with the rural. According to Mr. Farr's tables, in Surrey, at the age of five, only 20 per cent. of those born at the same time have died, while in Liverpool the proportion is 47 per cent. Now it is not easy to state numerically how many of the children in manufacturing districts are cut off by *tabes mesenterica*, and other diseases of a scrofulous character, in early life, but there can be no doubt that the proportion is much greater than in the rural districts. It is also certain that the more numerous deaths during the earlier years will weed the population, as we have already shown, of many sickly lives, which under the more favorable circumstances of a country life would survive long enough to die of such diseases as would be entered as scrofula in the register. To give any thing like trustworthy accuracy to statistical deductions as to the influence of factory labour on the health of the population in general, and on the development of any particular disease, it would be necessary to select a number of children the offspring of factory labourers; ascertain the deaths at each age; follow up the history of those who live long enough to be employed in the factories; note the diseases from which

they suffer or die, and when arrived at puberty and married extend the inquiry to *their* offspring. Such an inquiry would, of course, involve a laborious and lengthened investigation, but we really think the known laws of hygiene forbid the cautious inquirer to draw positive conclusions as to the influence of factory labour from less extensive data.

Mr. Phillips's inquiries are certainly corroborative of the views we provisionally advocated a few years ago in our Fifteenth Volume, in opposition to the general prejudice then prevalent against the gigantic manufacturing system. Those prejudices, however, were, we believe, to a considerable degree justified, because the greater attention to the sanitary condition of their manufacturing population which characterizes many master manufacturers is only of comparatively recent development. Some of the new factories are splendid palaces, compared with the older places of labour. In short, the popular prejudice is founded on what was, and not exactly on what is. We would observe, however, that there are yet factories in which the modern hygienic appliances have not been so extensively adopted as in the newer structures; so that this inquiry to be rigidly accurate, should not be confined to the latter, but extend to all. It is certainly more convenient to make inquiries in the newly-built, large, and well-regulated factories, but it is precisely in those in which the evils of the factory system are most diligently obviated, and its advantages developed. Such an example, we presume, is the Mayfield works, which Dr. Martin assures us are very carefully regulated as to temperature and crowding.

*Treatment.* We now come to a more congenial, because a less controversial, subject. Mr. Phillips considers the treatment of scrofula under two heads, the prophylactic and the therapeutic. The prophylactic treatment insists upon well-assorted marriage as an important aid in preventing the occurrence of scrofula. Then the dietetics of infants should be regulated; they should be supplied plentifully with fresh air; and should be encouraged in all the active exercises in which young animals generally delight; and the games should be spontaneous, and left to the guidance of their own instincts. Mr. Phillips objects to infant schools as constraining young children in this respect. He observes:

"I know that the health of those infants, who are suffered to amuse themselves as they please during the day is better, *cæteris paribus*, than that of those children who have been for many months regular attendants at infant schools. And the reason of the difference I apprehend to be this, that in children the blood is vigorously circulated through the entire frame, by means of the exertion of the muscular system, and this exertion of the muscular system can only be maintained by providing such amusement as will keep the body in motion. The listless walk around the school-rooms, though repeated many times a day, will not quicken the heart's action, and will not warm the hands and feet. And so long as the hands and feet, and the surface of the body remain cold for many hours of every day, so long the child will have congestion of some internal organs, and a state of permanent disease is readily induced; digestion is ill performed, nutrition is defective; and if this state of things be long continued, scrofula may be the consequence." (p. 251.)

We entirely agree with Mr. Phillips in these statements, and have always denounced infant schools conducted on the plan here referred to. But we believe, at the same time, that any plan involving such manifest disregard

of all hygienic laws constitutes no necessary part of the system of infant schools. On the contrary, we believe that they may be made as directly conducive to bodily health, as to the regulation and improvement of the mind. And we are happy to know that some infant schools are so conducted.

We cordially agree with the following opinions, quoted from a communication by Mr. Turnbull, who reported on the Austrian School at Neustadt.

“ Amid modern theories of education, and which prevail in other countries besides Germany, few perhaps are more particularly erroneous than the system which would be always teaching something ; always in every form of play seeking to impart instruction. The gymnastics and equitations at Neustadt become thus as completely matters of study, and are probably performed with as much gravity of attention as the task of mathematics, or of history, because they are performed under the eye of the teacher. In the inaptitude of youth for any long continued application, nature herself points out the expediency of alternate repose to the mind, of entire vacancy of thought ; but man too often endeavours to counteract this wise disposition by ever endeavouring to engage the attention by some new object of instruction. The animal spirits, those delightful harbingers of health and energy, mental and corporeal, are stunted in their very spring when the boy is debarred from those alternations of idle, thoughtless independence in his sports, which is not less essential to the formation of his future character than the practice of his severer studies. The mind is frittered away by the multitude of pursuits, and filled with a number of crude and confused ideas. It becomes paralysed by over-work, or precociously and morbidly active by over-excitement. A being of dull and orderly correctness may be produced by such discipline ; or the memory may be overcharged (to the probable ruin of the reflecting power), so as to delight unthinking relatives with the multitude of acquired ideas ; but as the lad was wanting the freshness of youth, so he will probably in after years be without the vigour of manhood.” (p. 254.)

*Curative treatment of scrofula.* Mr. Phillips devotes several pages to the history of the cure of scrofula by the royal touch, in which much literary research is displayed. He thinks that the popular notion as to the remedial powers of the monarch were founded on *post hoc ergo propter hoc* evidence, the fallacy of which we have taken some pains to expose and denounce. Wiseman declares he was “ a frequent eye-witness of many hundreds of cures performed by his Majesty’s [Charles II] touch alone, without any assistance of chirurgery, and those many of them such as had tired out the endeavours of able chirurgeons before they came thither. It were endless to recite what I myself have seen,” &c. Wiseman was a courtier ; is it an unfair imputation on his character to add, that he knew his part well, and played it well ? Mr. Phillips observes :

“ The truth is, that of the scrofulous patients who suffer from enlarged glands in the neck, nine out of ten do get well under almost any rational plan of treatment. The tendency to improvement is most remarkable in spring and summer months, those being the seasons of the year at which a large portion of the persons touched by Charles II. were presented to his most gracious Majesty, and who, whether touched or not, might be reasonably expected to be much better by the end of summer. Ignorant as even many professional persons were at that time of the natural history of the disease, it was not surprising that they should attribute

the cures which followed the touch to the ceremony itself. But with increased knowledge, we may form other judgments, and can, without presumption, refer those cures to other causes than the imposition of the king's hand." (p. 269.)

*Iodine.* Mr. Phillips thinks its virtues have been much overrated, and observes that the 35 cases reported by Lugol as cured out of 105 treated, would have been as well cured without it, if no other means than suitable food, air, clothing, and exercise, had been employed, and the season had been favorable. He further states :

"In my own practice I have exhibited every form of iodine extensively in cases of scrofula, and supposing the patient to remain exposed to the influence of the same conditions in which the disease was first manifested, and the period of the year to be that which has not been found favorable for the cure of the disease under other modes of treatment, I cannot say that I have reason to estimate the curative powers of iodine so highly as many others have done. I know that, among the out-patients of hospitals, whose circumstances remain unchanged, and who apply at the latter end of autumn or the beginning of winter, we may often exhibit iodine in every form for weeks or months without producing any sensible amelioration in the patient's condition. I know also that at the beginning of summer, a patient similarly affected and similarly treated, will, often in a few weeks, exhibit a marked improvement—but how much of this should be referred to iodine? How much to season?" (p. 275.)

Mr. Phillips, in common with all experienced practitioners, objects to the use of the simple tincture. He has found the iodide of iron a useful tonic, given in syrup, to the amount of four grains in the twenty-four hours. In whatever form the remedy be administered, he thinks it imprudent to continue its use longer than a fortnight, or three weeks, at a time. "At the end of that time the preparation should be set aside, aperient medicine should be employed, and its use should then be resumed with the same precautions. In this way, any virtues which the medicine possesses are more surely brought out, and the inconveniences sometimes experienced from its administration will be, as nearly as possible, avoided."

We have known numerous instances in which a scruple of the hydriodate of potass was taken daily for many weeks without any perceptible effect. The only rule we have for its discontinuance is the development of catarrhal symptoms, which in some people supervene on its administration even for a short period.

*Barium*, or rather the *chloride of barium* (the old muriate of baryta), is a remedy, in Mr. Phillips's opinion, little inferior to iodine :

"Barium, however," he observes, "seems to be a more certain stimulant than iodine, or, rather, we might say, irritant; and, in my judgment, its use is clearly contraindicated where there is much free inflammatory excitability of the system. But in those cases where the tallow-like complexion, the pale tongue, and the languid circulation, accompanied by irritability of the mucous surfaces, are present, the virtues of the barium are often very remarkably demonstrated. I usually give it in solution, a grain to an ounce of distilled water, with ten drops of compound tincture of gentian. Of this solution, I commence with half an ounce twice a day, and on no occasion have I exceeded three grains in the day; and up to this moment I have not experienced any check in the administration of the medicine." (p. 282.)

The *hydrochlorate of lime* is the basis of the anti-scrofulous nostrum of



**Nieumann.** Mr. Phillips has frequently used it in the following form: a drachm to twenty drachms of distilled water, of which a teaspoonful was taken in milk two or three times a day. He has carried the dose up to two teaspoonfuls, but has not exceeded that dose. He is not convinced that it has any very evident action on scrofulous glands, but in moderate doses it is more generally tolerated than chloride of barium.

The alkalis are of doubtful value; so also burnt sponge.

*Cod-liver oil* is useful, Mr. Phillips thinks, rather as an animal oil, than because it contains infinitesimal doses of iodine or bromine:

"The conviction on my mind is, that when good is derived from it, it is to be referred to the effect of the animal oil in improving digestion and nutrition, rather than to the presence of iodine; and if Popkins's impression be correct, that he has observed quite as much good to follow the daily use of fried bacon in such cases, and if it be further true, which I by no means admit, that butchers, oilmen, tallow-chandlers, tanners, and other persons who are continually coming in contact with fatty matter, are particularly robust and well nourished, and are known to be remarkably free from scrofula, then the case in favour of the oily principle is so much the stronger. If the impression be correct, that cases are occasionally presented in which the good effects of cod-liver oil are remarkably apparent, and if the amelioration seem to concur with a much improved condition as to nutrition, I conceive myself justified in assuming that the one is the consequence of the other without being required to frame a theory why animal oil improves nutrition." (p. 288.)

*Sea water and sea bathing.* Mr. Phillips is full of doubts as to the merits of these remedies. Change of inland air and cold bathing may be equally, if not more, useful. Some years ago, an experiment as to the value of sea air was made in nine obstinate cases of scrofula, in the St. Marylebone Infirmary. They were sent to Margate, and remained there several months.

"On their return their condition was carefully ascertained. The glandular tumours had very nearly, but not completely subsided; of the sinuses one was dried up, the other nearly so. In the former two cases of joint affection no amelioration was experienced in the local disease, but the general health was improved; the third patient, after remaining at Margate some weeks, came home to die.

"The eight surviving children had returned home at the close of the Margate season. In the succeeding November, three of them were again under treatment: one with glandular tumour, which had re-appeared; one with a re-opened ulcer; the other with a single sinus, still discharging. These results are, I conceive, in perfect accordance with the experience of careful observers; and certainly they do not tend to support the prejudice which exists in favour of the superior efficacy of sea-side residence in cases of scrofula. That those ganglia in which scrofulous matter had been deposited should continue foci of irritation is natural, because I know no satisfactory evidence that scrofulous deposits are ever absorbed. They, therefore, continue to exist, and, like any other foreign body, may excite new irritation from very slight causes. That a sinus should heal when properly treated, provided all the scrofulous deposit be removed, is to be expected; but whether the sea-side did much to determine the subsidence of irritation around the gland in one set of the cases above noticed, or to heal the fistula in the other, is with me a matter of doubt." (p. 293.)

*Mineral waters* are in as little estimation with Mr. Phillips as sea bathing. We subjoin some therapeutic statistics of French mineral waters used for the cure of scrofula, together with Mr. Phillips's remarks. The

facts are derived from the reports of the inspectors of mineral waters in France :

**" SCROFULOUS ENLARGEMENTS.**

	Cases observed.	Cures.	Ameliorations.	No benefit.	Deaths.
Bourbonne . . . . .	20	2	16	11	0
Balaruc . . . . .	46	14	16	14	2
" . . . . .	13	0	5	8	0
Mount d'Or . . . . .	19	3	5	11	0
Nérès . . . . .	4	0	2	2	0
Bagnols (Lozère) . . . . .	78	17	38	23	0
Bagnols de Luchon . . . . .	41	14	10	17	0
	<hr/> 230	<hr/> 50	<hr/> 92	<hr/> 86	<hr/> 2

**ABSCESSSES, ULCERS, FISTULÆ.**

Bourbonne . . . . .	132	57	62	13	0
Bourbon l'Archambaut . . . . .	43	18	15	10	0
	<hr/> 175	<hr/> 75	<hr/> 77	<hr/> 23	<hr/> 0

"The above table implies, I believe, as favorable an estimate as can properly be made of the influence of mineral waters in the cure of scrofula, and certainly the result does not show that they exercise any very decided curative influence over the disease.

"That they have been more indebted for the credit they possess to the enthusiasm of friends, than to the faithful register of the cures which it is alleged, have resulted from their employment, is, I think, true. And no doubt M. Patisier was near the truth when he said, 'Les eaux minérales naturelles guérissent quelquefois, soulagent et consolent toujours.' " (pp. 299, 300.)

The season of the year has, in Mr. Phillips's opinion, much influence in the development and amelioration of scrofula :

"I have fully satisfied myself that scrofulous cases are most numerous and most aggravated in spring and the beginning of summer; that they are least frequent and most ameliorated at the commencement or the middle of autumn; and that at the one period they have been aggravated by the cold of winter, at the other they have been ameliorated by the warmth of summer; and this has happened when all other influences have been apparently unchanged.

"When do we send patients to the sea-side? Precisely at that season when they would improve any where. When do we find any remedies best succeed? Precisely when the season is becoming favorable. When do we find all medicinal agents comparatively powerless? Precisely when we get no help from the season. True it is, we may take a poor child from the streets in the depth of winter, and give him good food and lodging, and his disease will be improved, and this without reference to the season; but then we have improved nutrition. But let a poor child remain at home, and we shall usually find every specific fail to improve his condition until favoured by season; and let almost any one of the unsuccessful remedies be used when the end of spring comes, and its apparent good effects will soon be obvious enough. Now, if that position be correct, it must be evident how defective the estimate of every plan of treatment is which does not include the influence of season." (p. 301.)

The *Appendix* contains many of the returns *in extenso*, which Mr. Phillips so industriously obtained from a numerous and wide circle of correspondents. One of the first of these documents is a short essay by Professor Albers, on the difference between scrofula and tubercles—the

latter term being used to signify not only tubercles of the lungs, but also phthisis. In looking over this we could not avoid remarking how completely Professor Albers had mistaken the point at issue. The differences which he lays down are simply differences in semeiology, and not in pathology—differences which arise from a difference in the organ affected, and not from a difference in the nature of the two diseases. The symptoms of disease of the lymphatic glands and of the lungs must necessarily differ; the one is a “noble organ,” intimately connected with vital action, the other of secondary importance.

In concluding our review of Mr. Phillips’s work, we feel we cannot estimate too highly the industry and perseverance with which he has pursued his researches into so important a subject, and we think the mass of valuable facts he has collected gives such a value to his work as will secure it a place amongst the important class of “books of reference.” We trust Mr. Phillips will not only continue his researches, but will also adopt a still more comprehensive plan. He should, we think, carefully avoid narrowing in any way the field of his inquiry. *Scrofula* should be investigated as “a disease of the constitution.” It should be traced step by step through each class of organs, and as well in their histological constitution as in their successive development from infancy to old age. The prosecution of the inquiry on this philosophical basis may raise a monument to his fame, which shall match if not overshadow the labours of Louis on a kindred subject, and possibly merge them into a higher generalization. The scrofulous diseases of the glandular structures of infancy might form one group; of youth another; of puberty another; of middle age another; of old age another. And although the main inquiry should be directed to the glandular and parenchymatous organs, the cutaneous cellular tissue, and the structures imbedded in the epidermis should not be forgotten. There would thus be a large family group of diseases investigated in their general and mutual relations, comprising not only mesenteric disease, scrofulous diarrhoea, enlarged submental and cervical glands, and scrofulous articulations, but also scrofulous disease of the gastro-enteric mucous membrane and its diverticula into the Eustachian tube, salivary glands, liver, kidneys, &c. (constituting obscure forms of indigestion, &c.); scrofulous hydrencephalus, hypertrophy of the lips, alæ nasi, and general subcutaneous tissue; scrofulous ophthalmia, cutaneous eruptions, abscesses, &c. We are convinced that anything short of a comprehensive and philosophical inquiry like this—of which the above is a meagre outline—will be insufficient to determine the litigated questions connected with the pathology and relations of *scrofula*, or materially advance the treatment of tubercular diseases in general.

## ART. XII.

*Traité des Maladies du Sein, comprenant ses Affections simples et cancéreuses.* Par J. CARPENTIER-MERICOURT, M.D.—Paris, 1845.

*A Treatise on Diseases of the Breast, both simple and cancerous.* By J. CARPENTIER-MERICOURT, M.D.—Paris, 1845. 8vo., pp. 312.

THIS book professes to contain a practical exposition of the pathology and treatment of all the affections to which the breast, in females and males, is liable. It is sufficiently plain and unpretending in character, and contains scarcely a single fact unknown to the well-read in the literature of these diseases; yet the volume may claim the merit of presenting a compendious and concise account of the subjects with which it deals. From place to place we find a passage for some motive or other worth notice; and of such passages we proceed to give brief intimation.

*Number of mammae.* Numerous cases of anomaly of number of the female breast have been reported by writers in this country; but we were not aware that the unfortunate Anne Boleyn (wife of Henry VIII) had three mammae, and six fingers to each hand, until we found the statement in M. Méricourt's compilation.

*Sore nipples.* The severe ulcerations produced in the nipple of nurses from suckling an infant affected with syphilis, may, the author says, "undergo cancerous degeneration, if they be kept up by stimulant regimen or ill-directed treatment, and if *the female be predisposed to cancer.*" This we believe to be a correct statement; but it would have been well had the author added, that in point of actual fact the necessary combination is extremely rare, inasmuch the origin of cancerous ulceration of the breast in a syphilitic sore is itself extremely rare. His notions concerning the origin of cancer are not always just however; for we find him elsewhere (p. 30) talking of "little *lipomatous tumours* degenerating into cancers."

Not a work reaches us from Paris without exhibiting fresh proofs of the literary ignorance of the French: here we find this author talking of "MM. Tanchou and Eguisier" as the persons who have drawn attention to kiestein in the urine as a sign of pregnancy!

*Swelling of the mammae.* When swelling of the breasts follows suppression of the menstrual discharge, itself dependent on the formation of tumours in the uterus, secretion of milk has, according to M. Méricourt, been known to occur. We have not met with any example of the fact, and wish the writer had furnished us with his authority for the statement. He insists much on the practical importance of a knowledge of the "sympathies of the breasts," as treatment may sometimes be effectually directed on the indications thus furnished. He observes, "one of the best methods that can be employed for the suppression of uterine hemorrhage consists in the application of large cupping-glasses to the breasts, so as to determine a salutary fluxion towards those organs." We have no experience of the practice; but can conceive its being useful as an *auxiliary*.

*Hypertrophy of the mammae.* Turning to the section on hypertrophy of the breast we find another example of the combined ignorance and petty

larceny, which it grieves us to be so often obliged to notice in the productions of the Parisian press. M. C. Méricourt divides hypertrophies of the mammæ into two classes,—those of hypertrophy of rather acute course, coinciding with the establishment of menstruation, and those of slow course, connected with certain disorders in the functions of the uterus. The merit of this division (such as it is) he attributes to “M. Aug. Bérard,” who used it in “the remarkable essay which signalized his admission into the Faculty of Medicine as Professor of Clinical Surgery.” But the truth is that the Professor of Clinical Surgery is no more the author of the arrangement of these diseases than the man in the moon; the arrangement is appropriated or adopted from the essay of Fingerhuth, which said Fingerhuth’s essay, be it observed, M. Méricourt refers to more than once.

*Milk abscess.* In describing the treatment of milk abscess the author avows himself in strong terms a partisan of early opening with the lancet, no matter how deeply seated the suppuration may be: he conceives that delay leads to the risk of “exterior ravages” on the part of the pus. He is well aware that some surgeons do not interfere until the purulent fluid has actually made its way to the skin; and that others hold that these abscesses should be allowed to open spontaneously. He admits that by following his plan several abscesses may require to be opened one after another, but maintains that there is an inconvenience much less serious than the risk incurred of ulterior injury to the mammæ by the undermining and destructive influence of the accumulating pus. “I do not conceal from myself,” adds the writer, “that it requires a deeply-felt conviction to induce one to act in this manner; for patients, more prone to ingratitude than to the contrary, are ready to believe that their medical attendant is to blame, if several abscesses appear, and require opening, one after another; they accuse him either of having allowed all these successive accumulations to take place, or of having opened the first badly.” We cannot too highly commend the ethics of M. Méricourt which lead him to prefer what he conceives to be the real advantage of the patient, to the furtherance of his own interests in families. But we differ from him on the point of practice; we are advocates of the single late incision (this is not a case in which a “stitch in time saves nine,”) and have not seen the mischiefs arise which he appears so seriously to apprehend, though we are aware such have sometimes occurred. He speaks of numerous incisions into the breasts, as though no physical suffering or danger of any kind attended their performance,—a conclusion to which the incised woman, on the one hand, and the records of unfortunate cases, on the other, would offer very strong demurrer.

*Erectile tumours.* Under the head of erectile tumours of the breast the author relates from a French journal a very singular case (no matter how the narration be understood), of which the following may be taken as a condensed account. A woman lost her infant four days after its birth; she then had her breasts suckled by a new-born lamb. After the lapse of a few days the latter appeared ill, and violet stains, like flea-bites, appeared on its lips. Subsequently on the seats of discoloration small mam-millated fungous excrescences, bleeding on the slightest touch, grew up. Under the continued application of vinegar, oil, and salt, mixed together, these little fungi blackened and fell off. The animal used the woman’s

breast for a month. But the curious part of the story is that after the suckling had been going forward for a fortnight, and the animal had been ill for a week, the woman perceived on the areola of the left breast some spots of violet colour, from the centre of which sprang seven little bodies about the size of a pin's head. These rapidly became fungous, enlarged, and bled on the slightest injury. On the 17th of January (three weeks after the loss of the infant) three other little pimples appeared on the *right* breast; on the 24th, the little productions on the *left* breast equalled a pear in size. Leeches, compression, the application of calcined alum, opiate cerates, &c. were employed, but to no purpose; indeed the fungi on the *right* breast, uninterfered with, had increased much less rapidly. On the 9th February, M. Nozaran, of Montpellier, found a cauliflower-shaped mass, about 43 lines broad at the broadest part, and 20 at the pedicle, formed chiefly of veins, but containing some minute arteries and nervous filaments. The patient's general health was good; but the local disease visibly increased day by day. A ligature, and next the Vienna caustic were applied; the mass on the right breast disappeared under these means; that on the left resisted them, and was eventually cut out, cicatrization following rapidly. The case as we said is curious; but our interpretation of the facts would not accord with that of M. Méricourt. The vegetations were to our minds probably syphilitic in origin, and the woman tainted the lamb, and not the lamb the woman. The application of the term "erectile tumours" to the excrescences is in keeping with the vague manner in which the term is constantly used; no proof exists that a particle of true erectile tissue existed in the mass.

*Neuralgia.* We turned to the writer's chapter on neuralgia of the breast with some interest, hoping (perhaps we had no right to hope any thing of the kind, for how many have laboured in vain on the subject) that some novel and useful suggestion might present itself for the treatment of those obstinate affections. We have been baffled in our own practice in affording any thing but temporary relief in cases when every method ever dreamed of was, we conscientiously believe, employed before cure was despaired of,—with the exception of subcutaneous incisions and *amputation of the breast*. Nor are we disposed to have recourse to either of these practices. Although improvement has occurred from the former, it has neither (as far as we are acquainted with the evidence) been more permanent or more complete than from acupuncture or even the endermic application of morphia. And with respect to amputation of the breast, which has been performed by M. Rufz of Martinique at the eager solicitation of a sufferer from the disease, we hold it to be utterly unwarrantable, unless the operation have been decided as indicated in *consultation*—an infinitely unlikely occurrence. We do not believe that this affection is by any means so frequently connected with that state, unknown in its real nature, to which the name of spinal irritation is commonly given, as it is the fashion of clinical teachers to assume and affirm.

Fragments of foetus, hair, bone, &c. have in excessively rare instances been found in tumours in the breast; and the mode of their first discovery by the patient is of interesting practical significance. Though these tumours *must* in part, from the nature of things be congenital, yet they have not in the few known cases attracted attention until accidental



circumstances (a blow, a fall, &c.) have given an impulse to the growth of the solid matter connected with them. They have been ascribed by the patients wholly and completely to an injury received after they had reached adult years. Does not the fact throw much light on the error of writers and practitioners, who are so ready to admit the statements of patients that scirrhus and other tumours in the breast have been produced by local injuries?

*Cancer.* It is remarkable how fond numerous writers appear of the hypothesis, that the frequency of cancer in certain organs is to be accounted for by the vicissitudes and varying states of function to which those organs are exposed. Our author applies this theory (though, to do him justice, hesitatingly) to the explanation of the well-known frequency with which the mammary gland grows cancerous. The enlargement of the gland at puberty, its still further increase during pregnancy, the phenomena of lactation, the sympathetic swelling and tension of the organs during the menstrual period, and eventually their flaccidity and atrophy are all notable changes which cannot be without influence on disease. Plausible enough is argumentation of this stamp. But we can easily overturn it by availing ourselves of observations employed by Dr. Walshe in refuting a very similar, if not identical, doctrine put forward by another French writer in respect of the uterus. The ovaries are subjected to all the modes of change observed in the mammæ; some of them are even considerably more marked in the former than in the latter organ: now, while 1147 deaths occurred from cancer of the breast in ten years in Paris, 64 only were due to the ovary.

The author's chapter on cancer of the breast is judiciously written; but we have of late been obliged to accord so much space to the subject, that we can afford none on the present occasion; besides there is nothing in the present work which could prove novel to the readers of the late numbers of this Journal. But we must in a few words convey to our readers the practical results at which the author has arrived; they are in some respects, if not in all, distinctly accordant with those to which Dr. Walshe's investigations have led him, as referred to in a previous number. (Brit. and For. Med. Rev. No. XLII., p. 436.) Cancer may be cured by possibility by medical means; the operation may frequently be avoided; early operation is not advantageous, and may even be dangerous and hasten relapses; it should (if practised) be practised *at a late period, when every other means have failed*. This latter proposition is exactly that taught by Dr. Walshe also; we need scarcely remark how completely at variance it is with the common doctrine, according to which the knife cannot be used too early. We have perused the evidence put forward on this subject by our countryman especially with deep interest, and confess ourselves to have been startled at finding how fully our own experience, when closely sifted, confirms the experience he puts forward. Thus, in speaking of extirpation of the cancerous testicle, Dr. Walshe says: "it must to all seeming prove a point of insurmountable difficulty to determine what cases are suitable for operation,—inasmuch as death has very speedily followed the use of the knife, when the general health was unimpaired, the tumour small, the skin free from morbid change, and the cord unaffected." (On Cancer, &c., p. 427.) A melancholy truth of which but too many examples rise in memory's sad array before us.

## ART. XIII.

*Lectures, illustrative of various Subjects in Pathology and Surgery.* By Sir BENJAMIN BRODIE, Bart., F.R.S.—*London*, 1846. 8vo, pp. 412.

THE pen of Sir Benjamin is at all times practical; and, as such, its labours are well appreciated by the profession. Whatever appears under his name and sanction is sure of attentive perusal, by all who care to inform themselves in the current literature of the day; and few, if any, will rise from such perusal without being very sensible of having obtained no inconsiderable amount of profit and instruction.

The Lectures now before us have been delivered at various times; and, through the journals of the day, the majority have already been made public, but in such a detached manner as to render their previous printed existence of but little avail for the purposes of consultation and reference. We rejoice to see them now collected by the author himself, and by him given forth in a solid, suitable, and satisfactory form. For more than one obvious reason, the contents of the volume are unsuited to ordinary criticism; and we believe that we shall best discharge our duty by abstaining from all other notice of them than what is necessary to convey to such of our readers as may have not yet perused the volume itself, a digest of the practical bearing of the prelections.

LECTURES I. and II.—These have much to do with the ethics of the profession, and are chiefly addressed to its youngest members—students entering upon their studies; the first comprehending “the studies required for the medical profession;”<sup>\*</sup> the second, treating of “the duties and conduct of medical students and practitioners.” Both are admirable. Were we responsible for a text-book, or system of surgery, we should be sorely tempted to perpetrate an open act of piracy, and print them *in extenso* as the most suitable of all commencements of a new edition. Of these we can give no abstract; they must be read to be felt and understood. We cannot, however, refrain from giving a specimen, though small, of each.

In the first Lecture there occurs the following paragraph, the moral of which cannot be too strongly urged upon the rising generation. Every one acquainted with medical tuition must be familiar with very many painful examples of the sad results of the neglect of its truthful axiom.

“It is not uncommon for medical students, any more than it is for other students, to engage at first with zeal in their pursuits; then, as these lose the charm of novelty, to become careless and indifferent, and at last, when their education is drawing to a close, and it becomes a question how far they are qualified to undergo the required examinations, to endeavour to make up for the time which has been misspent and wasted, by excessive labour, such as is incompatible with sufficient physical repose and mental relaxation. But it is not in this way that great things are to be accomplished, either in our profession or in any other. Habits of attention which are once lost are not easily regained; and no durable impressions are made upon a mind which is exercised beyond its powers. The slow but persevering tortoise reached the goal before the hare, who was over confident of

<sup>\*</sup> Addressed to the Students of the Medical School of St. George's Hospital; the one in 1838, the other in 1843.

the speed which she could exercise if she were required to do so ; and this fable, which we were taught in the nursery, conveys a moral lesson which the philosopher need not be ashamed to learn." (pp. 5-6.)

In the second lecture there is much sound advice for old as well as young. What more true than this ?

"The self-sufficient, who do not keep before their eyes an ideal standard of perfection, who compare themselves only with those who are below them, will have an advantage with inexperienced and superficial observers; but I must say that I have never known any one to do any real good in the world, or obtain ultimately a bright reputation for himself, who did not begin life with a certain portion of humility. The greatest men are humble. Humility leads to the highest distinction, for it leads to self-improvement. It is the only foundation of a just self-confidence. Study your own characters; endeavour to learn and to supply your own deficiencies; never assume to yourselves qualities which you do not possess; combine all this with energy and activity, and you cannot predicate of yourselves, nor can others predicate of you, at what point you may arrive at last." (pp. 84-5.)

Or this ?

"There is no profession in which it is more essential that those engaged in it should cultivate the talent of observing, thinking and reasoning for themselves, than it is in ours. The best part of every man's knowledge is that which he has acquired for himself, and which he can only to a limited extent communicate to others. You will spend your lives in endeavouring to add to your stores of information; you will, from day to day, obtain a clearer and deeper insight into the phenomena of disease; you will die at last, and three fourths of your knowledge will die with you; and then others will run the same course. Our sciences are, indeed, progressive; but how much more rapid would their progress be, if all the knowledge that experience gives could be preserved." (pp. 38-9.)

Of two things let no member of our profession prove oblivious. First, that his profession both requires him and entitles him to be a gentleman. Second, that there is both a real and a false assumption of "the gentleman" to be found in the world. And if he have any doubt as to which is which, let him turn to page 52 of the present volume and he will then find the following solution of his difficulty.

"It is not he who is fashionable in his dress, expensive in his habits, fond of fine equipages, pushing himself into the society of those who are his superiors in their worldly station, that is entitled to that appellation. It is he who sympathizes with others, and is careful not to hurt their feelings even on trifling occasions; who, in small things as well as in great, observes that simple but comprehensive maxim of our Christian faith, 'Do unto others as you would they should do unto you;' who, in his intercourse with society, assumes nothing which does not belong to him, and yet respects himself; this is the kind of gentleman which a medical practitioner should wish to be." (p. 52.)

LECTURE III. This is "*on the effects of strangulation*;" and it, and the following, though now published for the first time, were delivered in the theatre of the Royal College of Surgeons, in London, as long ago as the year 1821. The subject is physiological, but the object thoroughly practical. And it is obviously most important for every practitioner to be well versant in this matter; as, without a knowledge of the physiological department, he will make but a sorry figure in a witness-box, under the fire of a cross-examining barrister; and without familiarity with the required surgical treatment, he will be worse than useless in the professional emergency.

Strangulation may affect the vertebræ, the blood-vessels, the nerves, the muscles, and the trachea. Even in cases of criminal suspension, at least in this country, it is seldom that the bones suffer, either by fracture or displacement. The inner coat of the carotid has been found ruptured. The veins must always be so compressed as to cause accumulation of blood in the vessels of the brain; and sometimes, though rarely, cerebral apoplexy has resulted from this; but "neither vascular congestion, nor sanguineous apoplexy, is the common cause of death from strangulation." Pressure on the nerves of the neck is not likely to be such as to prove altogether fatal to their function, even for a time. Muscles may be ecchymosed and torn; but that is still more trivial. "There can be no doubt that strangulation causes death by closing the trachea, or preventing respiration; and that whatever other effects it produces are of secondary importance to this." May we be permitted to add, that in the execution of criminals, and in cases of strangulation of a like nature, in which there is any considerable "drop" given to the patient, there is more or less *concussion* inflicted on both brain and spinal cord; which not only tends towards the extinction of life at the time, but proves also a serious obstacle to resuscitation subsequently attempted.

The following is the *mode of death* :

"1. The trachea is obstructed, so that air cannot enter the lungs. 2. The blood passing through the lungs does not undergo that change which respiration produces, and which is necessary to life. 3. Dark-coloured blood, which has not been purified by exposure to air, is transmitted to the left side of the heart, and from thence to the brain and other organs. 4. The heart continues to act, circulating dark-coloured blood, but its actions gradually become weaker, and, in the course of a very few minutes, cease altogether." (pp. 60-1.)

The heart is the *ultimum moriens*; and ceases to act, according to our author, not from insufficient stimulus applied to it by the blood, but from impairment of its nervous energy—the result of black blood poisoning the brain. Mr. Beck has recently observed that the cardiac nerves are accompanied "by processes or elongations of the gray material of the nervous system;" and this confirms Sir Benjamin in his belief that these nerves "possess the property of generating nervous influence," and that on their nervous influence the movements of the heart depend.

*The symptoms of strangulation* follow this course: 1. Discoloration of the lips, and other parts; the result of venous congestion, and of the want of oxygenated blood. 2. Involuntary actions of muscles; convulsive; not accompanied with pain, probably, and caused by "contact of the dark-coloured blood with the brain and spinal cord." 3. The diaphragm and intercostal muscles continue attempts to act, respiratorily,—on an average from a minute and a half to two minutes. 4. When the efforts at inspiration cease, "the pulse of the head and arteries is still distinctly to be felt; and the action of the heart continues of sufficient strength to maintain the circulation for two or three minutes longer." 5. The heart having fairly ceased to act, death is fixed; recovery is impossible.

If the ligature be removed from the neck before the contractions of the diaphragm have ceased, air is drawn into the lungs, respiration is restored, the blood is again oxygenated, and the animal rallies. Also, at a further stage, if during the short interval during which the heart continues to act,

after the contractions of the diaphragm have ceased, artificial respiration be maintained, circulation and oxygenation are continued, and normal respiration is resumed. But the heart having fairly ceased to act, Sir B. Brodie is decidedly of opinion that nothing can restore the animal—that the heart will not again be brought to act so as to circulate the blood.

*Treatment* resolves itself into simple indications; it being remembered, however, there are necessarily but few cases in which the surgeon has an opportunity of hopeful interference. 1. If the ligature is removed before the efforts of the diaphragm have ceased, “all that you have to do is to watch the patient carefully; if natural respiration continue, leave him to himself; if it cease, supply the want by inflating the lungs artificially.” 2. If the efforts of the diaphragm have already ceased, have recourse to artificial respiration without delay. There is no time to lose. In two or three minutes, after the last heave of the chest, the heart’s action will have ceased, and then all hope is over. 3. In successful cases, so soon as normal respiration is established, inflation is desisted from. But treatment is not to cease. The patient is not safe. Dark blood has been circulating in the brain; and symptoms like those of poisoning by a narcotic may exist. Coma may remain. By and by the respiration may cease. Then has arrived a second period at which artificial respiration may be necessary to preserve life. And, in truth, the practitioner may expect to be called upon to inflate the lungs more frequently at this second period than at the first.

As to the *mode of inflating the lungs*, it is obvious that in the hurry and excitement of the emergency, we are not to trust to syringes, bellows, tubes, elastic-gum bottles, or other contrivances which may be constructed very ingeniously and suitably for this express purpose. They are not to be had. And the surgeon is lucky if he can secure a common bellows. In a certain rural and remote district of Scotland, the belated traveller has found that “there is nae wale o’ wigs,” and wisely secured the first that came to hand. And so here. Secure the bellows. And if that cannot be had, look for a tube of any kind which may be inserted into the nostril; a large elastic catheter is very suitable; if this cannot be got, roll up a piece of card into a cylinder, and with this, and one’s own lungs, a tolerably efficient substitute for bellows may be put in play. Then the following practical points require attention: 1. Avoid undue forcing of air into the lungs; otherwise the air-cells may be burst; air may enter the blood-vessels, and the result is certainly fatal. 2. Inflate at proper intervals; imitating as clearly as possible the rhythm of natural respiration. 3. There is no necessity for warming or oxygenating the air; the attempt is just time lost. And it is fortunate that such manœuvres are not essential; seeing that the surgeon is not likely to be provided with either a pocket stove or a portable gasometer. 4. The upper part of the body is exposed; so that the movements of the chest may be accurately noted. 5. The inflating tube is introduced into one nostril. There is no necessity for opening the trachea; that is only required when previous disease has obstructed the larynx. 6. The other nostril and the mouth should not be closed. They are safety-valves, by which over-distension of the lungs is prevented. 7. An assistant presses the box of the larynx against the vertebra, so as to prevent inflation of the stomach. Were this cavity filled, the descent of the dia-



phragm would be prevented, and no air could enter the lungs. 8. Electricity and galvanism are inferior to the bellows, for they waste time in application. And the following assertion—repeating more broadly the opinion already stated—hurries him who believes it to the best employment of the very few moments he has to spare.

*“If that action of the heart by which the circulation is maintained should cease, as a consequence of the suspension of respiration, it can never be restored. This I positively assert, after having made it the subject of a very careful investigation. If others have held a different opinion, it is because they have confounded those feeble and irregular contractions of the heart, which may last for a long time, but which mean nothing, with those regular and powerful movements which are necessary to propel the blood through the system.”* (p. 80.)

In the after-treatment—natural respiration having been restored—it may be necessary to abstract blood, on account of congestion; but this must be done with extreme caution seeing that the powers of life have been brought low by the faulty circulation in the brain. The warm bath is not essential. But the patient should be kept in an atmosphere of a moderately warm temperature, “to compensate for the insufficient generation of animal heat, which is the consequence of the impaired state of the functions of the brain, whether arising from the influence of a narcotic poison, or from another cause.”

LECTURE IV, treats of *drowning*. The lamprey and benito can often vary their existence, pleasantly, by keeping their heads in air; but man cannot follow their example by immersing his head in water with impunity. Death takes place, as if by strangulation; in both cases “the want of due oxygenation or decarbonization of the blood being the sole cause of the animal’s destruction.” After immersion, a deep expiration takes place, by which bubbles of air are expelled from the lungs. Then comes an ineffectual effort to inspire; but water does not enter, instead of air; spasm of the muscles of the larynx seeming to prevent this. The attempts to breathe are repeated several times, and after each attempt a small quantity of air is expelled from the mouth and nostrils, until the air-cells of the lungs are almost completely emptied. Then insensibility occurs, and convulsive actions of the muscles mark the instant when the brain begins to suffer from the influx of the dark-coloured blood. Soon all motion ceases; save in the thorax, where the heart may be felt yet feebly pulsating. Perhaps some further ineffectual efforts at respiration are resumed, and then all is still. The interval between cessation of respiratory effort and cessation of the heart’s action is brief in the case of strangulation; but it is still more brief in drowning. And the whole succession of events, in the latter case, succeeding rapidly, are complete within a very few minutes. Our author is firmly of opinion that in cases of complete and uninterrupted submersion, life has never been retained after more than five or six minutes have elapsed under water. All stories to the contrary he holds to be apocryphal; and, we believe, most justly.

“The cases which have been reported to the Royal Humane Society of drowned persons who have been restored to life, when taken up cold and breathless after an immersion of half an hour, show that it is not travellers alone that are guilty of the vices of exaggeration and invention. We are compelled to regard these as mere extravagant fables, not more authentic, though certainly less poetical and



elegant than those of nymphs and mermaids, who reside in grottoes beneath the waves of the sea, or than those Arabian fictions, which have astonished our youthful imaginations with the history of submarine nations, whose princes dwell in palaces of crystal at the bottom of the ocean." (p. 90.)

The time during which divers, professed and accomplished divers, are able to remain under water, probably never exceeds two minutes; although it may *seem* to be much longer, and the exaggeration of the time of submersion, by a bystander, in the case of either drowning or diving, may very readily be imagined to take place as it were involuntarily, without any intention to deceive; the observer being himself deceived as to the lapse of moments, by the multiplicity of events which have been crowded into them.

"We all know that our estimate of time depends on the number of circumstances which successively attract our notice. When an event occurs which powerfully impresses the mind, we watch every one of the minutest changes that take place, and the time which elapses before the whole event is completed appears to be proportionably prolonged. Thus we hear of earthquakes in which the commotion of the earth is said to have continued during the space of eight or ten minutes, although in all probability they lasted for no longer time than thirty seconds; and in the same manner we may account for the mistakes to which I have just alluded. When the infidel sultan of Egypt refused to believe that Mohammed could have ascended into the seven heavens, and held one thousand and one conferences with the Deity in the brief space of a few minutes, the Mussulman divine, who was consulted on the occasion, endeavoured to bring his majesty to a more strict faith, by demonstrating that a short space of time was converted into a long one when a great number of important events were crowded into it." (p. 93.)

The treatment of the drowned is similar to that of the strangled. If the removal from the water take place before the diaphragm has ceased to act, respiration may be resumed naturally; if not, artificial inflation is to be employed. And at two periods such inflation may be necessary; first, during the short interval between the cessation of the natural efforts to respire and the cessation of the heart's action; second, when the patient lies in a state of stupor, in consequence of the injurious effects produced by transmission of dark coloured blood to the brain. During resuscitation, "it must be of importance to supply the waste of animal heat, by placing the patient in a warm temperature;" the warm bath forms a simple and convenient method of attaining this object; and it may produce another good effect, "by promoting the natural efforts to respire." Abstraction of blood is of even more doubtful propriety than in cases of strangulation. And there are other things which should not be done.

"We have been directed to employ friction of the surface of the body for the purpose of assisting the circulation of the blood; as if this could do any real good when the action of the heart is ceased; or as if it would not do actual harm by overloading (if I may be allowed to use such an expression) the right auricle and ventricle, when the action of the heart was still going on. The injection of tobacco, and the application of stimulants, belong to the same class of remedies which are either mischievous or useless, proposed formerly by those who did not know what to do, but who thought that they were expected to do something, but now rejected by a more enlightened physiology." (p. 99.)

In the same lecture a few observations are made on "*death from lightning.*" The substance of them is found at p. 104.

"It appears to me that the facts which I have been able to collect relating to this subject lead to this conclusion, that the influence of lightning, or of a powerful shock of electricity, in the majority of cases, is expended chiefly in disturbing, or destroying, the functions of the brain; and the treatment necessary to counteract the effects of the injury may be comprised in a few words.

"Expose the body to a moderate warmth, so as to prevent the loss of animal heat, to which it is always liable where the functions of the brain are suspended or impaired, and inflate the lungs, so as to imitate natural respiration as nearly as possible, whenever the animal breathes with labour or difficulty, or when he has ceased to breathe altogether by his own efforts." (p. 104.)

LECTURE V contains an account of "*some cases of cysts containing watery fluid, apparently connected with the liver*"—prominent fluctuating tumours formed in the hypochondriac region, with more or less distress. Tapping was had recourse to, and clear, colourless, watery fluid escaped; not coagulable by heat. In one case a fatal result almost took place, in consequence of suppuration having occurred. The cyst inflamed; the neighbouring parts became matted together; by ulceration the suppurated cyst made its way into the intestinal canal, and was thence discharged. The supposed reason of the untoward inflammatory attack is thus stated:

"I suspect that in this instance I made an error in being over-anxious to draw off every portion of the fluid, and that in compressing the cyst for this purpose the canals of the trocar became a source of irritation, and laid the foundation of the inflammation which followed. I have seen the same thing happen in more than one case of ovarian dropsy, where the over-anxiety of the surgeon to empty the tumour completely has been followed by inflammation of the ovarian cyst, and placed the patient's life in danger." (p. 112.)

LECTURE VI treats of "*ununited fractures*." Of this chapter there is little or nothing required to notice or comment, until we come to page 126, and there we find a good description of the state of parts in the ununited fracture. Usually the two fragments are connected by a ligamentous substance; firm and dense, but not of a distinctly fibrous structure. In some cases, however, a new joint is ready formed. And as, on this point, our author speaks more decidedly than is usual, as to cartilaginous tipping of the ends of the bones, and production of new synovial membrane, we had better let him speak for himself.

"A new joint is formed—absolutely a false joint. The broken ends of the bone become rounded: there is a capsule nearly as thick as the capsule of the hip-joint or shoulder; this capsule is fibrous, like ligament; it is attached to the bones above and below the fracture, and there is a cavity, like the cavity of a joint, in which the broken ends of the bone lie. And there is more than this: the rounded ends of the bone being covered by a thin, ligamentous substance, and the inner surface of capsule being lined by a smooth membrane, like the synovial membrane, and capable not only of secreting synovia, but of secreting it in abundance. The capsule is a new formation; and the synovial membrane is a new formation also. It is not to be wondered that a synovial membrane should be formed under these circumstances. It seems very easy for the system to construct a membrane of this description. The bursæ mucosæ are made of synovial membrane, just like that of the joints. There is a bursa between the patella and the skin, and this in housemaids sometimes becomes diseased, and converted into a hard lump or tumour. I have frequently removed such a tumour from the knee of a housemaid, and sometime afterwards, on examining the limb, I have been satisfied that the bursa had been regenerated. Nor is this a mere supposition; I have positive

proof that the fact is as I have stated it. There was a woman in the hospital from whose knee the late Mr. Rose removed an enlarged bursa. A year or two afterwards she returned, and came under my care; and not only had the bursa been regenerated, but the new one had become diseased like the old one, and I had to repeat the operation which Mr. Rose had performed formerly. She had gone back to her former occupation, which included a good deal of kneeling, and, under the influence of pressure, the new bursa had become converted into the same diseased structure as the old one. These cases of artificial joints are comparatively rare; the union by ligamentous substance is much more common." (pp. 126-8.)

As to treatment, Sir Benjamin prefers what he terms "Mr. Amesbury's method;" keeping the ends of the bones in perfect repose, and at the same time applying pressure, particularly on the broken surfaces, so as to keep them in the closest possible contact with each other. This may certainly lay fair claim to our recommendation; "if it do no good, it can do no harm." For ourselves, we have quite made up our minds that the most feasible mode of treatment is that by subcutaneous puncture; at first proposed, we believe, by Professor Miller of Edinburgh:\* this converts the parts into a close resemblance of the state immediately consequent on the infliction of the original fracture, and obtains, in consequence, union by satisfactory callus.

LECTURE VII. "*On sero-cystic tumours of the female breast.*" This form of tumour, mainly through the services of Sir B. Brodie, has been long well known to the public. Notwithstanding, its peculiarities may be here briefly restated. In its first stage, it consists of one or more membranous cysts; probably produced by dilatation of portions of the lactiferous tubes. The swelling is globular, painless, movable, and imbedded in the substance of the gland. The contained fluid is straw-coloured and transparent. The general health is unaffected. The ordinary age is between puberty and the middle period of life. By puncturing the cyst and withdrawing the fluid, temporary subsidence of the tumour is obtained; but the contents, in no long time, reaccumulate. To prevent reproduction, our author employs a stimulant embrocation to the skin; but, avowedly, with a result by no means invariably successful. Probably it were better to treat these, as other serous cysts of adventitious growth or enlargement, by injection of iodine. But should any of our readers prefer the more gentle mode, here is the embrocation: "*R spiritus camphorati, spiritus tenuioris, aa. ʒiiss; liquoris plumbi diacetatis, ʒj; fiat embrocatio.*" (p. 152.)

In the second stage, the contents of the tumour thicken and grow opaque; the cysts take on a productive action, studding their inner surfaces with "growths or excrescences," and at the same time, it is probable, deposit of adventitious matter takes place on the exterior, constituting a stroma, as it were, in which the cysts come to be imbedded. Sometimes the cysts become wholly filled with solid matter; the originally cystic structure only becoming apparent when a section has been made of the compact mass.

In the third stage, the integument is involved and ulcerates; or an aperture is made by wound or accident. The contents then protrude, in a fungous form, and both structure and action are prone to degenerate.

\* Principles of Surgery, p. 692.

It is obvious that the only satisfactory treatment of the last two stages, is early and free removal by the knife. Though simple or "benign" at first, this tumour, like all other cystic formations, is apt to change sadly for the worse; more especially when the open condition has succeeded to the occult.

LECTURES VIII and IX treat of "*varicose veins and ulcers of the legs*." The disease is well described; but nothing new is advanced, and nothing requires special notice. As to treatment, our author arrives at the conclusion, with which we feel pretty certain that the great majority of the profession fully agree, namely, that in ordinary cases the palliative treatment is enough, and that the means of obtaining radical cure should be reserved only for those cases which are severe—by extent of varix, by amount of pain, by repeated inflammatory accessions, by ulceration, or by hemorrhage. Of the various methods of radical cure he seems to have no decided preference; at least he hesitates between two:—his own method by subcutaneous section, and that of Velpeau by the twisted suture. For ourselves, we have no hesitation in awarding the palm of superiority to the latter; as equally safe, and rendering obliteration of the veins much more certain. Whatever mode be followed, however, careful bandaging must be employed for a long time afterwards; as there is certainly a strong tendency, otherwise, to the establishment of fresh varices at other parts of the limb.

LECTURE X is a valuable discourse "*on the cases of scirrhus tumours of the breast which require an operation*." An opinion on a disputed, or at least uncertain point, is given by a thoroughly and extensively practical man; and that opinion coincides with what is understood to be the general feeling of the profession.

"You may divide scirrhus tumours of the breast into two classes: one where there is a conversion of the gland of the breast itself into the scirrhus structure, there being no well-defined margin to it; the other, where there is a scirrhus tumour imbedded in what appears to be otherwise a healthy breast, as if it were altogether a new growth, there being a well-defined boundary to it.

"In the first order of cases, where the tumour has no distinct boundary, and where there is a conversion of the gland of the breast into the diseased structure, the operation not only never succeeds in making a permanent cure, but it rather hastens the progress of the disease. The patient dies within two or three years, and probably much sooner, from an effusion of fluid into the cavity of the pleura." (pp. 195-6.)

The following circumstances are to be considered as excluding any successful result of operation: 1. Involvement of the skin; by scirrhus turbercle formed in its substance; by conversion of the tissue into a thickened and brawny state; or by a dimpled tying down of part of the skin, by prolongations of the tumour which at these points cause incorporation. 2. Much retraction of the nipple. 3. Contamination of the glands in the axilla. 4. Adhesion of the tumour to the parts beneath.

On the other hand, under the following circumstances, the operation may be had recourse to, with a fair prospect of a fortunate issue:

"Where on a careful examination no appearance of disease can be detected in the skin; where there is no dimple in the skin over the tumour; where there is no diseased gland in the axilla; where there is no sign of internal mischief; where there is no adhesion of the breast to the parts below; and where the patient is not very much advanced in life;—in a case where this fortunate combina-

tion of circumstances exists, we may presume that there is a reasonable chance of an operation being successful." (p. 199.)

Further, Sir Benjamin coincides with the ordinary opinion, that in certain hopeless cases, it may be our duty to operate; without any hope of ultimate cure, but merely in order to palliate suffering, and perhaps protract existence. Even then, however, rejecting "altogether those cases in which the skin is distinctly contaminated by the disease, whether it be that there are scirrhous tubercles in it, or that it be converted into the brawny structure which I have formerly described. In neither of these cases will the patient obtain even a respite by submitting to an operation." (p. 206.) Tumours which originate in the nipple are supposed to offer a better chance of cure, by operation, than those which commence in the substance of the gland. On this point, let the reader consult Dr. Walshe's treatise.

LECTURE XI treats of "*corns and bunions*;" but seems rather out of place, and might have been omitted without much loss.

LECTURE XII. "*On the administration of mercury in cases of syphilis.*" Our first remark here is, that the author, in our humble opinion, leans rather much to the mercurial mode of treatment; that is to say, he seems to express himself so as to lead the unwary to believe that no case of syphilis can be cured, or well cured, without the use of mercury; and, further, that the employment of mercury will seldom if ever prove thoroughly beneficial, unless given in large quantity, and for a long period. We need not remind our readers that to the assigning of such a broad position to the antisymphilitic mineral, a large body of the profession stoutly and wisely demur. And they do not agree with Sir Benjamin in thinking that they have retrograded in their art, by departing from the Pearsonic treatment of syphilis. As to the mode of exhibition, our author is very partial to inunction.

"If you ask me which is the best way of using mercury where the symptoms of syphilis are not of the very mildest character, I must say that that by inunction is infinitely to be preferred. Mercurial inunction is dirty, laborious, and troublesome, and it makes the matter public to the family in which the patient lives; for one or other of these reasons it will generally be unpleasant to him. But it has these advantages: it is much less liable to gripe or purge; it cures the disease a great deal better, and does not damage the constitution half so much, as mercury taken by the mouth; nay, I will go so far as to say, that, except in the slighter forms of the disease, you really cannot depend upon any other kind of mercurial treatment for the production of a cure. You may patch up the disease by giving the remedy internally, but it will return over and over again, and then you may cure it at last by a course of mercurial ointment properly rubbed in. I say properly rubbed in, for much depends on this. The patient, if not well instructed, will perhaps continue the friction for a few minutes; but it ought to be continued before a fire at first for at least half an hour, and very frequently for three quarters of an hour. After some time the ointment will be absorbed more readily, and it may then be rubbed in for a shorter period." (pp. 241-2.)

To children, especially he conceives inunction applicable. And the following is his mode of using it:

"I have provided a flannel roller, on one end of which I have spread some mercurial ointment,—say a drachm, or more; and I have applied the roller, thus prepared, not very tight, round the knee; repeating the application daily. The motions of the child produce the necessary friction; and the cuticle being thin



the mercury easily enters the system. This causes neither griping nor purging; in a child it does not even in general cause soreness of the gums; but it cures the disease. Very few of those children ultimately recover in whom the mercury has been given internally; but I have not seen a single case in which this other method of treatment has failed." (pp. 244-5.)

We cannot take leave of Sir Benjamin, in this chapter, without thanking him very heartily for "putting in a good word" for sarsaparilla; a useful medicine, which it is too much the fashion of the present day to neglect and decry.

LECTURE XIII. "*On tic-douloureux, or facial neuralgia.*" The uncertainty of our pathology of this lamentable ailment is well stated; in regard to those cases in which the mischief obviously depends on some disorder of the system at large, not on any local affection.

"There is something or another, somewhere or another, in the system, which acts as a source of irritation to the nerves of the face; but where that something is, and what it is, we cannot discover." (p. 259.)

Contrary to what we apprehend is the general impression, Sir Benjamin maintains that little or no good ever follows the removal of diseased teeth.

"I never knew a case in which a patient was relieved of a genuine *tic douloureux* by the extraction of a tooth; and I remember in a conversation which I had with an experienced dentist some years ago, that he told me that he had very frequently been called upon to extract teeth on these occasions, and that he was not aware that the operation had been of service in any one instance." (p. 256.)

It may be so; yet we should consider it but a clumsy practice which would permit diseased and useless teeth to remain in the head of a patient afflicted with *tic douloureux* of the face.

As to the merits of the "*operation*" for tic, no one will dispute the propriety of its being placed in the following position.

"It is altogether an unscientific operation, from which we have no more right to expect benefit than we should have from the amputation of the testicle in a case of pain referred to that organ in consequence of a calculus being lodged in the ureter." (p. 261.)

In speaking of the empirical use of certain remedies, our author states that he has never known any benefit to accrue from increasing the dose of the carbonate of iron beyond a drachm thrice daily, and properly insists on the occasional administration of purgatives, to prevent accumulation of the medicine in the colon, as is otherwise apt to occur. But in regard to these, and all other *quasi specifics*, it is most important to remember that it by no means follows that because they are doing no good they are doing no harm. "*No one can be dosed constantly with medicine without the health being ultimately injured by it; and if you have not some reasonable grounds for giving medicine, you have no right to run the risk of doing harm by its continued exhibition.*" (p. 265.) We are satisfied that many systems have been improperly ruined in empirical and unsuccessful attempts to purchase a local advantage at too high a cost. How often in amaurosis, for example, has not the routine of mercury, colchicum, strychnia, bleeding, blistering, left the patient much worse than it found him? As regards *new remedies*, the following quotation contains a wholesome truth:



"I shall take this opportunity of observing, that I am not disposed to try indiscriminately all the new remedies which in these days are being constantly brought before society; nor can I think well of this modern fashion of resorting on all occasions to novel methods of treatment. I advise you, if you wish to succeed in your profession, and to be useful to society, to pursue a different course. Make yourselves masters of the old remedies. Learn how to handle them, and what good they will do, and, as a general rule, have recourse to them in the first instance. If the old remedies fail, and you are at a loss as to what you should do, then, and not till then, have recourse to the new ones. If you always begin with new remedies, you throw away the valuable results, not only of your own experience, but of the experience of those who have gone before you. You have to begin, as it were, *de novo*; and the first consequence of this will be, that you will not cure your patients; and the second, that you will have none to cure." (pp. 267-8.)

LECTURE XIV. "*On fatty or adipose tumours.*" In some cases of lipoma, a hypertrophied condition of the integument and subcutaneous adipose tissue—not an example of the true fatty tumour—our author is of opinion that benefit is to be derived from the internal use of liquor potassæ in large doses; a drachm, thrice daily in small beer.

The next six chapters treat of *mortification*. They are, for the most part, quite elementary in their character; but the following points are more suited for especial notice:

"Another plan may be adopted to prevent mortification from pressure; that is, to prevent the inflammation which precedes it. The thicker the cuticle the more it will protect the parts beneath; you may, if you attend to it in time, add to the thickness of the cuticle by stimulating the surface of the skin. Nurses know this very well, for when patients are bed-ridden they wash the parts subjected to pressure with brandy. What is still better is a lotion composed of two grains of bichloride of mercury to an ounce of proof spirits. When you think that a patient is likely to be confined so long in bed that sloughs may be formed on the os sacrum, begin at an early period to wash the parts two or three times a day with this lotion. I have found it useful in other cases where a patient suffers from pressure. For example, in a case of hernia, which requires to be supported by a very powerful truss; the truss galls and frets the skin, and may at last cause inflammation and sloughing; but under the use of a stimulating lotion a thicker cuticle is generated, and such mischief is avoided." (pp. 311-12.)

"Almost always, when you use a caustic, it is prudent to have some counter-agent at hand to stop its action if it reaches a sound part. Acids may be neutralized by alkalies; caustic potash may be neutralized by vinegar, or by a solution of the diacetate of lead. If you are afraid of nitrate of silver burning the neighbouring parts, its action may be neutralized by common olive oil. A solution of bicarbonate of potash will decompose chloride of zinc, and so with other caustics." (pp. 328-9.)

Subcutaneous nævi may be destroyed by the subcutaneous puncture of a knife, freely lacerating the morbid structure, followed by a probe dipped into nitrate of silver, melted in a platina or silver spoon. The advantage of such a procedure is, that when the nævus is situate on the face, it may be removed effectually thus, without having any cicatrix or other deformity. The operation "causes inflammation and sloughing, at the same time obliterating the vessels beyond the margin of the slough."

Is our author certain that the following is a fact?

"The fact is, that a wound always heals much more readily after the application of caustic, than after the use of the knife. Take two cases: if you destroy one

tumour of a given size by the knife, and the other, supposed to be of the same size, by caustic, in spite of the time occupied by the separation of the slough, the sore in the latter case will be healed sooner than that in the former." (p. 384.)

We think we could get up a good deal of evidence to the contrary.

In applying caustics to the scalp, observe caution. Extensive destruction of the pericranium may cause necrosis of the corresponding portion of the skull, throughout its entire thickness; this is likely to be associated with purulent detachment of the dura mater, and from that a train of fatal symptoms may arise. Our author has seen this unfortunate sequence.

Chloride of zinc is not a suitable escharotic for soft parts, unless a raw surface previously exist. When applied to the unbroken skin it produces intense pain, and fails of the escharotic result.

Most cordially do we agree with the following. There are a few cases certainly, which we need not here stop to specify, in which the actual cautery is preferable to caustics; but only a few. Of late years we were much afraid that a retrograding tide had set it in favour of the "rude piece of farriery;" but for some time back we have been charmed to find such fears gradually subsiding, the cauteries having again resumed their little disturbed position in the armamentarium.

"The actual cautery does nothing which caustics would not do as well or better, and it is much more alarming and frightful, both to the patient and to bystanders. It was the habit of surgeons here, fifty or sixty years ago, to use the actual cautery to a great extent; and it appears to be one of the many proofs of the advancement of English surgery that we have got rid of what Sir Astley Cooper used to call 'a rude piece of farriery.'" (p. 345.)

LECTURE XIX. "*On senile gangrene*," we could have wished expunged. The description contains nothing new, and some of the treatment is, in our humble opinion, both old and bad. Our readers need not to be told that the animal-food and port-wine system is not that which, in a large proportion of such cases, ought to be recommended.

LECTURE XXI.—"*On chronic abscess of the tibia*." Here Sir Benjamin is peculiarly at home. The profession is undeniably indebted to him for a large boon, in having determined the existence of this formidable disease, and the simple and satisfactory means whereby it may be remedied. The affection is most liable to occur in the tibia, of either extremity, but most frequently at the upper. In 1824, the first case occurred to our author; but unfortunately its true nature was not discovered till after amputation. In 1827, a second patient appeared; and, being subjected to the suitable treatment, retained his limb, and was rid only of the disease. Since then it must indeed be very satisfactory to Sir Benjamin

"to know that this simple method of treatment has already preserved many limbs which must have been sacrificed otherwise, and it cannot be doubted that it will be the means of preserving many more, when it is generally adopted by those who are engaged in operative surgery." (p. 403.)

A trephine of less size than what is ordinarily used for the skull is generally sufficient; and it should have no projecting rim or shoulder, so as to permit of deep penetration. The circumstances which point to the operation are the following:

"When the tibia is enlarged from a deposit of bone externally—when there is excessive pain, such as may be supposed to depend on extreme tension, the pain

being aggravated at intervals, and these symptoms continue and become still further aggravated, not yielding to medicines, or other treatment that may be had recourse to, then you may reasonably suspect the existence of abscess in the centre of the bone. You are not to suppose that there is no abscess because the pain is not constant; on the contrary, it very often comes on only at intervals, and in one of the cases which I have related there was, as I then mentioned, an actual intermission of seven or eight months. After the disease has existed a certain number of years, indeed, the pain never entirely subsides, but still it varies, and there are always periods of abatement and of exacerbation." (p. 404.)

Our last quotation is from the preface.

"If what I now offer to their perusal should prove acceptable to the profession, I may venture to publish a Second Series of my Lectures, as soon as I find the necessary leisure for that purpose." (p. iii.)

We venture to say that the profession will gladly take Sir Benjamin at his word. But if we may be allowed, we would throw out a hint for his consideration in regard to the next volume; namely, the propriety of not giving us the lectures exactly as they were delivered; but holding back the elementary, the well-known, and—even though good—the stale,—giving prominence and bulk to his own views and practice when varying from the ordinary routine, and bringing all up quite to the level of modern scientific advancement.

#### ART. XIV.

*The Structure and Functions of the Female Breast, as they relate to its Health, Derangement, and Disease.* By E. W. TUSON, F.R.S., Surgeon to the Middlesex Hospital.—London, 1846. 8vo, pp. 485.

THE earliest impression produced by this book is, that—

" . . . . . one Mr. Tuson  
Has taken up arms against English and Grammar;"

and it is an impression no less enduring (scarcely a page fails to give it fresh distinctness) than it is early. The preface introduces sundry examples of the belligerent propensities of Mr. Tuson against the parts of speech; and to illustrate these, as well as to give the reader some inkling of the aims and qualifications of this anti-grammatical warrior, we shall examine the contents of the said preface more closely than is our wont.

The book was, we learn, "compiled during those hours which could be spared from professional engagements, both in public and private practice,"—an intimation which might be regarded merely as a satisfactory indication of the marvellous extent of Mr. Tuson's practice, did it not serve to introduce a very remarkable apology for all the deficiencies of the subsequent pages. The practice caused interruptions, "which on some occasions broke the chain of reasoning already commenced, and perhaps may have prevented the same line of argument being continued; so that should the reader find any defect in this respect, he must attribute it to *this cause*." Certainly, to this cause we shall invariably ascribe the contradictions, the total absence of reasoning power, nay, the want of common sequence of ideas, which throw a certain veil of obscurity over what would otherwise doubtless prove a mass of living light. Mr. Tuson's

facilities were money-gathering; people, too, would not let him alone,—and each absurdity he has put to paper is but an indirect proof of his social and intellectual greatness. And yet with touching modesty Mr. Tuson apologizes for trespassing on public indulgence with this his seventh book. Let him take heart,—when individuals of his mental caliber condescend to produce books, what right have poor devils, like ourselves, to be nice? *They* may, it is true, be “prevented from continuing the same line of argument;” but what is *our* business, but to establish the missing nexus, and exult that we are permitted to exercise our intellects upon matter so profound? And Mr. Tuson talks so humbly of his early productions—his waistcoat-pocket Remembrancers, &c.—that our very hearts yearn towards a man so modest and yet so great. True, we do remember to have heard it said, there was a wondrous adaptation of the bulk of the said productions to the capacity of the intellect producing them, but for our parts we always saw in such observations naught but envy and malice. And does not the event justify us? Here is a book “as big as the best of them;” nor does its character allow the reader for a moment to imagine that Mr. Tuson has given the world one more illustration of the immortal fable of the Frog and the Bull.

Mr. Tuson’s opportunities of observing have, he tells us, been great; but he feels “little satisfaction with the progress made in such a number of years.” He has, however, “of late been *more* than particularly active [he was always, as is well known, particularly active] in his researches, and feels a *greater* gratification in having accomplished much more in a shorter space of time.” Of course; all Mr. Tuson has to do at any time is to take to a subject in earnest, and he must do wonders; in the present instance he has kept the disclosure of the wonders for, we suppose, the “second edition;” and if we cannot have them just at the moment, it is well to know that they are to appear *in futuro*.

In page x. Mr. Tuson discourseth in the following wise, “the brain and spinal marrow have entering into their formation *certain acids* and products which are not present in other structures, and *which are* perfectly distinct from *other animal tissues*.” There are several (doubtless valuable, though to our limited comprehension somewhat obscure) scraps of information contained in this sentence. We learn, for example, that “certain acids” are “animal tissues,” and yet further, that these “certain acids” are “perfectly distinct from *other* animal tissues.” We have worked at this till our brain has reeled, and yet fathom its entire depth we cannot. It is indeed clear there is a *lapsus* somewhere, and we would venture to suggest that Mr. Tuson was “called to a case” at the words “certain acids,” and was thereby unfortunately “*prevented from continuing the same line of argument*.” That no effort of his could ever recall the profound train of thought, in which he had obviously launched, when he reached the words “certain acids,” is not in the remotest degree strange. Your twelve-and-sixpenny Dutch clock may be shaken and shaken, and put out of sorts in all manner of ways, and yet on it goes still;—but your finely and delicately organized repeater must be more gently treated,—misuse it, and its train of action cannot be coaxed back into regularity. Need we say that Mr. Tuson’s mind is among minds what the most precious of repeaters is among time-pieces?

Mr. Tuson taketh high rank among organic chemists: "I," he says, "have pointed out that brain and nervous matter contain cerebrie acid, oleophosphoric acid, cholesterine, &c." We certainly never knew till this moment that it was Mr. Tuson who "pointed out" these facts; but it is never too late to learn. "What effect," he asks, "would cerebrie acid have in certain deranged states of the mind? What effect oleophosphoric acid?" Aye, indeed, what effect? These are certainly puzzlers for our weak noddles; but no wonder, for even our philosopher himself confesses they are questions "he is *at present* unable to answer." We do not despair, however; "in a short time he hopes to be able to place before the public some interesting particulars on this and other points." May time flow rapidly till the blissful advent of the said particulars!

Mr. Tuson next saith: "Liebig has pointed out that certain *chemical changes* occur in the lungs, differing from *other parts of the body*." Liebig has unquestionably done and said strange things, but if he has spoken of "chemical *changes*" which "differ from other *parts* of the body," then hath he outsaid himself. Our active friend was evidently called out here to another case. But if Liebig outsays himself, Mr. Tuson does more, he even outdoes himself,—only fancy, he "*looks forward* to an advancement in the scientific practice of our profession, to an extent of improvement that *cannot be here anticipated*." He looks forward to that which cannot even be anticipated! How he manages this, we are left to divine, as he immediately turns to a just denunciation of those "*members* who fancy themselves *branches* of a liberal profession, who on many occasions have been pleased to term my endeavours to advance the practice of medicine 'Quackery.' " Who are these audacious and outrageous "members" who go the length not only of "fancying themselves branches," but even of fancying Mr. Tuson a Quack? Give us their names—that we may hold them up to deserved execration.

Mr. Tuson proceedeth again to discourse chemically: proteine becomes the subject of remark. "Scherer analysed this *original matter* prepared from animal albumen and fibrine, from the crystalline lens, from hair and horn, and the results of all these analyses agreed with the formula  $C^{46} H^{86} N^6 O^{24}$ , which is about identical with the blood in a healthy state." This remarkable statement of the almost identity of a chemical formula and healthy blood ushers in another no less striking. "From a variety of practical observations, made during a period of nearly twenty-five years, I have been frequently struck with the want of power in the system to enable the constitution to bear up against disease." Profound and startling discovery! After twenty-five years of labour (active labour, too, as we have already said) Mr. Tuson has succeeded in establishing to his satisfaction the novel and momentous truth that people may die of disease!

Quis potis est dignum pollenti pectore carmen  
Condere, pro rerum majestate hisque repertis?  
Quisve valet verbis tantum, qui fundere laudes  
Pro meritis Ejus possit, qui talia nobis  
Pectore parva suo, quæsitæque præmia liquit?

The arrangement of his book naturally occupies Mr. Tuson's attention in the preface, and among his comments thereon we find the following: "Next follows the classification of diseases of the breast, *which* have been

divided into three." What have been divided into three? not the classification surely,—nor the diseases, for the learned author admits some forty or so;—nor the breast, for breast is a singular noun, and Mr. Tuson has too deep a respect for Lindley Murray to give it a plural verb. What then *has* been divided into three? of this we fear we must be content to remain in ignorance; while we attend Mr. Tuson to the following pages, where he complacently refers to "*many interesting practical remarks, of service to those afflicted with abnormal formations,*" contained in certain parts of his volume;—also to "*considerations . . . . which terminates;*"—also, "*to the following works,*" which following works, be it observed are *names of writers* simply, one half of the foreign ones being misspelled, &c.

"Finally," says our modest author, "all reasonable means have been bestowed to render my labour complete, yet after all there may be certain parts which require correction and improvement." Let no such misgivings assail thee, O great man,

E tenebris tantis tam clarum extollere lumen  
Qui primus potuisti:—

nothing can be more complete or perfect *in its way* than thy volume,—to alter would be but to spoil;—to improve would be impossible.

But to descend from the heights of Tuson to the level of common men and common physic:—it does not require the experience, which our habit of examining books critically has of necessity given us, to know that the author of such a preface, as that just noticed, must be incapable, under any estimate of human possibility, of writing a treatise worth reading upon any possible subject. But it may be that the reader desires some proofs of the fact, as deducible from the volume before us; and though we confess ourselves most strongly disinclined to the task, we proceed to discharge, as quickly as possible, what perhaps may be esteemed a duty. But of analysing the book we have no intention,—indeed it defies analysis,—we shall simply give *a few instances*, as they chance to fall under our eye, of the various and manifold *peculiarities* which pervade the pages before us, and which, though we mortal critics may deem them blots, or blunders, or blemishes, may, in reality, be in the eyes of the gods and Mr. Tuson, beauties of the first water:—to enumerate all would be tantamount to reprinting the work—minus some of its quotations.

Mr. Tuson discourseth concerning the importance of *classification*, and exhibits his power in this way by placing "hysterical affections of the breast" among the "Organic Lesions" of that part. (p. 134.) But this is a small effort: see the classification of "cancerous diseases." (p. 219.)

Scirrhus.	Carcinoma simplex.
Cancer, fungus hematodes, medullary sarcoma, and melanosis	Carcinoma reticulare
	" alveolare
	" medullare
	" melanodes
	fasciculatum.

Was there ever such an attempt made at classification since the creation of scatter-brains? Each word absolutely constitutes or implies an error—that is, to us, critics of the earth, earthy: but, *Dīs et Tusoni aliter visum.*

Mr. Tuson occasionally talketh concerning subjects of which (with all his "activity") he may be presumed not to be over deeply acquainted.



He dippeth deeply into the fantastic chemistry of the day,—but in a mighty safe way, by quoting *monster-passages* from Liebig, of the real signification of which he probably comprehends no more than of so many problems in astronomy. Now we do not expect Mr. Tuson to be a chemist; he has enough to do—or should have enough to do—without wandering from the regions of practice; but we do expect a great pathologist like him—one leading on the front ranks of “active” pathological investigation—to have at least some glimmering of accuracy in his ideas on such subjects as the generation of pus. Yet truth—for which our veneration is greater than even for Tuson—compels us to affirm that the man’s mind is in a state of Cimmerian darkness on the subject. Conceive him—at the present hour—quietly accepting the notion of pus-globules being produced (in ordinary suppuration) in the interior of the vessels from changes of the red corpuscles of the blood, and admitting by inference the heresy in physiology that the capillaries have open mouths!

Mr. Tuson’s singular power of appreciating the bearings of a question of morbid anatomy, and of his grand off-hand way of getting rid of small difficulties that trouble weaker minds is deliciously exhibited in his account of “fibrous tumours of the breast.” This account is simply a *reprint* from an English journal of M. Cruveilhier’s opinions (as these existed previous to the discussion at the Paris Academy) on the subject; it occupies *eighteen* pages of our learned author’s volume, and is put down without one syllable of comment in any form or shape. Now, as is notorious (we should have imagined that even Mr. Tuson knew this), Cruveilhier was obliged, during the progress of discussion, to retract many of the positions which he started with; what is printed by Mr. Tuson consequently misstates the existing opinions of Cruveilhier; and gives aid (such as it is) in disseminating abundant error. What singular obliquity of intellect—that betrays the author, even in reprinting huge masses of other people’s writings, into flagrant perversion of historic and actual truth!

One method by which Mr. Tuson evidently fancies he produces a very striking effect, is by suddenly turning off from the subject before him to some other utterly unconnected topic: the specimens of such “Tusonian mental catenation,” as we would venture to christen it, are really wonderful. Chemistry is the subject to which the wandering intellect constantly turns; and may be termed the ignis fatuus of the Tusonian mind. Thus, among a multitude of such vagaries, Mr. Tuson in one place discourses concerning the process of suckling and the properties of human milk, and then and there launches into *quotations* of *eleven* pages in length from Turner and Liebig, concerning the composition of the animal textures. These quotations are a tolerably safe game (not always so, however, as appears from the instance already given); but the moment Mr. Tuson becomes chemical on his own resources an occasional Tusonianism of course occurs. Thus appears the phrase, “caseine which is distinguished from fibrine and albumen by not coagulating when heated;” (p. 23)—“coagulation by heat” is somewhat of a novel characteristic of fibrine, we believe.

The chapter on cancer displays another of the peculiarities of our author. All manner of opinions are quoted with approbation; they are all of them right; and yet these opinions are, in numerous instances, more or less

totally subversive of each other. In the very narrative of these opinions there is curious disorder; it would seem as if the facetious Tuson had put his quotations on separate pieces of paper, shaken them in a bag and then printed away on the principle of first come first served. But there is much novel instruction in some parts of this chapter; such as that derivable from the fact that Mr. Tuson describes "*fungus hæmatodes*," (p. 315,) "*carcinoma medullare*," (p. 331,) and "*medullary sarcoma*," (p. 343,) as though they were perfectly different productions!

In describing atrophy of the breast Mr. Tuson misses the only point of interest connected with that state,—namely, the possibility (more than once actually realized) of its being mistaken for the form of scirrhus attended with diminution of size of the mammary gland. The chapter on hypertrophy, on the other hand, exhibits the author's total ignorance of the only valuable essays ever produced on the subject,—among others of that of Fingerhuth.

We cannot refrain from copying some few sentences wherefrom further idea may be formed of the lucidity and perspicuity of the Tusonian style;—and we give them as they stand, without comment or illustration, however temptingly they solicit this:—"If the mother cannot, when her child by the most pathetic cries demands, yield it a genial balmy food, uninjured by fatigue, agitation of mind or indigestion." (p. 22.)—"There are certain constitutions unfit and improper to the performance of lactation."—"The intermarriages of cousins have been fully established to produce weak and delicate children, *by the admixture of the blood of one branch*." (p. 61.)—"The deaths in Paris and its immediate environs (within a radius of five to six miles) from cancer in 1830, 668 persons were said to have died of cancerous complaints, which was 1—96 per cent. of the deaths in that year." (p. 252.)—"The maternal office of suckling is always attended with a calm serenity of mind, scarcely felt *in other situations*." (p. 40.)—"It is stated upon authority that girls *of the best character*, by the irritation of a child sucking, have become able to support it."

Now against all these innumerable forms and varieties of defect—or to speak safely, peculiarity—what counterbalancing merits are to be placed in the scales? We shall not say—not a single one; but truly not a single one that we, mortal men, have been able to discover! We beg pardon—there is one—the greater for its very loneliness—namely, the modest delicacy of expression that pervades these pages, especially visible when Mr. Tuson has to speak of certain unmentionable parts of the frame. Thus Mr. Tuson would not for worlds talk of putting leeches to the pudendum, anus, or perinæum—he would "*apply ten leeches so as to abstract blood from the portal system*." He writeth for mothers of families, and would not offend their chaste eyes or ears,—he looketh forward to a domestic edition for their especial use, and desireth to save his editor the task of "*expurgating*,"—indeed he evidently liketh not the idea of a "*Tuson Expurgatus*." How the mothers of families are to guess where the leeches may best be made to act on the "*portal system*" does not clearly appear,—but Mr. Tuson is doubtless always to be found, and always ready to listen to the cries and suffering of ignorant humanity.

It is with feelings of severe pain and annoyance that we find this book dedicated to that truly excellent man, Lord Northampton, in his capacity

of President of the Royal Society. It is true that Mr. Tuson is (by what singular coincidence of chances remains a curious problem for solution) a Fellow of that Society; and it may be held a matter of common courtesy on the part of the President to accept a dedication offered by any of the Fellows. We doubt not that this was the case in the present instance. The noble Lord was assuredly unaware, when he granted his permission, that he was about to lend the influence of his scientific position to furthering the objects of a man who is not only ignorant of science, but actually incapable of writing his mother-tongue with the degree of correctness which, now-a-days, may reasonably be expected from a maid-servant. Mr. Tuson assures us that though he has been accused of quackery he is not a quack. We take his word for the truth of what he should know better than anybody else. But if Mr. Tuson really were a quack, we should expect to see him employing the presidential name, thus incautiously accorded, precisely on the same principle as "Professor Holloway" heads his placards with the respectable and respected name of the senior Physician of Guy's Hospital.

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#### ART. XV.

*Medical Notes on China.* BY JOHN WILSON, M.D., F.R.S., F.S.S., Inspector of Naval Hospitals and Fleets.—*London*, 1846. 8vo, pp. 267.

WE have on several occasions brought under the notice of our readers the valuable Reports on the Health of the Navy, compiled by Dr. Wilson, from the official returns of the medical officers of that branch of the service. The volume before us from the pen of the same author is of a different character, being rather a medical history than a statistical account of the naval portion of the Chinese expedition. The French possess many excellent works of this description, but their utility appears hitherto to have been much overlooked in our service. It seems highly desirable that when an army takes the field, the senior medical officer with it should be directed to collect the necessary information, and at the end of the campaign to draw up a general account of the diseases which have prevailed among the troops, their apparent causes, the measures adopted to preserve the health and efficiency of the men, and the success which has attended these measures. Similar instructions might be given to the senior medical officer of the fleet in cases where a naval force is employed. To enter fully upon the advantages which might be expected from a regulation of this nature would occupy more space than we can at present afford, we would merely remark, that had Sir John Pringle's work, which might well be taken as a model for such reports, been studied by the authorities in 1809, very many men might have been saved who fell inglorious victims to the Walcheren fever.

In March, 1842, the *Minden*, 72, Captain Quin, which had been fitted up as an hospital ship, calculated to accommodate 200 patients, besides the crew, and to which Dr. Wilson had been appointed principal medical officer, with an adequate staff, was sent out to Chusan, where he arrived on the 15th of August. No pains or expense had been spared to render her in

every respect suitable for the purpose for which she was intended. "Nothing," says our author, "was omitted which, in the opinion of the authorities who ordered and designed the hospital establishment of the *Minden*, could conduce to the comfort, convenience, and well-being of the sick and wounded who might be received into it; and it may be unhesitatingly asserted that no such moveable hospital, in respect of magnitude, means of efficiency, and completeness, ever left an English port."

From the period of his arrival at Chusan, Dr. Wilson noted "from time to time, whatever appeared to him most worthy of record in China: first, in respect to disease, its nature, treatment, progress, and results; and second, regarding its causes, apparent and probable, whether permanent and necessary, or artificial and capable of being removed." These notes, extending over a period of above two years, from August 1842, to the end of 1844, constitute the volume now before us, and are printed as originally put down without any alterations. We cannot but regret that our author adopted this form of publication in preference to a more finished and systematic work founded on his notes, and that he has not given us more information of a statistical nature, which, from his previous pursuits and acquaintance with the subject, he was so well qualified to collect. The meteorological observations also, which appear to have been made with great care, would have been much more useful if tabulated, than dispersed as they at present are throughout the book. It is but just, however, to mention that during a considerable portion of our author's service in the China seas his health was much affected, and it is most creditable to him that, notwithstanding the bodily suffering and languor under which he laboured, he continued to collect so much interesting and valuable information as is contained in the work before us.

We have already stated that the "Notes" extend over a period of upwards of two years; during the first nine months the *Minden* was in the harbour of Chusan, and for the rest of the time in that of Hong-Kong.

I. At the date of the arrival of the *Minden* the principal part of the force was employed in the Yang-tse-Kiang, whence the most severe cases of disease were sent to Chusan, as opportunity offered, for treatment on board the hospital ship.

On the 1st of July, 1842, the numerical strength of the naval force was about 4200 men; during the quarter ending 30th September, the number entered on the sick lists amounted to 5201, or 124 per cent. of the strength; and the deaths to 88, or  $2\frac{1}{10}$  per cent., while the proportion of deaths to the number treated was one in 59.\*

The diseases by which this enormous amount of sickness (the admissions during one quarter being a fourth more in number than the whole force employed) was occasioned, were principally fevers and diseases of the bowels; of the former, 1951 cases occurred, whereof 1644 were intermittents, and of the latter, 1606 were entered in the sick returns, of

\* Among the ships composing the "Home Force," the annual ratio of admissions into hospital amounts to 190 per cent., and deaths to 1.07 per cent. of the strength. Consequently, in the Chinese fleet, the sickness in three months was rather more, and the mortality double what would have occurred in twelve months among the men employed at home. It must, however, be remembered, that the three months in question constitute the sickly season there. We have unfortunately no statement of the number of admissions and deaths during the rest of the year, but there can be no doubt that, although less than that noted above, it would still be found very considerable.

which 1086 were diarrhœa, and 226 dysentery. The last number, however, is by no means a correct index of the amount of dysentery which came under treatment, because cases admitted as diarrhœa often ran into that form, and it also frequently attacked patients labouring under intermittent fever. This indeed formed one of the principal peculiarities in the fevers of these shores, and materially increased the difficulty of treating them.

“With very few exceptions the periodic fevers and fluxes have been complicated with each other, often coexisting, but more frequently alternating; and this complication has made a principal difficulty in adjusting the means of treatment; for those fitted to be useful, and promising utility in the fever, having generally proved injurious in the flux; it happens constantly that, on the subsidence of the former, the latter sets in to yield in its turn, and again to recur, till the subject is worn out, the exhaustion being generally associated with an immense amount of organic injury.” (p. 53.)

“Here the flux ought to be considered a constituent part of the periodic fever, rather than an independent malady in most instances, but is often so prominent a part, and presses so closely on the attention both of patient and practitioner as greatly to distract the observer, and disguise the original essential affection. The obscurity thus occasioned is apt to be increased by the conflict of indications in treating the two phases on the supposition that it is but one and the same in reality; for as has been stated above, and is pretty clear from the nature of things, the means which are fitted to act remedially in the intermittent fever, often cannot be borne, or act injuriously in the intestinal flux.” (p. 58.)

Another very troublesome disease which appeared intimately allied to fever, and prevailed to a very considerable extent, was sloughing ulcer. It frequently happened in patients admitted on this account, that as soon as the ulcer began to heal favorably, periodic fever appeared, and alternated with the ulcer, or *vice versâ*, leading to the belief that, in many cases at least, they were caused by the same morbid influence. This occasional identity of origin is to a certain extent corroborated by observations made in the West Indies, where, during epidemics of remittent fever, soldiers affected with ulcers generally escaped an attack.

The fever at Chusan was usually of an adynamic character, and bore reductive treatment badly. Sometimes a sudden and unaccountable sinking of the powers of life took place.

There can be little doubt that the fever and dysentery were the product of marsh miasm. In the Island of Chusan no natural marshes exist, but every spot of ground capable of being irrigated has, with great perseverance and ingenuity, been converted into a rice field, or in other words, an artificial marsh. “If heat, moisture, and vegetable matter excite periodic fevers, as they evidently often, but not always do, few places can be supposed more prolific of their cause than the city and plain of Tinghae,” the capital of Chusan. After taking a review of the topography of the island and nature of the seasons, Dr. Wilson says—

“The meteoric influences and the aspect of the country appear highly favorable to health. What is detrimental is believed to be chiefly the wilful work of man's hands, or of his neglect, and perverse ignorance. If he acted his part intelligently and well, there is little doubt that this would be one of the most salubrious, as it is naturally one of the most favoured portions of the earth's surface. It would not be becoming to assert that ague, flux, and ulcer could be



entirely banished by any human means ; but it may be alleged confidently that they might be much reduced both in frequency and force, if the inhabitants would abandon some of their agricultural and economic usages, supplying their place by other methods simple in themselves, and at once more easily practised and more beneficially operative." (p. 113.)

The same source of endemic disease, the malarious exhalations from the irrigated rice fields, exists to a still greater extent on the shores of the Yang-tse-Kiang, where, during this period, the sailors were chiefly employed, as the flat nature of the country is more favorable for this species of agriculture.

"Here there appears to have been abundant materials for the production of periodic fever. . . . . The wonder is that with such high degrees of heat acting on such a soil, they were so generally some well-defined type of intermittent ; cases of remittent fever with much precipitancy and fatal force were not nearly so numerous as might have been expected ; indeed they were very rare." (p. 51.)

II. On the 7th of June, 1843, the *Minden* arrived at Hong-Kong. The diseases experienced here among the sailors were of the same general character as at Chusan, being chiefly endemic fever and dysentery. There were, however, several points of difference worthy of remark, and there was almost complete exemption from sloughing ulcer, which, as already stated, had been found so troublesome.

"It was noticed as a subject of some surprise in the north, considering the atmospheric heat and excess of artificial miasmated soil, that remittent fever should have been so rare, the fever there so constantly assuming the intermittent form, and generally, however complicated with other morbid actions, exhibiting regular and well-defined types. Here, on the contrary, there have been during the last three months (June, July, and August, 1843) a large proportion of remittent fever, frequently, it is true, as elsewhere, when not terminating fatally, issuing in ague ; but the agues have not been so formal in type as at Chusan ; and there [this ?] is another well-defined point of difference between the morbid manifestations of the two places. Then there is a striking feature of likeness between them, that, namely, of their being in both places associated with flux ; for here, as there, the two forms of disease—fever and flux—are often so intimately linked together that it is difficult to tell which is the primary, or ought to be considered the principal affection. Perhaps fever in a majority of instances appeared first, but the exceptions have been so numerous, fever being so often preceded by flux as to give it little claim to be considered a prevalent or necessary priority. Whichever appeared first it constantly happened that as one series of morbid actions declined the other rose. They were sometimes concomitant, but more generally appeared in rotation, whatever the order of precedence and of succession might be." (p. 130.)

The sensorium was generally so much affected in the fever at Hong-Kong as to procure it the name "head fever" among the men.

On examination, post mortem, the following were found to be the most constant and striking appearances :

"In the worst cases [in which reaction did not take place, but the patient died in the invasive congestive stage] nothing was detected excepting a loaded state of the large venous trunks, and a certain degree of venous congestion of dependent parts in the thorax and abdomen. The brain and its investing membranes were more exsanguined, and consequently paler than in normal condition, there being no serous effusion either under the meninges or into the ventricles. Neither here nor in other parts was there any thing which, with the utmost latitude of language,



would be considered inflammatory, or having the least approach to it. The vascular condition of the tissues was, indeed, the very reverse of that which constitutes inflammation."

"When the disease was more protracted, became distinctly paroxysmal, and did not terminate in death for some days—from five to fifteen—the organic appearances had a similar pervading character, but were to a certain extent modified with additions. What could be ascertained of abnormal in structure, was in these, as in the former, with little exception, congestive, not inflammatory, especially manifested in the lungs. . . . . When fever had followed flux, which was not uncommon, ulceration was occasionally found in the colon, the result evidently of the preceding affection. The integrity of organs, including the liver and spleen, when the violence of paroxysms in many cases is considered, is surprising." (p. 135.)

From a consideration of the morbid phenomena and the pathological appearances, Dr. Wilson concludes that the fever was an idiopathic affection, and not symptomatic of local disease; that it was "primarily and emphatically a disease of the whole system, though it might not affect every part of it equally at the same instant;" and that it was the product of pestilential miasm, acting in many respects like a concentrated poison.

On comparing the remittent fever observed in China with the proper endemic of the West Indies, our author observes that there is much resemblance

"In the apparent want of consent between the real force of the affection and its external appreciable manifestation. Here, as there, when there is no complaint made or admitted, and while there is no violence in the febrile signs, it not unfrequently happens that the subject is suddenly seized with hurried respiration, fluttering pulse, partial unconsciousness, or slight coma—a condition which speedily terminates in death. But while there is this point of agreement, there are many points of difference between the two forms of fever. It would be tedious to go over the whole, which, taken together, form a distinct line of diagnosis, but one may be noted in passing. In West Indian fever, death generally ensues, or convalescence is fairly established in a few days; for the most part, within a week, and when established it proceeds, steadily in most cases, speedily to completion. Relapse is remarkably rare, and the subject in a short time is as well, strong, and little liable to disease as he was before the attack. In the Hong-Kong fever, on the contrary, when it is subdued, its duration being very uncertain, intermittent fever or flux, as already stated, constantly sets in. There is great proneness to relapse; and after the disease has been apparently subdued, and the tendency to recurrence overcome, the subject continues long listless and emaciated, has a sallow countenance, with pale lips, and hovers on the verge of jaundice, dropsy, or fatal flux." (p. 133.)

The prevalence of fever at Hong-Kong is to be attributed to the same cause as at Chusan, and in the Yang-tse-Kiang, marsh miasm. Notwithstanding the rocky character of the island, rice cultivation was pushed to as great an extent as possible; not only were the level grounds applied to this purpose, but the sides of the hills and ravines were cut into terraces, and by dint of manure and irrigation, the latter being accomplished by means of dams and sluices, were converted into artificial marshes. Since the occupation of Hong-Kong by the British these rice fields have been much neglected, and have in many places fallen out of cultivation—a state in which they are even more injurious and more active sources of endemic

disease. To this circumstance Dr. Wilson chiefly attributes the greater intensity and the type of the fever here :

" May it not be inferred," he asks, " that the exhalations from cultivated rice-ground saturated or covered with water at times, but under a regular process of management, and yielding the healthy products of vegetable growth, give rise to intermittent, while the stronger poison exhaled from marsh land, through processes of rapid and multiplied destruction of vegetable matter, under equal degrees of heat, and other apparent circumstances, occasion the more concentrated and fatal remittent fever ?"

It is much to be regretted that no steps were taken for the removal of such evident causes of diseases. The barracks for the troops have been erected in the immediate vicinity of some of these swamps, and as might have been anticipated, the soldiers have suffered from endemic disease in a much higher ratio than the naval force. There may have been cogent reasons, political or military, for the selection of such a site, but, in the name of common sense and humanity, why was nothing done to render the spot more healthy ; or if common sense and humanity are to go for nothing, why was not a system of draining adopted as a measure of economy ?

" A few workmen," says our author, " with spades in their hands, under the simplest direction, would do all that is required, at any rate for present benefit, and the benefit would probably be most important ; nay, in many places, all that is necessary is to remove the artificial obstructions that have been formed to the natural flow of the surface-water—a practice common in China, and consonant with Chinese notions of salubrity, as well as of productiveness, but which should surely not be permitted now in Hong-Kong. There is a considerable portion of the surface close to West Point, where barracks were built, and last year abandoned, on account of the ravages of periodic fever, which might be rendered inoffensive to the sense, and probably innoxious to health, in a few days, at the expense of a few dollars ; and there are many such places in the neighbourhood." (p. 187.)

If we consider that to replace each soldier, who has fallen a sacrifice to fever arising from this neglect of the most ordinary precaution, costs the government not less than £100, at the very lowest estimate, we may safely conclude that this has been, to say the least of it, a most expensive economy of dollars.

The amount and intensity of disease was much less in the hot season of 1844 than the preceding year, without any obvious reason for the difference. This is a very common case in countries where the prevalent diseases are principally of endemic origin, a comparatively healthy season succeeding a sickly one without any apparent cause by which to account for the circumstance. Adverting to this peculiarity our author judiciously remarks :

" On such a difference as this is often raised many a lofty but indifferently founded pretension to improved methods of treating disease ; and the vaunted superiority has its origin less, it is believed, in want of candour, than inattention to, and ignorance of, the dissimilar force with which the same disease acts at different times. A practitioner arriving here in the summer of 1844, would find a much larger proportion of the sick under his care recover than did the practitioner of 1843 ; he would probably, with a laudable desire to accomplish more than the men earlier in the field had done, modify the means of treatment used

by them or apply something different; and to this modification or alteration of means, not to the comparatively slight morbid impression, he would be apt to ascribe his better fortune, counting it the effect of professional merit, not of changed circumstances. Having satisfied himself of this, he, and some others who are prone to believe much and to hope much, readily arrive at the conclusion that his predecessors were inefficient practitioners; that they did not understand, or did not properly perform, what was required of them by the obligations of their office, and that they are therefore chargeable with the heavy crime of having allowed men to die who might have been saved." (p. 189.)

Surely, such considerations, backed as they are by facts, should lead us to judge more leniently of the want of success which may occasionally attend the practice of any of our professional brethren, and to look with distrust upon the more fortunate results which may, perchance, follow our own treatment.

We have hitherto noticed the pathological appearances in the fever cases only; the following is a brief description of those usually found in the men who died of dysentery at Chusan:

"Ulceration has been all but universal, and obviously in most instances of long standing, the ulcers being scattered more or less thickly over the whole of the colon and rectum, of various forms and superficial dimensions, generally circular, and from the diameter of a split-pea to that of a shilling or more, the surface being purulent, hemorrhagic, ragged, or clean and smooth, like the effect of excision; of various depths, sometimes destroying the mucous tissue only, sometimes the muscular also, being generally arrested by the peritoneal, but occasionally perforating it. In most cases the rectum has been much thickened and indurated, in many assuming a semi-cartilaginous character; in other cases it and the colon have been attenuated in some places while they were thickened in others; and in both at spots free from absolute ulceration, the mucous lining has been loose and lacerable or abraded, frequently there has been serous infiltration of the peritoneal covering, the mucous lining having a leaden hue, through which light-coloured spots were sometimes interspersed, giving it there the appearance of dark marble; occasionally it had a purple colour, with a rough granular surface, resembling the exterior of a mulberry; and it has not unfrequently happened that the whole tube has been so much softened, broken down, and disorganized as to tear on the slightest touch, or fall to pieces on being lifted. With scarce an exception, the organic lesions have been limited by the ileo-cæcal valve, the small intestines and stomach presenting a perfectly healthy appearance, although venous congestion, and small spots, denoting increased vascular action, have been found occasionally. When it is said that the small intestines and stomach presented a perfectly healthy appearance, it should be added that portions of the former, generally the distal extremity of the ileum, were often, in cases of long duration, much attenuated, and pale, and diaphanous like a prepared colourless membrane." (p. 56.)

Dysentery presented the same general features at Hong-Kong as at Chusan, but was more frequently attended with some affection of the stomach and small intestines.

"Vascular patches, occasionally diffused inflammation tending rapidly to disorganization, often appear in the stomach; similar appearances are found in the small intestines, where ulcerated spots have besides been detected. The numerous autopsies which have taken place show that the primary and permanent source of the intestinal symptoms has been in the large, lower portion of the tube; but it is not, as was almost universally the case at Chusan, confined in a great degree to the ileo-cæcal valve, the cæcal portion of the colon and rectum. On the

contrary, the transverse and descending portion of the colon as well as the rectum, are deeply involved; being livid, thickened, indurated at one point, softened at another, abraded, and ulcerated, ulceration occurring in small section-like points, or deep excavations, often penetrating to, occasionally through, the peritoneal membrane, which is generally tumid from serous infiltration. The rectum is hypertrophied and ulcerated in an especial degree, and in some instances there is a great amount of vascular turgidity and irregular thickening of a mulberry appearance on the mucous surface, from which, and through the ulcerous openings, the unmixed blood and brown-coloured homogeneous dejections which occurred soon before death, occasionally, were supplied." (p. 139.)

One of the most remarkable circumstances connected with the dysentery of China was the complete absence of diseased appearances in the liver. Considering the frequency with which hepatic disease occurs in India in conjunction with dysentery, this fact is very striking. After three months' experience at Chusan, Dr. Wilson observes that it would seldom happen, in so many cases of post-mortem examination, instituted any where, without selection, that this organ should be found so thoroughly entire; and at Hong-Kong in like manner, he remarks, "it very seldom happens that any alteration from normal formation can be detected."

We have confined our observations hitherto to fever and dysentery, because they are the chief causes of sickness and mortality among the force in China; indeed, all other diseases are comparatively rare and of little intensity. Notwithstanding the difference between the temperature of the summer and winter months, and the diurnal variations of the thermometer during the latter being considerable, the sailors enjoyed a great exemption from catarrhal and rheumatic affections. Phthisis also has been scarcely met with there; a fact of considerable interest when viewed in connexion with the immunity enjoyed from it by the troops in India. Dr. Wilson seems inclined, although he does not positively assert it, to attribute this to the influence of marsh miasm, remarking that "such exemption is in conformity with what has been noted in other miasmatic districts; it has often been observed that where ague prevails, consumption of the lungs is not rife." This, however, is a question still *sub judice*, and with the fact before us of the great mortality from phthisis among soldiers in the West Indies, where there is no lack either of ague or marshes, we are not prepared to admit the accuracy of this explanation.

A great disposition to the formation of lumbrici was observed; these were voided in great numbers, sometimes even by the mouth, but gave rise to no symptoms by which their existence could be ascertained previous to expulsion.

"It can scarcely be questioned," Dr. Wilson observes, "that the excessive tendency to, and occasional accumulation of them, arises out of the enfeebled, unhealthy condition of the alimentary apparatus—more particularly of the interior membranes; being infested by depraved secretions, and coated with adhesive mucus, it ceases to perform its proper functions adequately, and from the same cause becomes the prolific bed of these creatures. . . . Their existence is incompatible with a sound state of the living parts in which they are developed; and what is required for their prevention is vigorous healthy action in these parts." (p. 193.)

III. The diseases to which the natives are most subject, besides fevers, cholera, and diseases of the mucous membranes of the lungs and bowels,

are stated to be dyspepsia, scrofulous affections, ophthalmia and its results, and diseases of the skin. The causes of these are briefly, but sufficiently, accounted for in the following terms :

"Diet and the habitual practices of the inhabitants contribute powerfully to the frequency of most of their maladies, and are not without influence on them all, in connexion with the malarious influence of the locality. Their food consists almost entirely of vegetables, especially rice, with salted fish badly cured, and often in a semi-putrid state. The large quantities of weak tea drank, and opium-smoking indulged in as often as possible, combine to produce a cachectic condition of the body. Opium, however, is only an occasional indulgence as a luxury ; tobacco is considered a necessary, and is universal among both sexes. When otherwise unoccupied, they never drop the tobacco-pipe, smoking from morning till night, and drinking largely, at short intervals, a miserably weak infusion of coarse tea. With innutritious, unwholesome food, and vicious indulgence, cribbed, damp, ill-ventilated apartments, and want of personal cleanliness, will concur to occasion diseases of the lymphatic, cutaneous, and alimentary organs." (p. 22.)

That itch should be almost universal among the natives of all ranks will not appear extraordinary, when we learn that—

"Of water, except in tea-making and some culinary processes, they appear to have no practical knowledge. Bathing, as well as ablution of the person, is unknown ; and this is one of the many instances in which they differ from all other people, especially those of the East, inhabiting warm or temperate regions. They go literally unwashed from the cradle to the grave ; the only thing in the shape of a substitute observed was a cloth moistened with hot water, and passed lightly over the face and hands by persons of distinction ! !"

Truly, baths and wash-houses are needed in China as much as in White-chapel and St. Giles's.

Among the diseases of the skin under which they occasionally labour, two are deserving of mention. The first is Elephantiasis, or *Barbudoes Leg* ; which prevails in China to some extent among the poor, but the cause of it is unknown.

"It consists essentially in inflammation of the subcutaneous cellular tissue which leads to effusion, the effused matter becoming imperfectly organized, occasioning tumefaction, and impairing more or less the motive power of the limb. Repeated attacks of inflammation, effusion, and consolidation produce, if not arrested, in the course of time, generally many years, the highly enlarged, tuberculated, scaly leg, resembling very closely that of an elephant."

The treatment which Dr. Wilson found most beneficial was local abstraction of blood by incision or puncture, according to circumstances.

The other disease to which we alluded is an affection of the same general character with elephantiasis, which attacks the genitals, and is thus described by our author :

"It begins in the scrotum, but soon affects the other parts, producing immense enlargement, agglutination, and confusion of the different tissues, eventually destroying their entire structure. The morbid mass sometimes acquires such dimensions as to descend far below the knees, and requires the limbs to be kept wide apart to make room for its encroachments. In one instance, not at Chusan, the urethra was found by the author eighteen inches below the seat of the tumour. It was discovered only on examination, in front of the tumour's uniform surface, all appearance of the penis being lost. In this affection, as well as that of the limbs, it is probable that early, often repeated, and free incisions, would prove highly beneficial." (p. 25.)



IV. A section is devoted to a short and general sketch of the treatment adopted in the cases of remittent and intermittent fevers, dysentery, and ulcer. We do not intend to follow our author into the details, but would recommend the chapter for the perusal of all—more especially of young medical officers who are likely to be employed in tropical climates. It contains many judicious suggestions, and will make them pause, we trust, before adopting a mere routine system of treatment of these diseases, or concluding that the only hope of success in dysentery is from *throwing in calomel*.

“Routinism,” our author well observes, “the besetting sin and bane of medicine, is apt to be indulged in treating tropical diseases, on account of a certain degree of uniformity in their character, and in nothing is the tendency more displayed than in the use of mercury. Calomel, its most favorite, and, on the whole, best preparation, is a remedy which is at once possessed of great power, easy of application by the practitioner, and generally acceptable to the patient. It is, therefore, capable of effecting great good, but is also, and for the same reasons, converted into an instrument of much mischief. Every one has observed its valuable qualities, and occasionally in circumstances which, but for its agency, appeared desperate. Hence has arisen the disposition to ascribe more to it than it has deserved, to carry it further in other cases than was safe, and to employ it in others again, in fact, unlike, though somewhat similar on the surface, to those in which it had proved powerfully remedial. In the use of no medicine is it more necessary than in this to determine what is desired and expected from its operation; and to be fully satisfied, before prescribing it, that the condition that makes such effects desirable, really exists.”

The last remark is one deserving of serious consideration by the medical practitioners in this country also, and not in its application to calomel alone, but to every medicine we prescribe. “*Cui bono?*” is, we fear, a question too seldom asked in writing a prescription.

We have already expressed regret that our author published his work in the form of notes, instead of writing one of a more systematic character. He would then have had an opportunity of lopping off a few redundancies, of bringing his facts more compactly together, of adding materially to the numerical information, and of entering at greater length into several very interesting questions, which he has barely touched upon. The literary execution of the work also would have benefited, as it is natural that notes, written in the bustle of numerous and arduous professional duties, should, in this particular, possess many defects. But in its present shape it is still an excellent and valuable work, and deserving a careful perusal. We have adverted only to a few of the leading topics in it; it contains a large amount of interesting information regarding the topography of Chusan and Hong-Kong; the habits and diseases of the Chinese; the sickness among the troops employed in the expedition, and the causes which produced it; with many useful hints on the means of preserving sailors on shipboard, and of rendering our newly acquired colony in the China seas more salubrious. Into these points we cannot at present enter, but must content ourselves with recommending the work to our readers as one fraught with valuable knowledge, and suggestive of many important reflections. We shall conclude with one more quotation from it. After noticing how little we are able to effect by medicine in many forms of disease, and how often, notwithstanding the boasted improvements of science, we return to the same



course of treatment which was adopted by our forefathers, our author asks acutely and answers wisely :—

“To what decision should such things lead? Not to the abandonment or careless pursuit of improvement certainly, but to make us more laborious and rigid than we too often have been, in investigating the fitness of means before they are used, more vigilant in observing, and scrupulous in recording them, and more certain that we do not parade as new and useful, instruments which are old and worthless, or boast of their superior efficacy in cases unlike those in which they have been found unavailing by others. The careful culture of an art, still so indeterminate in many of its applications, cannot be too much encouraged or earnestly pursued; for while so employed, although the immediate object should be missed, it may happen to us, as it happened to the men of old digging for hidden treasure, that we shall turn up something more valuable than that for which we were searching.”

#### ART. XVI.

*De Quantitate relativâ et absolutâ Acidi Carbonici, ab Homine sano et aegroto exhalati.* Auctore ADOLPHO HANNOVER, Med. Lic., Med. secund. nosocomii Regii Fredericiani, &c. &c.—Hafniæ, 1845.

*On the relative and absolute Quantity of Carbonic Acid exhaled by Man in Health and Disease.* By ADOLPHUS HANNOVER, M. D., &c.—Copenhagen, 1845. 8vo. pp. 91.

THE object which Dr. Hannover has proposed to himself, in the investigations detailed in this little work, has been to compare the amount of carbonic acid exhaled by patients suffering under certain diseases,—to wit, phthisis, chronic bronchitis, and chlorosis,—with the healthy standard. Towards the determination of the latter he does not afford any new data; but contents himself with bringing together, and comparing, the results obtained by Allen and Pepys, Lavoisier and Seguin, Dr. Prout, and others among the earlier experimenters, with those of Andral and Gavarret, Professors Scharling, Valentin and Brenner, and Vierordt, in more recent times. The following summary affords a useful digest of the conclusions to which these combined results seem fairly to lead, with the authorities for each statement.

“1. The relative and absolute quantity of carbonic acid exhaled varies in different individuals, and in the same individual under different conditions.

“2. In both sexes the absolute quantity is governed according to the age. (Scharling, Andral, and Gavarret.)

“3. A greater absolute amount is exhaled by the male than by the female (Scharling) at every period of life, from the age of eight years to old age; but the difference is peculiarly obvious between the ages of sixteen and forty, when the male usually exhales nearly double the quantity exhaled by the female. (Andral and Gavarret.)

“4. In the male the absolute quantity increases from the age of eight years to that of thirty, and undergoes a sudden increase at the time of puberty; from the age of thirty the quantity diminishes, and this in proportion to the advance of age. (Andral and Gavarret.)

“5. In the female, also, the quantity increases up to the time of puberty, but, as soon as menstruation is established the quantity remains unchanged; whilst,

on its cessation, the quantity suddenly undergoes a distinct increase, and then diminishes with the advance of age. During pregnancy the absolute amount rises to the standard of the non-menstruating female. (Andral and Gavarret.)

"6. In either sex the absolute quantity has a simple relation to the vigour of the bodily constitution, and to the evolution of the muscular system. (Andral and Gavarret.)

"7. In proportion to the weight of the body the infant exhales the largest absolute quantity, and the youth more than the middle-aged man. After the age of forty the quantity decreases, and the female seems to exhale a considerable amount in proportion to the weight of her body. (Scharling, Andral, and Gavarret.)

"8. The relative and absolute quantity increases shortly after food has been taken, and whilst digestion is going on; it is greater in the state of satiety than in that of fasting. (Prout, Valentin and Brunner, Lavoisier and Seguin, Scharling, Vierordt.) Imperfect digestion diminishes the absolute quantity. (Lavoisier and Seguin, Scharling.) After the use of alcohol in any form and quantity, especially when the stomach is empty, and even after the use of strong tea, the relative and absolute quantity becomes less. (Prout, Vierordt.)

"9. A moderate amount of muscular movement increases the relative and absolute amount of carbonic acid exhaled. (Prout, Scharling, Vierordt.)

"10. Depressing passions of the mind diminish the relative quantity. (Prout.)

"11. Frequent and deep respiratory movements augment the absolute quantity of carbonic acid exhaled (Allen and Pepys, Andral and Gavarret, Vierordt), but they diminish the relative amount. (Vierordt.) The frequency of the pulse seems to be of no consequence.

"12. The relative quantity is increased by the repeated inspiration of the same air, whilst absolute is diminished. (Allen and Pepys, Scharling, Vierordt.) The absolute quantity appears to be increased by the respiration of pure oxygen. (Allen and Pepys, Nysten.) When the respiratory movements are impeded, or are altogether brought to a close, the relative quantity of carbonic acid contained in the air of the lungs is increased. (Vierordt.)

"13. By night and during sleep the absolute and relative quantity is less than during the day and in the waking state (Prout, Scharling); the relative quantity is less in the morning than in the evening (Brande); but, before noon, the relative and absolute quantity is greater than in the evening. (Prout, Vierordt.)

"14. The absolute quantity is greater in winter (Lavoisier and Seguin); the relative is increased by electricity and low barometric pressure (Prout, Vierordt), but the absolute is diminished by greater rarity of the air. (Vierordt.)

"15. An increase of temperature diminishes both the relative and absolute quantity. (Vierordt.)

"16. The transpiration of carbonic acid through the skin is small in amount; the greatest activity in this function being in a rigorous adult male (Scharling)." (pp. 33-5.)

The whole subject of the cutaneous transpiration of carbonic acid seems to us to require much more careful consideration than it has yet received. We are inclined to believe that the cutaneous respiration is a very important adjunct to the pulmonary; and, in particular, that it may become greatly increased in amount in certain diseased states, when the pulmonary respiration is imperfectly performed. The only way to ascertain the truth on this subject, would be to employ such a closed chamber as that constructed by Professor Scharling, into which the product of the cutaneous respiration might freely pass; whilst the pulmonary respiration during the same period should be measured by a distinct apparatus, such as that employed by Andral and Gavarret, or by Valentin and Brunner; and to sub-

ject to this test not merely healthy subjects, but others suffering under various morbid conditions, especially such as produce obstruction to the due action of the lungs.

A larger number of experiments, moreover, seems yet wanting to determine the limits of variation in the absolute quantity of carbonic acid exhaled under different circumstances, as regards temperature, exercise, &c. This is a subject to which the attention of our arctic and antarctic voyagers might be advantageously directed, as well as that of residents in climates where the external temperature occasionally equals that of the body itself. As an average statement of the total quantity of carbon exhaled by a male adult of ordinary vigour, and leading a life of moderate activity, we think that the estimate of Scharling is entitled to the greatest weight; the amount deduced from his experiments on respiration in a closed chamber being 4080 grains, or  $8\frac{1}{2}$  oz. per day; whilst the estimate formed upon a calculation of the carbon contained in the food and in the excretions of a body of sailors (in whom we should expect that a larger exhalation would take place, conformably with the muscular exertions they employ, and the vicissitudes of temperature to which they are exposed), makes the daily excretion of carbon in a gaseous form no more than 5040 grains, or  $10\frac{1}{2}$  oz. And as this last estimate is founded, not upon the actual amount of provisions consumed, but upon the amount given out, of which a considerable part is wasted, it seems next to certain that some error must exist in the calculations of Liebig, who gives 6672 grains, or nearly 14 oz., as the average quantity of carbon daily exhaled by each individual of a body of soldiers in garrison.

Our information as to the amount of carbonic acid, whether relative or absolute, exhaled in morbid conditions of the body, is very imperfect. The researches of Nysten seem to lead to the following conclusions:

“1. In chronic diseases, without fever or pulmonary disorder, the chemical phenomena of respiration differ but little from those of normal condition.

“2. The air expired in severe continued fevers sometime appears to contain a slightly-increased proportion of carbonic acid; but new experiments are required to determine whether this is a constant or an accidental occurrence.

“3. In certain diseases the expired air contains an unusually small proportion of carbonic acid; this is especially the case in severe dyspnoea, whether this result from degeneration of the tissue of the lungs, or from hydrothorax, ascites, or other causes interfering with their due expansion.” (pp. 35-6.)

The well-known observations of Macgregor, relative to the decided increase in the amount of carbonic acid exhaled from the lungs during the acute stage of smallpox, measles, and scarlatina, and upon the augmentation which was presented in a fatal case of extensive ichthyosis, are well known; but we think it doubtful whether this increase was due to an absolute increase in the quantity of carbonic acid exhaled, or whether it was not rather referable to the stoppage of the cutaneous respiration, which would throw more work upon the lungs.

We have now to notice the researches of Dr. Hannover which were carried on in conjunction with Professor Scharling. We hope that these are to be taken but as an earnest of more extended observations; since at present the amount of information contributed by them is very limited. In four cases of chlorosis, the amount of carbonic acid exhaled appeared

to be above the average of that of menstruating females of corresponding age and bodily development; thus harmonizing with the statement of Andral and Gavarret; but this remarkable fact presented itself in addition, that an increase in the number of respirations per minute diminished, not merely the amount of carbonic acid exhaled at each expiration, but the total amount given off per minute. Thus the number of respirations per minute being, respectively—17·3, 20·4, 25, and 37·8, the hourly exhalation of carbonic acid was—123·6, 118·6, 116·9, and 106·3 grs. thus showing a regularity of diminution corresponding with the increase of frequency, which could be scarcely attributed to other circumstances, more especially as the remarkably increased frequency in the last case was accompanied by a strongly-marked diminution of the exhalation.

In five cases of phthisis, a decided diminution was apparent in the total amount, excepting when exercise taken a short time previously had greatly augmented the rate of the respiratory movements,—this augmentation not being accompanied by a diminution, as in the preceding case, but by an increase in the amount of carbonic acid exhaled. The diminution, as compared with the healthy standard, was absolute, not only in itself, but also when due allowance was made for the loss of weight in the diseased state.

In two cases of chronic bronchitis, there was no decided departure from the normal standard. The same may be stated as to two cases of jaundice, one case of morbus cordis, and one case of morbus Brightii. A case of lithiasis is added, in which the respiration seemed much below the normal standard; by a strange omission, however, although a short summary of the case is given, no information is afforded as to the chemical nature of the calculi which was voided whilst the patient was under observation.

Besides the details of his experiments, Dr. Hannover propounds some interesting speculations founded upon their results, with regard to the connexion of the respiratory function with different states of the system, natural and morbid. Into these, however, we shall not follow him; since it appears to us that a much larger mass of data must be collected, before it is safe to begin to build upon them. We may remark, however, that these results so far as they go, afford additional evidence against the assertion of Liebig, that phthisis is a disease of increased oxidation,—the very reverse being apparently the case. We need, however, to be informed, whether there is any increase in the cutaneous respiration in the advanced stages of this disease, when a large portion of the lungs is rendered unfit for the discharge of their function, and when the temperature of the body is sustained at its normal standard, or even higher. Again, the marked augmentation in the exhalation of carbonic acid in chlorosis, taken in connexion with the diminished amount of red corpuscles in the blood, in that disease, seems adverse to the view of Liebig, that the red corpuscles are the chief carriers of oxygen in arterial blood, and of carbonic acid in venous. We apprehend, however, that this increased exhalation of carbonic acid must be regarded as compensating for the deficiency of other secretions, and not as indicating any unusual activity in those interstitial changes to which the act of respiration is subservient.

## ART. XVII.

*A Practical Treatise on Abdominal Hernia.* By THOMAS PRIDGIN TEALE, F.L.S., Fellow of the Royal College of Surgeons, and Surgeon to the Leeds General Infirmary. With numerous Illustrations.—*London*, 1846. 8vo., pp. 383.

At the head of our table of contents stands the announcement that we are "critical and analytical reviewers." We need not, therefore, argue at any length that it is our especial function to analyse and criticise whatever is submitted to us for our perusal and candid opinion: and we are free to confess that when we have performed *both* of these duties, we lay down our pen with peculiar complacency and self-satisfaction. Yet it does occur to us at times to be foiled in this our laudable desire to fulfil our engagements with the public; which is of course no fault of ours, but is to be charged exclusively on those who favour us with their books. Some are pleased to write in such a mystical strain, and their meaning is wrapped up in such unintelligible language, or expanded into such vague generalities, as to defy our power of *analysis*; and we are forced to acknowledge ourselves blissfully ignorant of the great truths which are haply concealed from our ken. Others, again, are below *criticism*; and we are then sometimes tempted to lay aside our dignity, whilst we expend our spleen for the amusement of our readers and the wholesome chastisement of ignorant pretenders. But there is yet a third class of writers who no less baffle our efforts to criticise,—who, in short, write so cautiously and so well,—who advocate such sound practice, and broach such sensible theories, when they theorise at all, as to leave the *honest* critic without a handle for his weapon: for them his occupation is gone, and he is driven to the necessity of satisfying his readers by a simple analysis of the work he is reviewing,—itself no easy task, where neither tempting novelty is promised nor originality professed. To this last class belongs the book before us; which we at once pronounce to be unexceptionably good in every respect, whether literary or professional; and fulfilling to the utmost the stated object of its publication.

We are indebted for the present treatise, in its existing form, to the suspension of the work for which it was intended as a contribution.\* It further appears from the author's statement, that the article on "Hernia," for this publication, was to have been undertaken by Sir Astley Cooper, whose decease, however, occurred before he had commenced the task. Mr. Teale was consequently requested to supply the article in question; but the work itself, in the mean while, became defunct, and our author was thus "kindly released from his engagement by the editor and publisher, when more than two years had elapsed since the last part of the work appeared." Additional matter and woodcuts, together with a reconsideration of the whole subject, "with a view to publication in an independent form," completed the preliminaries for the appearance of the volume before us.

As it is not very long since we devoted an article of some length to

\* We presume *The Cyclopædia of Surgery*.

the subject of hernia, in reviewing the works of Lawrence, Malgaigne, Verdier, &c.,\* we shall avoid as much as possible going over the same ground again; at the same time that we are anxious to do justice to a volume which, though unpretending in its extent and scope, possesses no ordinary merit as a concise systematic treatise on this important branch of surgery.

Mr. Teale's work is divided into two "Parts," the first of which comprises a "general consideration of abdominal hernia," and the second treats of "abdominal hernia considered in reference to its species and varieties." Each of these parts is subdivided into chapters, of which we now proceed to lay a brief analysis before our readers, together with such observations and illustrations as we may deem applicable.

Some remarks on the anatomy of the abdominal walls constitute an appropriate introduction to the surgery of these parts; and a clear understanding is at once established between the author and his readers as to the strict meaning to be attached to certain terms which admit of different interpretations,—a licence which should be inadmissible in the nomenclature of any branch of science. For the most part we approve of the definitions adopted by Mr Teale; and especially of those which have reference more strictly to the ultimate anatomy of textures: such, for instance, as the substitution of "filamentous" for "cellular" tissue. The fascia iliaca and fascia transversalis are severally denominated "internal" and "external" abdominal aponeurosis; and the term "fascia" is restricted to "membranous expansions of areolar or filamentous tissue." This, again, we should not object to, if the sheet-like expansions of tendons were not thus deprived of their appropriate appellation: and on this account we should have preferred expressing the latter structures by the word "aponeurosis," and confining the term "fascia" to analogous and often continuous textures, such as that which invests the muscles of the leg, thigh, &c. The restriction of the title "subcutaneous filamentous tissue" to all superficial fasciæ, so called, has our entire concurrence.

After a brief notice of the "hernial apertures," our author proceeds to make some remarks on the "hernial sac," its mode of formation, forms, and varieties. With respect to the latter subject of inquiry, Mr. Teale adopts M. Cloquet's division into four primitive types; "the cylindroidal, spheroidal, pyriform, and conoidal;" and appropriate illustrations accompany this part of the letter-press. We need scarcely remind our readers that this classification, though very useful as far as it goes, by no means includes the infinity of forms which a hernial sac may present. The structural alterations which the sac undergoes in the course of time, is an important subject of consideration, as bearing upon the not infrequent seat and source of stricture; and the most important of these changes which Mr. Teale notices, is that of "the gradual conversion of the filamentous or areolar tissue into the white fibrous element." "I have," he says, "in my possession the sac of an old direct inguinal hernia, around the neck of which are numerous opaque, white, glistening fasciculi, precisely similar in their physical characters to those of which the dura mater is composed." We have ourselves dissected an old and large hernia presenting a similar



character, though the glistening fibrous appearance was extended for some little distance on the sac, and consisted of scattered bands, which we were disposed to identify with the tendinous structures through which the hernia passed, rather than to the development of a new texture, or the modification of the texture of the sac itself.

The "Constituents of herniæ" (which is the subject of the next chapter) are almost as various as the contents of the abdomen: thus, not only have the membranous viscera contributed to such formations, but the uterus, kidneys, liver, &c., have been found as part of the contents of a hernial sac. One remarkable case Mr. Teale cites, in which Mr. Nourse removed the ovaries which formed a hernial tumour in each groin of a young woman in St. Bartholomew's Hospital. "The tumours had become so painful as to incapacitate her from pursuing her usual avocations, on which account she desired their removal. After the operation she enjoyed good health, but ceased to menstruate." Mr. Teale also points out the peculiar anatomical relations of the cæcum, as influencing its hernial state, remarking that "after it has descended as low as the internal ring, its posterior surface is the part usually first protruded into the inguinal canal. Hence, when the protrusion is of small size, it is constituted by a portion of the cæcum altogether devoid of peritoneum, and a true hernia without a sac is produced." Again, "as we can seldom be quite certain, before operating, whether the cæcum or the floating viscera constitute the hernia, we should bear in mind, even in small herniæ, the possibility of the cæcum being protruded, and also the anatomical peculiarities of this form of hernia; but still more should we remember the possibility of this being the case when the herniæ of the right side are unusually large." We may add that, as it is not at all unfrequent to find the cæcum entirely encased in peritoneum, and to a certain extent floating in the iliac fossæ, the above relations of this portion of large intestine are not constant. There is considerable analogy between the ordinary relations of the cæcum and the bladder, as regards their serous investment; and the latter organ has been in many instances found to occupy the sac of a scrotal hernia. But this condition, as Mr. Teale remarks, can only exist under circumstances which have a tendency to produce an abnormal state of this viscus, and which enable it to find its way through the hernial apertures when undistended. Such are protracted repletion during pregnancy, or more frequently, diseases of the prostate and urethra, which impair the contractile power of the bladder, especially in the aged; and it thus remains "in a flaccid state in the immediate vicinity of the abdominal or femoral rings." The absence of a peritoneal sac constitutes the analogy between this form of hernia and the cæcal protrusion. When once within the scrotum, the urinary bladder may become gradually distended, and retention of its contents may supervene. Our author reminds us that our earliest information respecting hernia of the bladder was derived from cases which had been mistaken, we presume, for hydrocele, and punctured. The complication of calculus with cystocele has been met with in many instances; and the removal of the stone in many cases, and its spontaneous escape in others, has been known to effect a cure.

In a former article, already referred to, we entered somewhat at length into the statistics of hernia, and shall therefore pass by this division of

our subject; especially as the authorities quoted are the same as those from which we drew our own materials. Great, we had almost said undue, importance is attached by our author to M. Malgaigne's calculations and tables.

The causes of hernia are divided by Mr. Teale into the "predisposing" and "exciting;" and of the former, inordinate size of the hernial apertures of course stands prominently in relief. We believe, with our author, that deficiency of the intercolumnar bands is a frequent cause of this abnormal size of the external ring; and we may add that the structure known as the "triangular ligament, or Colles's fascia," varies much in its density and development, and thus probably constitutes a negative predisposing cause to direct hernia. Amongst the ordinary *exciting* causes, the fair part of the community would do well to remember that all writers include "pressure from stays," by which the bowels are driven to take refuge where they may, so that the waist has its due proportion of compression.

The "effects of hernia" are briefly noticed, and then our author proceeds to the consideration of its various conditions. In speaking of reducible hernia, the circumstances which indicate the presence of omentum, as distinguished from intestine, are well set forth: its flabby and compressible character, its freedom from tension, uneven surface, ill-defined outline, and its restoration to its normal position in successive portions, render it highly probable that omentum is contained with the sac. Again, the importance of distinguishing between the symptoms resulting from inflammation, and those which indicate strangulation, are properly dwelt upon, and this more especially when the hernia is omental. "In inflamed hernia, the pain in the first instance is referred to the *body* of the tumour; while in strangulation, when the hernia is large, the *site of stricture* is more particularly the seat of pain. Again, in inflamed hernia, the ring is generally free from tension, whilst the swelling itself is tense." Mr. Key's high authority is then quoted to support the importance of this distinction, as, says that gentleman, "in the inflamed state of the omentum, without strangulation, the operation will afford no relief; on the contrary, it will aggravate the inflammation."

The obstruction of an irreducible hernia may, it is well known, produce symptoms very analogous to those of strangulation, although there be no stricture at the mouth of the sac. Attention has been recently called to this subject by Mr. Stephens,\* but we have only room for our author's summing up of this gentleman's account, and the cautious advice which he appends thereto.

"The facts which have now been deduced are sufficient to prove, in the first place, that dangerous, if not fatal, obstruction of the bowels may occur, in consequence of the intestine being fixed in an angular or distorted form by adhesions between the protruded viscus and the sac; and, secondly, that such a state of obstruction may sometimes be relieved by dividing these adhesions.

"But whilst these facts should never be lost sight of, nor be without their full influence in practice, it is necessary to use caution against committing the dangerous error of imputing to adhesions *all* obstructions occurring in hernial subjects not labouring under strangulation, and of rashly proceeding to attempt their removal by operation.

\* *Treatise on Obstructed and Inflamed Hernia.*

"It must be remembered that the cases which justify this mode of proceeding are extremely rare; that the very adhesions which are supposed to have produced the obstruction have existed probably for years; and that it may depend upon some temporary superadded cause, as the presence of crude undigested food, which, by the natural efforts, or by medicine, may be removed. I may also add, that there is no necessity in these cases for hasty interference, since their progress is far from rapid; and consequently, that time is afforded for the judicious employment of less hazardous measures. Above all, it should be remembered that there is much greater danger in opening the sac of a hernia which is not strangulated, and thus exposing the general peritoneal cavity, than in opening the sac of a strangulated hernia, which is usually excluded from the general serous bag by plastic effusion near the mouth of the sac." (pp. 52-3.)

In the judiciousness of these observations every practical surgeon must coincide. In speaking of the periods of life at which strangulation may occur, Mr. Teale informs us that he had occasion to operate on a child under two years of age; and Dupuytren has operated as early as twenty days. These, however, are rare exceptions to a general rule; for it would appear from the statistical tables of Mr. T. W. King, quoted by our author from the Guy's Hospital Reports, that most herniæ exist for years before they become subject to dangerous strangulation. Our own observation certainly tends to bear out this conclusion, though we are disposed to refer the fact, in part at any rate, to the heedlessness occasioned, especially amongst the lower classes, by familiarity with a complaint to which they have long been subject. Such, at least, is our impression in looking back to the history of many cases which have come under our notice in hospital practice; yet we cannot deny that the more scientific explanation of Mr. King may have some share in producing the effect; viz. "a certain decline of vigour and health, connected with manifest deterioration of the great depurative organs of the body in persons of middle and advanced age, which renders the protruded part more liable to tumefaction, so that it becomes strangulated in consequence of its own ready turgescence."

The pathological effects of strangulation upon the involved textures are well described, and the deleterious nature of the effusion in diffuse peritonitis pointed out. It is true that this is a point acknowledged by all, and too well known to many who have experienced in their own persons, or watched in others, the virulent power of this poison. Yet the subject, as illustrated by Mr. Teale's own observation in one instance, is of such painful interest to a large proportion of our readers, that we venture, at the risk of being thought tedious by some, to extract it.

"One evening, at the dissection of the body of a patient upon whom I had operated for strangulated hernia, several surgeons were present. Of these, two attended one case of midwifery each during the following night, and a third three cases. The two patients attended by the first two surgeons died of puerperal fever. Two of those attended by the third surgeon also died; and his third patient escaped death from this formidable malady with the greatest difficulty, after having been in extreme danger several days. It is an important fact that no other cases occurred in the practice of these gentlemen." (p. 62.)

We could have wished Mr. Teale had mentioned how far these gentlemen meddled with the subject in question, or whether either of them abstained from handling the diseased parts. It is truly an impressive warning.

Constipation is justly regarded as so constant a symptom of the mechanical obstruction arising from strangulated bowel, that it is requisite the practitioner should be on his guard against being misled by the absence of this sign, when other evidence of a conclusive character is present. Thus, Mr. Tyrrell made the remark in several cases which came under his notice, that there was a free action of the bowels when perfect strangulation existed; and this he accounted for, no doubt correctly, by supposing that "the lower intestine had been much loaded at the time the hernia was strangulated; and that either the injection stimulated it to act, not once only, but three or four times, or that the sympathetic influence of the purgatives taken into the stomach had produced an action on the lower part of the bowels."<sup>\*</sup> It is also, on the contrary, right to bear in mind, as our author remarks, that in strangulated omentum "constipation is less complete, for stools may, in most instances, be obtained by purgatives and clysters; yet sometimes constipation attends strangulated epiplocele, but it may, in most instances, be traced to peritonitis which has supervened."

In the chapter on the treatment of reducible hernia, the means proposed for effecting a radical cure are noticed, from Paulus Ægineta down to Velpeau; such as excision of the testicle; incision of the sac; excision, suture, and cauterization of the sac; ligature of the sac; acupuncture, &c. No one of these operations has received the sanction of the profession, though some of them have been attended with partial success, and have thence acquired a transient reputation. Langenbeck,<sup>†</sup> for instance, "exposed the sac, and applied a ligature to the neck close to the ring, without detaching the sac further than was necessary for the application of the ligature." His statement of the result of this operation is that he had "performed it twelve times with the most successful results, and all the patients are capable of the heaviest labour without wearing a truss." The *modus operandi* of the ligature is analogous to that of a similar application on an artery. Yet the risk of this operation, to say nothing of the attendant suffering, caused it, in common with others, to be abandoned. The plans by injection (Velpeau's) and acupuncture (Bonnet's) are briefly dismissed by our author, with the remark that a repetition of them would be altogether unjustifiable; their results indeed can lead to no other conclusion. M. Belinas's mode of treatment by the introduction of gold-beater's skin into the mouth of the sac seemed to promise a more favorable prospect of success; but although the required adhesive inflammation appeared to be established without any important constitutional disturbance, it seemed very questionable in many of the cases whether a radical cure was effected; in some it was clear that the neck of the sac remained patent, as the herniæ reappeared. However, as Mr. Teale justly remarks, "the obliteration of the sac affords but a feeble barrier to a fresh hernial descent, and only a very slight advance is thereby made towards a radical cure of the disease." In favour of the practice of applying a ligature on the sac and its envelopes, the success of Desault and Thierry may be quoted; but it has been condemned by Sir A. Cooper, and Scarpa's opinion was not favorable to it. Mr. Teale regards it as a means whereby a radical cure *may* be obtained, but considers the attendant risk and suf-

<sup>\*</sup> St. Thomas's Hospital Reports, as quoted by Mr. Teale.

<sup>†</sup> Bibliothek für die Chirurgie, Band ii., quoted by Mr. Teale.

fering as a sufficient bar to its introduction into general practice. Lastly, this plastic era has induced MM. Velpeau and Gerdy to propose operations based upon this principle for the cure of reducible herniæ. Our space will not allow us to analyse this part of the subject, for the details of which we refer to our author, in whose summing up we concur, viz. that

"The operation can only be considered justifiable when well-directed attempts at palliative treatment have failed, and the inconvenience and suffering occasioned by the hernia are so great as to interfere seriously with the patient's avocations, comfort, or health." (p. 88.)

The treatment of "irreducible hernia" constitutes the subject of a short chapter, in which our author wisely recommends that the attempt to render this form of the disease reducible should be limited to cases of comparatively recent date and moderate size, and which are irreducible from hypertrophy; in such he advocates, where the system will bear it, the free and repeated abstraction of blood, the exhibition of purgatives, the employment of cold and compression, and the recumbent posture, with a rigid diet. All these remedial agents have been applied more or less in combination with marked success, where no permanent mechanical obstacle to the reduction existed; but when such is the case, the support afforded by a truss with a hollow pad is unquestionably the appropriate treatment to adopt.

Amongst the "agenda," when a case of strangulated hernia presents itself, the *taxis* stands first, and Mr. Teale very properly devotes some space to the necessary cautions to be observed in the employment of this powerful remedy. Let our younger readers take warning by the following illustration which he gives, of the injuries which may be inflicted by the injudicious use of the *taxis*.

"A man, aged 39, was admitted into the Glasgow Infirmary, having been twenty years the subject of a reducible hernia, which had been strangulated ten hours. During the greater part of this time a surgeon had made powerful and continued efforts to return the displaced parts, and on his admission into the hospital, the *taxis* was again rather forcibly employed. When the patient was seen by Dr. Macfarlane, he considered it improper to make any further attempts at reduction. The scrotum was much swollen and discoloured. When the sac was opened, not less than a pound and a half of dark-coloured blood escaped, a considerable quantity of which was pressed from the depending part of the scrotum. The hernia consisted of a large portion of omentum, which was covered with coagulated blood, and of nearly two feet of intestine. The omentum was bruised and lacerated, and the protruded gut was almost wholly separated from the mesentery. It contained several rents, which passed in a longitudinal direction; and into each of these openings two or three fingers could be introduced." (p. 96.)

But not only is force to be deprecated in the employment of the *taxis*, but its protracted use, even with due regard to care in other respects, is often fatal to the patient, by the loss of time which it entails. So strongly, indeed, are we impressed from experience with the importance of an early recourse to the knife, that we do not hesitate to assert our belief that a large number of the cases which prove fatal after operation owe this result to the prolonged employment of the *taxis*, and consequent waste of precious time. We are persuaded, that more success would attend the operation for strangulated hernia, if attention were paid to this circumstance; and that the unfavorable issue of cases subjected to the knife is more generally



due to the condition induced by protracted strangulation, than by exposure of the contents of the hernial sac. We would even go further, and prefer to err in occasionally operating where the stricture might perchance have yielded to persevering taxis, than risk the sacrifice of our patient by dangerous delay. Mr. Teale is scarcely less decided in his opinion on this subject; for he remarks that "of all the causes which determine the fatal issue of strangulated hernia, delay is the most frequent and dangerous." In speaking of position during the manipulation of taxis, our author says that he has on several occasions succeeded in reducing herniæ by adopting Lisfranc's suggestion of making the walls of the abdomen tense, when he has failed whilst they were relaxed. It certainly is true that the capacity of the apertures, from their peculiar conformation, admits of much less alteration by position than is generally supposed; and Lisfranc grounds his recommendation on the assumption that "it is much easier to push a substance through an opening of a given size in a tense than in a relaxed membrane:" this it is difficult to gainsay, and it is well to try the second method when the first fails.

We must pass over the clear and excellent directions given for the employment of the taxis and its adjuvants, recommending them to the careful perusal and study of our readers, and proceed to the consideration of the operation itself; and the first question which presents itself is, whether the sac should be opened, or an attempt be made to relieve the stricture without this ulterior step. Our author sets out with assuming that the fatality of cases, in which the ordinary operation is performed, is in part due to the exposure and manipulation of the protruded viscera, and ably advocates the alternative of dividing the stricture external to the sac. It is true that if this attempt fail, the sac may then be opened, and therefore, if care be taken not to push up the still strictured hernia with its sac into the abdomen, there can be no objection to pursuing this course. We repeat our conviction, however, that much of the mischief which has been attributed to the exposure of strangulated intestine, is really due to the impression made by the stricture, and thence propagated even to the interior of the abdomen. On this still mooted subject we allow Mr. Teale to speak for himself.

"My experience certainly justifies me in recommending this mode of operating whenever it is practicable, provided the necessary precautions are taken against incurring the evils to which the operation, when carelessly performed, might be exposed; and, in order to avoid these dangers, it is important to be able in the first place to recognize the symptoms which indicate the occurrence of gangrene, or of a state verging towards it; and, secondly, to guard against employing an improper degree of force for the purpose of replacing the hernia after the stricture has been divided."

We need scarcely add, that in this operation the object is, after division of the stricture, to empty the sac of its contents *without* returning the former; where this is effected, the surgeon may be pretty well satisfied that he has accomplished what he desired. Again, as the signs of gangrene, either local or general, are usually of a satisfactory character, and, as we have already remarked, the alternative of opening the sac always remains where any doubt exists, we are well disposed to concur in the opinion of our author, that the operation in question should be first



attempted in most large herniæ, and in many of middle size ; though it is more rarely admissible in small herniæ. At the same time, the cautious advice of Mr. Key cannot be too strongly enforced, viz. that, after division of a stricture external to the sac, no more pressure should be used than would suffice for reducing a hernia where no strangulation exists. We learn from Mr. Teale that it is likewise Mr. Liston's practice to try, in all recently strangulated herniæ, to divide the stricture without opening the sac. Sir A. Cooper's modification of this operation in cases of large herniæ, should be borne in mind ; it consisted in making a small opening into the sac, and dividing the stricture on a director from within ; though it is of course preferable, where practicable, to avoid even this interference with a large hernial sac.

The tabular arrangement supplied by Mr. Teale of 32 cases, in which the stricture was divided external to the sac, certainly presents us with an unusual average of success, as 27 recovered ; of these, 18 were femoral herniæ, 11 inguinal, and 3 umbilical.

The directions given by our author for conducting the operation for strangulated hernia are judicious, but do not present any thing which calls for remark. He speaks highly of a winged director, devised by Mr. Turner of Manchester, " which resembles an ordinary director attached to the upper surface of a thin, slightly convex plate of steel, terminating in a rounded extremity like the finger-nail, and allowing the grooved director to project about an inch beyond it." The object of this contrivance is to protect the viscera from injury.

The treatment of the intestine under the various conditions it may present, next comes under consideration ; and Mr. Teale lays down as a rule, that " no degree of discoloration of the intestine, short of its vitality being extinct, forbids the replacement of the part within the abdomen." This is, indeed, the recognized rule of practice, though the determination of the vitality or non-vitality of the affected part is not so easily arrived at in all instances. The directions given by our author for the solution of this problem are the best which can be followed. He approves of Sir A. Cooper's recommendation, to close a single, small perforation of the intestine by a fine silk ligature ; and, after cutting off both its ends close to the knot, to replace the intestine within the abdomen. With respect to the delicate question of what is to be done with adherent intestine, Mr. Teale recommends that, when recent, the adhesions should be destroyed by the finger, or handle of the scalpel, provided the intestine be not in a gangrenous state, and that " old organized adhesions," when of moderate extent, should be cautiously divided by the knife ; if even a portion of the sac be returned with the intestine no mischief is likely to ensue therefrom. When a large portion of omentum presents itself, Mr. Teale advocates the practice of cutting it away, though perfectly healthy, as he considers there is less risk in this operation, than is to be encountered by replacing it after exposure and strangulation. In accomplishing this removal, he further advises the employment of a temporary ligature, to prevent retraction of the remnant, and facilitate the subsequent securing of the vessels. Purgatives should be avoided for some hours afterwards.

An excellent chapter on " reduction in mass" succeeds. The conditions pointed out as the " chief, if not the essential, ones which favour the

accident," are "a contracted and indurated state of the neck of the sac, and a wide hernial aperture." Of course, in these cases the neck of the sac itself must be the seat of stricture, and their history does not seem to indicate that great violence in the employment of the taxis is necessary to this result; indeed, it would appear that it may even occur spontaneously, as in an instance which came under Dupuytren's notice. The sudden return of the tumour, as Mr. Teale remarks, in place of its gradual diminution under pressure, attended by the characteristic gurgling sound, together with the persistence of symptoms of strangulation, constitute grounds for suspecting the occurrence of reduction in mass; and this receives further confirmation by the external ring being found large, and unoccupied. The presence of a tumour in the abdomen, in the vicinity of the inguinal canal, is a further corroborative but not a necessary symptom; it was existing in one of Mr. Luke's cases, when the patient himself had returned the hernia. The treatment adopted and recommended by the last-named surgeon is approved of by our author, viz. by exploratory examination where the accident is suspected, and the completion of the operation in the ordinary way, by drawing down the sac, and enlarging the ring if necessary. A useful tabular view of this class of cases follows; for the details of Mr. Luke's cases, we refer our readers to his excellent paper on the subject in vol. xxvi of the 'Medico-Chirurgical Transactions.'

The concluding chapter of the First Part of the work is on "Intestinal Fistula, consequent upon gangrenous or wounded Hernia." The opinions of Scarpa on this subject are quoted, and an interesting case is referred to which is narrated by that eminent surgeon, illustrative of the mode of communication after separation of a coil of intestine; the medium of communication, in this instance, was a membranous funnel formed by the hernial sac. A just compliment also is paid to Mr. Travers's original and excellent treatise, which, though an acknowledged and high authority on the subject of 'Injuries of the Intestines,' has not received that general attention and study which its merit deserves. The anatomical characters, effects, complications, and treatment of intestinal fistula follow; in the details of which our space will not allow us to follow our author. He points out that the principal objects to be attained by surgical interference are, the removal of the obstruction caused by the projecting ridge or valve developed at the point where the two orifices of intestine are *parallel* to each other, (such being their usual if not invariable relation, after the sloughing of an intermediate portion,) and to close the external wound, when the proper time shall have arrived. To carry into effect these and other essential parts of the treatment of the above class of cases, clear and judicious directions are given, including an ample view of Dupuytren's operations with the seton and enterotome.

We now come to Part II. of the work, which, as we have already stated, treats of abdominal hernia, in reference to its species and varieties. The opening chapter is on "Inguinal Hernia," the anatomy of which is accurately given and nicely illustrated. Mr. Teale appears to coincide with Mr. Curling in the view that gentleman takes of the structure of the gubernaculum testis; stating that to him "attaches the merit of having proved by the microscope that a great portion of the gubernaculum consists of muscular fibres; that its attachments are identical with those of

the cremaster; and that through the agency of this structure, which must be regarded as the foetal cremaster, the descent of the human testicle is accomplished." In the statistics of inguinal hernia, and the causes to which it is referrible, the tables and calculations of Cloquet and Malgaigne are laid under contribution; but as these have been discussed in our previous article, already referred to, we shall not stop to devote more time to them in our present. When speaking, in the succeeding chapter, of oblique inguinal hernia, Mr. Teale gives a preference to the French term, "interstitial," as applicable to the *incomplete* herniæ, or those which have not projected beyond the external ring. In this form of rupture, in its early stage, the importance is indicated of investigating with the utmost care the site of the internal ring, where no external swelling exists, but when the "symptoms of intestinal obstruction are present without the cause of the obstruction being apparent." We quite believe that in these obscure cases, as our author remarks, a fatal termination not infrequently ensues, without the real cause of the symptoms being suspected.

In discussing the comparative frequency of oblique inguinal hernia in women, M. Malgaigne's novel views and inferences are noticed, but without comment. The characteristics by which this form of rupture is distinguished from direct and femoral hernia, as well as from hydrocele of the spermatic cord, are pointed out: we perceive that our author agrees with all experienced and practical surgeons in adopting, in all cases of inguinal hernia, the safe recommendation of Sir A. Cooper, to divide the stricture directly upwards. The complication of inguinal rupture with hydrocele of the cord in many recorded cases renders it desirable, where symptoms of strangulation exist, to cut down carefully upon the tumour: a case is quoted by Mr. Teale, in which Mr. Liston discovered a small hernial sac by the side of the hydrocele; a similar case is recorded by Scarpa; and others have occurred in the practice of many other surgeons. Mr. Lawrence's diagnostic test is, "fluctuation of the watery tumour at its lower part; its imperfect removal under pressure, so that the cord can never be felt in a natural state, and sometimes a visible enlargement of the inguinal canal and its neighbourhood, when the fluid is pushed upwards. If there be a question between the existence of irreducible omental hernia and hydrocele of the cord contained in many cysts (the diffuse hydrocele of Pott), our author recommends the employment of acupuncture, by which "the presence of fluid may be surely and safely tested."

Another source of error may exist in the presence of air in the intestine, which, from its translucency, may deceive the incautious practitioner into the belief that he has to deal with a hydrocele. An interesting case, which occurred in the practice of Arnaud, is cited by Mr. Teale, but we prefer illustrating this delicate point of diagnosis by quoting one of his own.

"A boy was brought to me at the Leeds Infirmary with a tumour of the scrotum of large size, tense, and elastic; the integuments being very thin from the distension which it occasioned. On examination by the aid of a candle, in the presence of several pupils, it was found to be as translucent as any hydrocele which I had seen. Whilst examining it, I observed that there were two opaque lines intersecting the tumour diagonally; which, by pressure, were made to a certain extent to alter their situation. This circumstance, together with the comparative lightness of the tumour, and slightly tympanitic condition which I detected,

excited my suspicion as to the disease being a hydrocele, and justified me in treating it as a hernia. The use of the taxis showed that the diagnosis was correct, and that the tumour was a hernia greatly distended with flatus; for by pressure the distended bowel was with some difficulty replaced within the abdomen. The opaque oblique lines indicated most probably the point of contact of distinct folds of intestine." (p. 253.)

Our space will only allow us to enumerate the heads of the other diseases which more or less simulate hernia, but which are to be distinguished by the rules which our author lays down: they are, "hydrocele communicating with the abdomen, varicocele, chronic abscess descending through the inguinal canal, adipose tumours, inflamed lymphatic gland, a testicle retained within the inguinal canal, and hæmatocele of the tunica vaginalis."

The *treatment* of oblique inguinal hernia forms the subject of the succeeding chapter, in which we have some good suggestions afforded on the subject of trusses. In old ruptures, where the posterior wall of the canal has been encroached upon, in consequence of the long standing and weight of the protrusion, by which the rings are rendered parallel, Mr. Teale approves of a triangular pad, "the inferior angle being most acute, and corresponding to the upper surface of the pubes, whilst the inferior border of the pad accurately corresponds with the fold of the groin." This pad, he adds, "should be well stuffed, having its greatest convexity along its superior border; so that, whilst its inferior angle rests gently upon the pubes, the superior border may make a more decided pressure upon the muscular wall." To the form of the spring our author attaches great importance, both as regards the efficiency of the truss, and its comfort to the wearer. He considers, and we agree with him, that it should adapt itself accurately to the oblique bearing of the back of the pelvis and lower part of the abdomen, as well as to the "vertical outline of the haunches." To attain this object, Mr. Teale employs springs with two reversed curves; the posterior of which has its convexity directed downwards, and the other, at the anterior extremity, is curved in the reverse direction. This arrangement is at once rendered intelligible in the text by the never-failing accompaniment of a woodcut. The *double* common truss is constructed on the same principle, presenting a triple curve. This chapter closes with a brief account of the operation for the interstitial and scrotal forms of oblique inguinal hernia, with and without opening the sac. We have already, in an earlier part of this article, referred to our author's views on the relative merits of these operations.

In the chapter which is appropriated to "hernia of the tunica vaginalis," the varieties of this form of the disease are brought before us, viz. that in which the protrusion occupies the sac of the tunic, in front of the testicle; that in which the hernia has the same serous relations, but to the exclusion of the testicle, which organ may either remain within the abdomen or have descended to the inguinal canal; and the *funicular* variety of the same, in which the vaginal tunic still forms the hernial sac, but is cut off from the testicle by intervening adhesion. This, as well as the other forms, may usually, but not invariably, be traced to an early period of life; though the ordinary term "congenital," is inapplicable, inasmuch as the disease does not occur till after birth. It is the last-mentioned variety which was denominated by Sir A. Cooper "encysted

hernia of the tunica vaginalis ; its relation to the testicle is not invariable ; and, where lying in front of that organ, might easily be mistaken for oblique hernia in its more ordinary form. In the treatment of these cases during infancy Mr. Teale prefers the employment of a pad of ivory invested with an appropriate and easily shifted envelope, which should be frequently changed for the sake of cleanliness. But when a hernial protrusion exists in conjunction with retention of the testicle within the inguinal canal, "a truss with a hollow pad must be employed." The existence of stricture at the mouth of the sac itself, when this form of hernia is strangulated, usually precludes, as our author admits, the practicability of operating without exposure of its interior.

The nature and treatment of "direct inguinal hernia" occupies the next chapter. Though presenting nothing original, to justify a lengthened quotation, we consider the directions given by our author for conducting the operation in this form of rupture when strangulated so judicious, and as constituting so fair an illustration of his practice and style, that we transcribe the passage which contains them.

"As the surgeon is unable to pronounce with certainty that the hernia is direct, it is his duty, whenever he operates for a strangulated inguinal hernia supposed to be direct, to proceed under the constant apprehension that the hernia may possibly be oblique. If the local and general symptoms are such as to warrant his attempting to relieve the stricture without opening the sac, a small incision should be made over the upper part of the tumour, from above downwards, so as to expose the external ring and the upper part of the fascia of the cord ; an opening of small extent being next made into the latter, a flat director (Key's) should be passed upwards under the external ring, and, if it exert any material pressure on the tumour, a few of its fibres should be divided by the bistoury. If, however, it be found that the stricture is not formed by the external ring, and if the cremaster be found spread over the tumour, it should be turned aside by the point of the director, or, if necessary, divided by the knife, in order to ascertain if the next envelope present the smooth, firm, resisting character of an aponeurotic membrane ; and, should this be the case, the membranous covering should be cautiously opened, and a flat director insinuated beneath it towards the abdomen, when any of its fibres which may appear to exert a constricting influence may be divided by the blade of the knife directed upwards. If, on the contrary, after opening the fascia of the cord, and turning aside the fibres of the cremaster, a loose filamentous tissue, more or less loaded with fat, present itself, the operator may presume that this is the subserous tissue pushed before the sac through a rent in the aponeurotic structures. It then becomes necessary to search for the upper edge of this aperture, and, after insinuating the director beneath it, to divide it upwards, and thereby remove the stricture. If, however, it is now found that these parts, external to the sac, have not been the seat of stricture, the operation must be completed by enlarging, if necessary, the external incision, and dividing the subserous tissue and the sac.

"Lastly, the director must be introduced within the stricture from the interior of the sac, and the constricting band divided ; the operator always bearing in mind that he must direct his incision in this, as in all other forms of inguinal hernia, *upwards* from the middle of the mouth of the sac ; and no presumptuous deviation from this rule, from a confident feeling in his own powers of diagnosis, can be justified under any circumstances." (pp. 295-6.)

As in the preceding chapters, that which follows on "femoral hernia" is introduced with some notice of the anatomy of the region and parts concerned. In his description of the femoral sheath, and the walls of the



femoral ring, we cannot help recognizing what we consider factitious anatomy rather than the simple reality. In the first place we should question, were we so disposed, the continuity of the femoral sheath and the fascia transversalis; but this may pass, for if we entered on this subject at all, it would be requisite clearly to define what is meant by the sheath of a vessel, which we hold to be any thing but identical with a tendinous or fascial bed in which it may chance to lie. But, as regards the crural ring itself, the anatomy, as far as we have ever been able to make it out, is very simple; this space being bounded by the iliac fascia, Poupart's ligament, the vein, and Gimbernat's ligament; and closed by the cribriform fascia, extending from the last-named boundary to the femoral vein. As to the existence of any septum, properly so called, between the ring and the vein in the normal condition, we know of none, nor can we conceive what purpose it could answer. It is true that the cribriform fascia (of Sir A. Cooper) may be and is so prolonged by continued and gradual pressure, as to form an investment of femoral hernia; and it is thus that the sac is separated from the vein. The condensed structure which occupies the saphenic opening is altogether independent of the femoral sheath, by which we mean the close cellular investment of the vessels, as well as distinct from the fascia transversalis, which can hardly be said, under any circumstances, to extend beyond Poupart's ligament. However, we are aware that we are on debatable ground; and our only desire is to enter our protest against the tendency which certainly exists with some writers of complicating practical anatomy by describing things as they think they ought to be, rather than as they really are. But this is not a charge we can bring against our author; although we think his anatomy of the femoral ring and sheath might a little puzzle a beginner. Again, we may remark, that though it is doubtless true, that the adhesion of the reflected border of the saphenic opening to the cellular sheath of the vessels is a barrier to the descent of a hernia *within* the fascial envelope, still we do not think that sufficient importance is attached to the ingress and egress of the blood-vessels and lymphatics at this part, by which the superficial and deep parts are, as it were, closely pinned together, and the protrusion is thus forced to take an upward direction, pushing before it the cribriform and saphenic fasciæ. A remark of our author's which follows we quite concur in, viz. that the tumour in crural hernia "usually projects over the semilunar edge of the femoral aponeurosis below, and over Poupart's ligament above, encroaching in the latter direction on the site of inguinal hernia. But," he adds, "this encroachment is not produced by the entire tumour turning upwards, as has been frequently represented, but is simply the result of the general enlargement of the body of the sac in comparison with the aperture through which it has escaped." Mr. Teale informs us that he has operated on a femoral rupture in a male, which was as large as two fists; this size, however, is very rare; as are other instances in which the relation of the hernial sac to the femoral vessels is described as different from that which we have indicated.

The complications of femoral hernia noticed by our author, and which it is necessary for the surgeon to be acquainted with and prepared for, are the coexistence of inguinal rupture, the presence of serous cysts, and enlarged lymphatic glands. He mentions an instance which recently came



under his notice, of the rare complication of two inguinal and two femoral herniæ in the same subject.

The seat of stricture in femoral hernia has been the subject of considerable dispute and difference of opinion ; and we do not doubt that the disputants have been all more or less in the right, though we are free to confess that our own observation leads us to place it in by far the largest proportion of cases in the sharp border of Poupart's ligament which overhangs the ring. Mr. Teale ascribes it to the femoral sheath in most instances, and quotes Sir A. Cooper and Mr. Key as confirming this view. It is certainly a very delicate point to decide, inasmuch as the adhesion of the so-called sheath to the posterior aspect of Poupart's ligament, (the "intermixture of aponeurotic fibres," to use our author's own expression,) entirely destroys the individuality of these structures at this point ; and though it may be true that these aponeurotic fibres below Poupart's ligament *aid* in strangulating a protruded portion of gut, yet we very much question whether their division alone would suffice to relieve the stricture. In short, we do not recollect to have witnessed a case in which the stricture was relieved (when its seat was the fibrous ring in front, and to the inner side of the neck of the sac,) without a more or less decided division of the posterior border of Poupart's ligament.

When reviewing M. Malgaigne's work we noticed his test for diagnosing crural as distinguished from inguinal hernia, we need not therefore now repeat it ; the directions are given at length in the volume now before us. Nor is it necessary to notice the distinguishing characteristics of varicose veins and psoas abscess, which can scarcely be mistaken for hernia where ordinary caution is employed in conducting the examination of the patient. The "statistics and causes" of femoral hernia we may likewise pass by ; but on the treatment of this form of rupture we have a few words to say before we leave it.

The employment of trusses in crural hernia is a palliative measure, which conduces very importantly to the patient's comfort and safety ; but they effect little else, on account of the nature of the bounding textures of the ring, which tends to render its abnormal size permanent. As Mr. Teale remarks, it is important that the pad for these trusses should not be of large size, as it is then so readily displaced by the action of the abductor and flexor muscles of the thigh ; and it is also important that it should not rest on the pubes. The following are his directions for the construction of a common single truss for reducible crural hernia.

"It should have the pad of a triangular form, narrow transversely, and somewhat elongated from above downwards ; its base corresponding with the edge of Poupart's ligament. The pad should be of size sufficient to close the saphenous opening, but should only extend to a very limited distance beyond the borders of this aperture. Its convexity should constitute a rather prominent ridge, directed from above downwards, situated a little towards the pubic side of the pad, commencing about a finger's breadth below its upper edge, and extending downwards to the apex, towards which part it should gradually diminish. A pad thus constructed gently closes the external aperture, namely, the saphenous opening ; and its most intense pressure is directed to the pubic side of the vein." (p. 327.)

Surgeons should not think themselves, as many do, above this purely mechanical part of their art ; for how could a mere mechanist construct

an apparatus, which it requires an accurate knowledge of anatomy to render available? Where a surgeon can add to his professional acquirements a practical familiarity with mechanics, he possesses a considerable advantage over another who is without this knowledge, not only in the use of his hands, but in the construction of his appliances to boot.

In the treatment of irreducible femoral hernia, our author strongly recommends a hollow pad, consisting of a "flat ring of metal supporting a concave metallic plate inclosed in a bag or cap of wash-leather. The metallic ring should be adapted to the size and form of the particular hernia, so that it may rest upon the parts immediately surrounding the tumour, without exerting any direct pressure upon it; whilst the leathern cap, spread over the opening of the ring, should possess such a degree of concavity as to enable it to support and to exert a gentle pressure upon the hernia." This form of truss Mr. Teale has not found obnoxious to the objections which Sir A. Cooper had to their employment in this form of irreducible hernia.

The operative course to be pursued when strangulation exists is judicious; and we are willing to acquiesce in our author's recommendation, that the stricture should be divided, where it can be done satisfactorily and safely, without opening the sac; and we quite believe that the surest rule for the division of the stricture is to direct the edge of the bistoury upwards and forwards.

In the chapter on "umbilical hernia," there is not much to attract our attention. A truss invented by Mr. England, of Leeds, and which was highly commended by Mr. Hey, is that which our author prefers in ordinary cases of umbilical rupture. It is figured in the work before us, and consists of a convex cork pad, and a horseshoe spring attached by a hinge to either side. When there is a very lax and pendulous state of abdomen, Mr. Teale advises that the pad should be attached to "a broad plate of steel of very moderate concavity;" or if necessary, an ivory compress retained by adhesive plaster may be employed. The frequent fatality of this form of hernia, when strangulated and subjected to operation, certainly justifies the remark that the sac should not be opened, if the stricture can possibly be relieved without this step.

The remaining chapters of the work treat successfully of "ventral, obturator, ischiatic, perineal, vaginal, pudendal, and diaphragmatic herniæ." Mr. Teale is enabled to add but little from his own personal experience to our information regarding these less common forms of hernia. He narrates one fatal case, which occurred in his own practice, of ventral hernia. In protrusions at the anterior wall of the vagina, a truss, "consisting of a compress supported by a spiral spring, so as to press upon the external labia," is spoken of in the highest terms, as applicable where an internal pessary cannot be borne; this instrument is also manufactured by Mr. England, of Leeds.

We must here bring our analysis to a close, hoping that our readers have seen sufficient to satisfy them that our commendation has not been misplaced, but fearing likewise the fulfilment of our own hinted prediction, that the task we are concluding is one which, from its monotony, it is not most easy to render agreeable to the reader. Yet, although we cannot award to Mr. Teale the merit of originality, to which indeed he does not

profess to lay any claim, we consider that he is entitled to our warmest thanks for having given to the profession so valuable a text-book in this important and intricate branch of surgery.

The subject is handled with that practical familiarity which cannot fail to engender confidence, and at once to instruct and interest the reader. The style is simple, terse, and pure, without bordering on dogmatism on the one hand, or falling away into vagueness or indecision on the other. Altogether, we may repeat, that we have rarely had it in our power to recommend a work with such unmixed confidence and satisfaction to our readers: and this we do most cordially to all classes, but especially to students and junior practitioners, who will find it an invaluable and unfailing guide in their early operations and maiden difficulties.

#### ART. XVIII.

*The Young Stethoscopist; or, the Student's Aid to Auscultation.* By H. J. BOWDITCH, M.D.—*Boston, (U. S.) 1846. 12mo., pp. 248.*

THIS is, in one respect, a very original book on auscultation. It includes a description of that method of diagnosis in its applications to the lower animals; it is illustrated by a variety of woodcuts representing square pieces of India-rubber, (p. 39,) numerous stethoscopes, and a horse (p. 224); while its sections are headed with pictorial letters, after the manner of our facetious friend Punch. All this may not be without its utility, as the book is expressly designed for junior auscultators. Our notice of its contents will be limited to comments on a few points which have struck us in turning over the pages.

Dr. Bowditch appropriates the phraseology and observations of writers on this side the Atlantic, with very imposing coolness; at this we do not feel any surprise: but how does Dr. Bowditch venture to lay hands on the property of his neighbours of Philadelphia (Dr. Gerhard is *located* there, we believe,) without some fear of exposure? He gives the exact explanation long since tendered by the Philadelphian physician, of the natural state of prolonged expiration at the apex of the right lung, without the slightest reference to its author's name. We ourselves doubt extremely whether Dr. Gerhard's be the true explanation of the fact; but, true or false, the idea of the phenomenon being dependent on the greater width of the right than the left bronchus, should have been conceded to Dr. Gerhard.\*

Dr. Bowditch has twice heard the "pulmonary crumpling sound" (of Fournet) in "apparently early stages of tubercular disease. It was connected with no other physical sign, and occurred at the end of inspiration. Both patients had had hemoptysis; both apparently regained their health."

In certain cases of bronchitis, both acute and chronic, says Dr. Bowditch, "you may get no physical signs." This is true most perfectly, if understood to apply to the ruder class of signs, as rhonchi and great change in the sound of percussion; it is untrue, if referred to trifling alteration in the respiratory murmur.

"Twice" Dr. Bowditch "has observed a very curious phenomenon in the second stage of pneumonia of the posterior portions of a lung; viz.

\* Or, if not to him, to Fournet, who taught the same doctrine, and illustrated it by a figure.

an almost tympanitic state of the breast." (p. 54.) Would not any reader infer from this passage that the observation it records is a novel one? yet the learned doctor is doubtless—or at least ought to be—as fully alive as ourselves to the fact, that the occasional occurrence of tympanitic resonance in pneumonia has for years been matter of established knowledge in these countries.

"In certain cases," observes the author, "there is an *augmentation* of the natural sounds on percussion, even when there are many tubercles." This we consider a correct observation, as also the explanation offered that surrounding emphysema is the source of this unusual condition. But, practically speaking, the fact is of little importance from its rarity: it is rare, as we conceive, because when emphysema to any extent is developed round tubercles, these have already increased sufficiently in quantity to deaden the sound, in spite of the coexistent dilatation of the vesicles.

Dr. Bowditch's "method of examining a case" of suspected phthisis, though not exhibiting a particle of novelty, and omitting all investigation of certain delicate points of which the refined auscultator knows the occasional importance, is the best thing in the division of his book referring to the lungs. What he has given, he has given clearly, and if his directions be followed, and the intellectual qualifications of the observer be not particularly obtuse, he will rarely fail in a correct diagnosis. Even this modified praise, however, cannot be extended to the observations on acute phthisis, and phthisis in children: seven lines and a half of peculiarly meagre matter contain the sum of what is said of both.

Talking of the veins, Dr. Bowditch says, that under certain circumstances a "sound called *bruit de diable*, or *devil's noise* [!] is heard in them. It was named by Bouillaud, and resembles the sound produced by a toy. It is similar to the bellows-murmur of the arteries, except that it is continuous." This short statement contains two errors; but these are of little consequence. Our motive for referring to the passage is to comment, in a gentle phrase or two, concerning the progress of knowledge of M. Bouillaud and his countrymen on the subject of venous murmurs. M. Bouillaud baptized the venous murmur of continuous type with irregular increase of force, as above stated—*humming-top noise*; and upon this wonderful achievement has had his praises echoed far and wide for years. But baptizing the thing was not enough—a full description of its characters, origin, relations, significations, source, of what it did do, and what it didn't do—must follow, for M. Bouillaud had a gaping auditory, who had decreed him the office of "high priest of cardiac disease," and he must deliver oracles, whether the spirit moved him or not. And the oracle spake, and spake with a vengeance! It is needless to remind our readers of the error of Bouillaud in maintaining venous murmurs to have their seat in the arteries, or of the thrice absurd inferences into which he fell, through the influence of that primary error. It is enough to state, that so late as 1841, Messrs. Barth and Roger, in the first edition of their 'Manual on Auscultation,' gravely circulated the mass of incorrect statement given forth by the oracle. And in the second edition of their work (1844), something worse than their previous ignorance prevails, in their notice of venous murmurs. "Ward and Hope," say they (p. 498), "admitted that the continuous murmur is produced in the veins." "But

*their theory was not accepted,*" add these writers; a fact new to us, we confess, for never was there a fact in clinical observation, within our memory, so instantly received as a truth as this very one. But M. Bouillaud and his clique of course did not "accept" it, the writers of the 'Manual' mean; and why should they? Dr. Johnson once said of our friends on the other side of the Tweed—"that Scotchman must be a sterling moralist, who would prefer truth to Edinburgh:" our *chers confrères* across the straits of Dover go further—they do not recognize any truth except their own truth. Even *now*, these French writers cannot bear to disguise themselves by being for once just to foreigners. Is it not monstrous, to find them coolly telling us that it is only since one M. Aran (an inhabitant of Paris) examined the subject, that the venous theory is admitted!

A prominence exists sometimes in the precordial region, in cases of pericardial effusion and hypertrophy. But the measure of this prominence? Happily the age we live in has given birth to a certain Dr. Felix Andry, in whom the *mens divinator* exhibits itself in the invention of a "cystometer,"—only think, a *cystometer*,—to take an exact measurement of the same; henceforth, no man attending a case of pericarditis need pine in hopeless anxiety as to the number of hairs' breadth, more or less, the precordial surface may have risen in his suffering patient!

But to return to Dr. Bowditch. We do not admire books superficial and imperfect, whether these characters be aimed at by their authors or not: we therefore do not admire this book. Secondly, we do not admire bad grammar or bad English; and as we find numerous specimens of both,—e. g. in pages 6, 9, 55, 157,—we do not admire this book. And, lastly, we do not admire books which tell the world nothing more than many of similar title: now as this book does not tell a sixth as much as, and that sixth not better than, its predecessors, we neither admire it, nor see any necessity for its production.

#### ART. XIX.

*Etudes de Géologie Médicale sur la Phthisie Pulmonaire et la Fièvre Typhoïde, dans leurs Rapports avec les Localités Marécageuses, &c.* Par S. CH. BOUDIN, &c.—Paris, 1845. 8vo. pp. 79.

*Medico-Geological Studies on Pulmonary Phthisis and Typhoid Fever, in their Relations to Marshy Districts.* By S. CH. BOUDIN, &c.—Paris, 1845. 8vo., pp. 79.

THE author of this pamphlet has had opportunities, in the military service of his country, to obtain personal information on the subject which he has discussed. He has served in Greece; he has been surgeon to various military hospitals in Algeria, and he is now surgeon-general to the military hospital at Versailles. It is not surprising that, with these opportunities, this essay should have obtained the prize for the best essay on public hygiene, given by the editors of the 'Annales d'Hygiène Publique et Médecine Légale,' in the year 1844.

It was in 1828, while serving with the French expedition to the Morea, that M. Boudin first noticed the remarkable antagonism between typhoid and pulmonary affections on the one hand, and marsh fevers on the other.



In 1832 he was appointed medical director of the quarantine hospital at Marseilles, and he had again occasion to notice that the troops debarked from the marshy localities of Greece and Algeria were rarely affected with either typhus or phthisis—the two diseases to which a great proportion of the mortality amongst French troops is attributed, and which “frightfully decimated” the garrison at Marseilles. In 1841 M. Boudin published his ‘*Traité des Fièvres Intermittentes*,’ in which he discussed this antagonism; and again in 1843, at greater length, in his ‘*Essai de Géographie Médicale*.’ A few days after the latter appeared, M. Rayer proposed to the Royal Academy of Medicine of Paris that a physician lately gone to Algeria should be instructed to inquire into the relations of phthisis to the marshy localities of that country. The proposition was unanimously adopted, and the subject became at once popular—nay, according to our author, just as every political question agitated in the French parliament finds an echo throughout the whole world, so the question how far marshy localities exercise a remedial influence was re-echoed beyond the Alps, at Vienna, at Berlin, and on the borders of the Thames, so soon as the French took it up.

In the introduction to his essay, M. Boudin, in making these statements, adds the names of several French authors on the subject, and mentions several facts which had come under his own observation during his services in the army. In the second chapter he discusses the question of antagonism in general, which he defines as the principle in virtue of which a diathesis or morbid state affords a protection more or less complete against the manifestation of certain morbid conditions. Examples of such antagonism are presented in the action of specifics, as quinine and arsenic, in the treatment of ague; or of prophylactics, as belladonna, in the prevention of scarlatina [which prophylaxis we entirely doubt]. Concurrently with this protection from certain morbid states, there may be a predisposition to others. Geological and climacteric influences are important agents in this respect, by determining the nature and kind of food, dwelling, clothing, employments, and other agents affecting the hygienic condition. M. Boudin observes that a glance at a geological map of England will suffice to show that the largest and most populous towns of England are situate on the hard red sandstone; the agricultural and pastoral towns on the chalks or oolites.

The influence of a previous residence is felt after removal from a district having certain hygienic relations. The English sweating sickness is quoted as an example of this general proposition. Mr. Boudin relates several more modern examples of the same kind. Towards the end of April, 1843, two infantry regiments arrived at Courbevoie (Versailles), the one (the 23d) from the north of France, the other (the 69th) from the citadel of Strasburg, the focus of a marshy district, where it had been quartered for two years. These two regiments occupied the same barracks, and were in every respect situate in the same circumstances. Yet the disease that affected each regiment differed widely in character. The 23d sent to the hospital cases of pulmonary inflammations and typhoid fevers; the 69th filled it for more than a year with intermittents, the disease attacking even those who remained free from it all the time they were at Strasburg. Another regiment, which subsequently arrived at Courbevoie from Strasburg, presented cases of disease identical with the preceding.



This predisposition is not confined to men. For several years the mean annual loss of horses by disease in the French army amounted to 197 per 1000. In the Prussian service it is only 20 per 1000; and in the French Gendarmerie is only 14 per 1000. This led M. Boudin to inquiries, and he soon found a case in point. Two cavalry regiments, towards the end of 1841, arrived from different quarters at Versailles, where they were placed under circumstances in all respects alike. Their sanitary condition was, however, very different. From the 1st October, 1841, to 1st December, 1843, the deaths of the horses in the two regiments were as follows:

	Number of horses sick.	Number of horses killed or died.	Numbers killed with glanders.	Numbers killed with farcy.
Cuirassiers .	513	180	93	28
Hussars .	214	48	12	3
Difference .	299	132	81	25

That this enormous difference in the sanitary condition of the horses in the two regiments was not owing to the difference of service, is proved by the fact that in the whole army the cuirassiers lost 18 per cent. of their horses, and the hussars 16½. M. Boudin thinks it can only be attributed to the circumstances in which the regiments were respectively placed *before* their arrival at Versailles. The hussars had been in the more comfortable garrisons of the east of France; the cuirassiers had been quartered in those of the north, which are considered inferior to the former, especially in the quality of the forage.

In the third chapter M. Boudin inquires more particularly into our Dr. Wells's doctrine of antagonism between intermittents and phthisis, and the facts which elucidate it. A river during a long course will present examples of this kind. Where it flows rapidly and within its banks, we should expect pulmonary phthisis and typhoid fevers to prevail; but at those points in which it spreads out, in a flat country, intermittents will supersede the latter. The Rhine, in its long course, presents an instance of this kind. Numerous other illustrations are brought forward by M. Boudin, particularly from our army statistical reports, to which we have previously called the attention of our readers. From these and other facts it is very clearly shown, that at our various stations in India, North America, and Africa, where intermittents are prevalent, pulmonary diseases are rare, and *vice versa*. The statistics of the United States' army indicate the same antagonism. In Algeria, intermittents and other forms of marsh fever are extremely common. From April to October, 1308 cases of fever were admitted into the military hospital at Algiers; of these, 288 were intermittents, (quotidian?) 239 tertians, 92 remittents, and 106 pseudo-continued. The hospitals at Medeah, Blidah, and Bona present the same proportions nearly. When the resumption of hostilities by Ab-del-Kader rendered it necessary to send reinforcements to Africa, typhoid cases became more frequent, and amounted to 1 in 28 of the total admissions; but it was remarkable that no soldier who had been more than eight months in Africa was attacked with typhus.

M. Boudin passes from Africa to the marshy grounds of Europe, and especially to those of his own country; and brings together a multitude of facts, all tending to the establishment of the same principle, namely, that there is a real antagonism between the two classes of diseases, and

that a residence in a marshy district is, within certain limits, prophylactic or curative of phthisis pulmonalis, or protects against typhoid fevers. Remarkable examples are given of the latter part of the proposition. One or two of these we shall note; they are from the '*Géographie Médicale*' of our author. In August, 1841, the garrison at Marseilles suffered so severely from typhus, that five of every seven admitted into hospital were affected by it. A regiment arrived from Africa at this time, and was quartered for twelve days at Marseilles. During that period it sent 49 fever cases to the hospital, but they were all intermittents of various types. In the beginning of the year 1842, another regiment came to Marseilles from Africa, and was in garrison there for about five months. Not one case of typhus occurred in it, although other battalions suffered as usual. Individual examples of the cure of phthisis, after residence in a marshy district, are given by M. Boudin, as communicated to him by those who observed them.

M. Boudin then proceeds to demonstrate the antagonism of races, pointing out, in particular, the greater liability of the white troops in the West Indies to yellow fever, of the black troops to pulmonary diseases, and *vice versa*. He then notices the antagonism of the sexes, and of different periods in history and of the year. Under these heads free use is made of British national statistics, both civil and military. The "conclusions" we give in M. Boudin's own words :

"1. Those localities in which the producing cause of endemic intermittents thoroughly modify the constitution of man are remarkable for the infrequency of pulmonary phthisis and typhoid fever.

"2. The localities in which pulmonary phthisis and typhoid fever are particularly prevalent are remarkable for the infrequency and mildness of intermittent fevers contracted on the spot.

"3. The drying up of a marsh, or its conversion into a lake, diminishes or prevents intermittent fevers, but seems to dispose the organism to a new series of diseases, in which pulmonary phthisis and (according to the climate) typhoid fever are particularly prominent.

"4. After a residence in a thoroughly marshy locality, an individual enjoys an immunity from typhoid fever, the degree and duration of which is in direct proportion, 1st, to the length of the previous residence; 2d, to the intensity of the fevers proper to the locality, considered under the twofold relations of form and type; 3d, or, in other words, that a residence in a country of remittent and continued fevers, such as certain points of the coast of Algeria, and the centre of the marshy part of Brasse, is more prophylactic against the disease referred to than, for example, a residence near the marshy embouchure of the Bièvre at Paris.

"5. The conditions of latitude and longitude, and of height [above the sea], which limit the manifestation of marsh fevers, equally limit the curative or prophylactic influence of the marsh miasm.

"6. Lastly, certain conditions of race, and possibly of sex, diminish the susceptibility of the system to the cause of marsh fevers, and in an equal degree diminish the therapeutic influence of that cause." (p. 76.)

The subject of which M. Boudin treats has considerable practical value in the distribution of troops, and in the hygiene of those predisposed to consumption. Although we hope this "echo from the banks of the Thames" may gratify our author, we must observe that it is an old subject of discussion in England, and the practical value of our empirical knowledge has in some degree been tested. We would recommend more of our

English authors to M. Boudin for perusal—especially Sir James Clark's work on Climate. He will there find that his theoretical views are not borne out altogether by facts. We do not say this testily, but with a kindly feeling, and a wish that M. Boudin will continue to work at his problem, for we are sure there is something in it, although not that which he anticipates. We apply this remark as well to the supposed antagonism of marsh and typhoid fevers, as of marsh fevers and phthisis.

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#### ART. XX.

*The Vital Statistics of Glasgow for 1843 and 1844.* Drawn up, &c. &c., by ALEXANDER WATT, LL.D., City Statist.—*Glasgow*, 1846. pp. 148.

WE noticed on a former occasion the valuable series of contributions to Vital Statistics, which emanated from the diligent pen of Dr. Watt, and of which the present is a constituent portion. It is not possible to notice here the sixty tables which Dr. Watt presents to the public in his closely-packed pamphlet; we must, therefore, refer the statist to the publication itself for details. Amongst the most interesting of these tables is a series exhibiting the fact, that there are specific laws which regulate the amount of deaths at different ages by the several diseases. These tables, Dr. Watt thinks, seem clearly to prove that, *ceteris paribus*, the mortality at different ages is uniformly in certain proportions to the amount of deaths by each disease respectively. To ascertain the precise effects of these laws, and the causes that may be brought into operation to produce a variation in their results, is, as Dr. Watt observes, of much importance, both for forwarding the science of vital statistics, and in the acquirement of a proper knowledge of the best modes of medical treatment, as well as for the advantages they must afford in coming to a correct estimate of the relative social conditions of the people of various localities. Illustrations in a tabular form of the operation of these laws are presented by Dr. Watt, derived from the Annual Reports of the Registrar-General. One of these tables shows, that although there was nearly double the amount of deaths by measles during the year 1840, in twenty-four town districts selected for comparison with the metropolis, than there was in the latter during the year 1842, the proportions at the same age are remarkably approximative. The difference under one year is 3·39 per cent.; this is the greatest, and it gradually becomes less at the higher ages. Under three years the difference is only 0·82 per cent.; under five years it is 0·58 per cent.; and under and above twenty years the difference is only 0·10 per cent. Scarlatina presents a similar proportionate mortality.

The causes of excessive mortality are considered by Dr. Watt, and in doing this he takes an opportunity to remark upon some critical observations we thought it expedient to make on some of his deductions in the report preceding this. It is gratifying to us to find that Dr. Watt appreciates our "spirit of fairness"—a spirit, we take leave to say, by no means foreign to the pages of this Journal—and that he acknowledges our competency to deal with the subject. On a careful perusal of Dr. Watt's statements, we do not feel it necessary to withdraw or modify the *juste milieu* opinions we then expressed. We stated it as our opinion

(No. XXXVI, p. 512) that much of the controversy respecting the casual relations of poverty to fever has originated partly in an indeterminate use of words, partly in misconceptions. We do not deny that the want of sufficient nutriment will render a person more liable to be attacked by fever, and will render the attack more severe, and more probably fatal. That is a point in pathology on which we could hardly imagine there were two opinions; and we hold also that the want of proper ventilation of the dwellings of the poor (so lamentably manifest) will increase the liability to, and add to, the mortality from fever. On this point we do not apprehend a dissentient voice. We are, however, most decidedly of opinion that the miasm arising from the extended cesspools of our large towns, termed sewers, and from open drains, when heat favours the decomposition of the animal and vegetable remains which they contain, will have two principal effects. Firstly, it will give increased virulency to any epidemic whatsoever that may be prevalent, whether it be measles, cholera, or typhus fever; and, secondly, it may so modify some of the ordinary epidemics that a new form or species of epidemic may arise. Thus a malignant remittent may be changed into a species of plague, as in some nautical epidemics, whether this modification occurs directly—simply by the reception of the miasm—or whether it be a new ferment generated by the miasm in a person whose blood is already contaminated by breathing the miasmatic atmosphere, may admit of question. The result of our investigations is, that it may and does occur in both ways. From some incidental remarks made by Dr. Watt, we should think he is not “well up” in the pathology of epidemical diseases, and that his sources of information are not such as to afford him large and comprehensive views on the subject.

The citizens of Glasgow must, however, feel much indebted to their city statist; and it is to be hoped that they will co-operate with the citizens of other large cities and towns of Scotland in a demand for an extension of the English system of registration to their own country. At present much of the labour expended by Dr. Watt on his tables is rendered effete by the want of the statistics of births, and of more exact and comprehensive nosology in the registration of the causes of death. When we find such entries as “bowel hives,” “bloody flux,” “shortness of breath,” &c., among the causes of death, we cannot but attach much uncertainty to the pathological statistics derived from so questionable a source.

#### ART. XXI.

*Essai sur les Tumeurs Solides Intra-Thoraciques.* Par J. M. H. GINTRAC.  
Thèses de Paris, Janv. 1845.

*Essay on Solid Intra-Thoracic Tumours.* By J. M. H. GINTRAC, &c. &c.  
4to, pp. 67.

IN this Essay, such tumours as are formed within the walls of the chest, and yet without the proper structure of the organs that cavity contains, are treated of. The author has succeeded in collecting thirty-two cases of the kind. The nature of the material composing the new growth in these cases varied, being encephaloid, scirrhus, tuberculous, steatomatous, &c. This variety of component material he does not regard as a fair ground

for separating the cases, because various kinds of structure sometimes appear in one and the same tumour, because the distinctions between these various kinds of structure are not always clearly defined, and because their local effects and ultimate influence are frequently similar. Hence the only division adopted by the author is one of situation as follows: 1, tumours developed in the pleuræ; 2, outside the pleural sacs, either between the lung and pleura, the ribs and pleura, or in the mediastina; 3, tumours connected with the bones of the thorax.

Passing over the narratives of the cases, transcribed (with one exception) from various published works, we reach the writer's general considerations on these growths.

In respect of causes, we learn that these productions are rare in infancy, youth, and old age; most common in adult age. There were nineteen men for nine women. The disease occurred in persons of all varieties of constitution, profession, and previous health; local injury appeared in a few instances, as did also some general causes, to have determined or accelerated the outbreak of the disease.

The analysis of the symptoms includes dyspnœa (the most constant of all); cough, which was common; expectoration, variable in nature or amount; and pain of the thorax, varying in character and in exact site. Inspection of the thorax sometimes discovered tumour protruding externally. "Percussion was in general dull in the regions where the tumours were seated. Auscultation disclosed absence or diminution of the respiratory murmur on the affected side. Mucous or crepitant râles have been observed, and in a case of sub-sternal tumour, the 'souffle cataire' was heard." This is absolutely the total amount of what is said on the subject of physical signs! We cannot avoid contrasting this ludicrously meagre notice of this most important branch of the author's subject with the minute and elaborate investigations and descriptions of these same physical signs by Dr. Walshe, in the sections on Cancer of the Lungs and Mediastina, in his recent work. While those sections furnish manifest proofs that the number and variety of physical signs is greater in cases of intra-thoracic tumour than of almost any other affection of the respiratory organs, they supply pleasing evidence of the superior advancement of our countrymen in sound acquaintance with this class of diseases.

Palpitation of the heart existed in some cases, attended in one instance with blowing murmur; the heart was pushed to the right or left, according to the seat of the tumour. The pulse varied in character. The face was sometimes flushed or livid; in other cases pale or infiltrated. The lower, and sometimes the upper, limbs were œdematous; the œsophagus, diaphragm, stomach, and liver, underwent pressure in various cases;—hence dysphagia, hiccup, vomiting, swelling of the hypochondria, and jaundice. The appetite held good till an advanced period; the state of the bowels varied.

The course of the disease, though generally equable, was sometimes intermittent. Its duration was scarcely capable of being calculated; death sometimes occurred suddenly.

There is nothing worthy of particular notice in the writer's observations on the diagnosis.

In the hope of effecting the resolution of intra-thoracic tumours, the

author recommends a local discharge to be kept up by caustic issues. He would wish an abundant suppuration produced by the successive production of eschars in various parts of the chest. We doubt exceedingly the value of such applications as curative agents: it is probable they might relieve the dyspnoea and pain, if made to act very energetically; but it would then become a question (and in our minds the question should at once be answered in the affirmative) whether the remedy were not worse than the disease. The iodide of potassium is recommended, *more solito*.—still we believe that something in the way of suspended progress may fairly be hoped for from the action of iodine well managed, and given internally and applied externally. The extracts of conium, hyoscyamus, belladonna, and aconite are thought worthy of trial by the writer. Blood-letting may be required for the relief of urgent dyspnoea and symptoms of congestion of various parts and organs; but the writer correctly insists on the importance of being cautious with the lancet: a symptom may be relieved by its use, but the activity of the main disease certainly does not seem to undergo (and why should it?) the very slightest favorable modification by abstraction of blood, local or general. The author seems an advocate of the *cura famis*, but he is not one of the ultra starvationists. It must be a strong faith indeed in the powers of starvation as a remedial agent, which would induce the physician to deprive of food the wretched sufferer, whose appetite is unimpaired, and who, having little but his own woes and apprehended death to dwell upon, turns to the comfort of satisfying hunger as his sole earthly indulgence,—as to the “*dernier fil auquel tient le bonheur d'exister*.” Probably M. Gintrac has not yet treated many such cases as these; when the sufferers become his patients, he will, we predict, find some difficulty in enforcing the *cura famis*.

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#### ART. XXII.

*Die Nervenkraft im Sinne der Wissenschaft gegenüber dem Blutleben in der Natur. Rudiment einer naturgemässern Physiologie, Pathologie und Therapie des Nervensystems.* Von Dr. CARL JOS. HEIDLER, &c. —Braunschweig, 1845.

*The Nervous Power of Science versus the Blood-life of Nature. A fragment of a natural Physiology, Pathology, &c. of the Nervous System.* By Dr. C. J. HEIDLER, Imperial Austrian Councillor, Senior Official Physician to the Baths at Marienbad, &c.—Brunswick, 1845. 8vo., pp. 392.

THE regular current of sciential progress is broken at intervals by some queer-shaped rock or uncouth apparition which rises or “crops up” above the ripples. The disturbance thus created is sometimes considerable; there is much chafing, and roaring, and muddiness, before the obstruction is fairly removed. Usually, however, the disturbance is trifling; just so much as an uncouth fish might make when lifting its head above the waves. Not long ago one book was published to demonstrate that the theory of the circulation of the blood was an ingenious philosophical fiction—no more: and another, to set forth a mystery about the blood and muscle; in which the professional public was requested to take notice



that blood and muscle were the all in all, and nerve comparatively nothing. Well, now here we have, in Dr. Haidler's book, a still more uncouth fish popping its queer-looking head above the rippling current of physiological and pathological research; resolved, apparently, to give a new direction to the stream, or, failing in that, to make at least a stir by its queerness. The blood, according to Dr. Haidler, is the first, the last, the supreme in the organism. The blood receives impressions from the external world, and is the medium of communication between these and the nerves. The blood is the immediate and active stimulant of all organs and tissues. There is no nervous power; there are no nervous diseases. The former is a fiction, the latter are dependent upon morbid states of the circulation; in short, nervous diseases, so called, are caused by congestion of the capillaries.

The object (to be more specific) of Dr. Haidler's dissertation, is to show that the practical study of the physiological, pathological, and therapeutical relations of congestions, either with or without "orgasmus," and of spontaneous or primary, and of secondary or consensual hemorrhages, is the most useful, intelligible, and certain means of advancing physiology, pathology, and therapy. A change in the quantity or (less certainly) in the quality of the blood and in the stimulation by the blood of tissues and organs, is the immediate or proximate general cause of all animal sensations, from the first or lowest degree of common sensation to those of the highest degree, and which involve pleasure or pain. The numerous modifications which sensations exhibit are dependent on modifications of the blood. These differ in degree, kind, and locality, according to the organ in which they take place. The nervous system is a machinery for communication only; an apparatus for "animal perception; in animals an animal addition,—in man a humano-animal addition to the vegetative and inorganic vital processes in both." Its functions are simply to be a medium for communicating vibrations, oscillations, or undulations developed by concussions. It perceives and directs.

Certain "introductory remarks" are made up of a variety of gossip. We have references to previous works; references to private criticisms made by persons to whom Dr. Haidler submitted the manuscript of his book; statements of what he did intend, of what he now intends, of what he thinks on this, that, and the other. This preliminary flourish accomplished, he sets himself seriously to work. He first pulls down the theory antagonistic to his own. His first effort is to prove a negative,—that the so-called nervous principle or power does not exist. He examines the evidence in its favour under the five different heads of anatomy, physiology, pathology, therapy, and metaphysics. This he does after a crafty method; he collects the notions of various modern writers as to the nature of the soul, the nervous principle, perception, sensation, volition, and elicits a variety of palpable contradictions. Opposing views on these subjects are also cleverly mixed up with ideas of a more rational character, in such a way that we cannot sufficiently admire the author's ingenuity. At the same time, he has a knack of asking puzzling questions, and one really begins to feel a regard for the shrewd and clever author. He appears to advantage as a good puller-down. When, however, we come to examine into his own theory, and into his powers of building up, we find a woful

declension. The proofs of his proposition,—that the nerves are merely an apparatus for the conduction of oscillations, that the blood receives the impressions of these oscillations and transmits them to the nerves, and that these oscillations do occur, consist simply in the vaguest analogies. It would be a useless tax upon the patience of our readers to set forth these arguments at length, and to demonstrate their absurdity. We will, however, present them with a specimen of the foggy notions in which Dr. Heidler indulges. It is well known that in certain neuralgic cases there is great sensibility to currents of air, to sounds, to light, to heat; and that paroxysms of pain are readily induced by these stimuli. Dr. Heidler thus discourses on this matter, and develops his theory:

“Lastly, it must not here be left out of consideration, that both kinds of occasions of increase of pain through light and heat, are amongst the obvious supports of my proposition, that pain is caused through the blood. Such a support to this proposition is—*a*, the increase of pain in consequence of the increase of blood in the affected parts, occasioned by the bio-chemical affinities of the blood for light and heat; and, *b*, the increase of pain in consequence of the increase of blood, the result of the vibrations in the atmosphere, caused by light and warmth. It is easily understood that the blood will be for the moment increased in quantity by the vibration communicated to the air, and the blood be morbidly congested in the morbidly painful tissue.

“Is not this communicated vibration plainly no other than a diminutive and retarded likeness of the pulse of the blood—of this other larger and slower modification of my law of periodicity of all-life in creation—the most fundamental and general of all natural laws? (vide §§ 15, 16, &c.) Do we not see very acute pain increased with each stroke of the pulse? And what is this vibratory motion of the blood (and of its vessels, and all solids)—this image of the pulse on a reduced scale—what else are they both than a small ebb and flow of the blood in a closed space? What else are they than a removal of the aggregated parts into another, and the one into the other, and a compression and heaping up there from whence they are removed; through each pulse-stroke, as well as through each vibratory impulse? What else is both, than that relatively excessive (arterial) congestion in the small vessels above [quoted] from the programme to the ‘new theory of pain’ (better, theory of sensation)?

“Further: truly what else is this increase of pain, effected by means of these pulse-strokes and vibratory impulses, than the twice-diminished similitude of all periodic efforts of nature; of the curative as well as the destructive exacerbation and remission—of the paroxysm and of the intermission of disease,” &c.&c. (p. 218.)

This extract is a fair example of our author’s lucubrations, and the theory which he proposes as a substitute for that which he has so diligently destroyed. It has been our fate to grope through many cross fogs of the German intellect, but we never yet were so absolutely at a loss for one guiding ray as during our pilgrimage through Dr. Heidler’s fog. One thing is evident; Dr. Dickson (the chrono-thermal sage) will be down upon him for plagiarism. There is mention made of “my law of periodicity of all-life in creation.”

With regard to the pathological and therapeutical views of Dr. Heidler, we have only to remark that they are simple enough, and old enough. Congestion of the abdominal viscera, either induced by the suppurative hemorrhoidal discharges, or accompanied by them, is the proximate cause of neuralgic and spasmodic affections. The treatment recommended is to keep the bowels open.

## ART. XXIII.

*Liebig's Physiology applied in the Treatment of Functional Derangement and Organic Disease. With Observations upon Hahnemann's Practice.*—Part I. *The Heart, Lungs, Stomach, Glands, Joints, Bones, &c., with Cases, showing the Advantages of Modern Science over Former Methods in the Treatment of Disease.* By JOHN LEESON, Practitioner of Medicine, Divisional Surgeon of the Metropolitan Police Force, &c. &c.—London, 1846. 8vo., pp. 219.

AMONG the penalties, which are attached to any post of distinction, is the impudent assumption of a name which has acquired eminence or notoriety of almost any kind, to force into public notice the productions of those pretenders to science, whose ignorance and venality are about upon a par, their sole idea of it being—what it will fetch in the market. Thus, we have seen Liebig's Manure, and Liebig's Beer advertised like Locock's Pulmonic Wafers, the Bug-destroyers to her Majesty, the London University Wine-vaults, the Great Western Shoe-shop, and the like. And we now have Liebig's Medicine for the Million advertised, not by a professed empiric, but, we are sorry to say, by an M.R.C.S., a Fellow of the Royal Medical and Chirurgical Society, and a Divisional Surgeon to the Metropolitan Police Force.

We shall not inflict upon our readers any extended demonstration of the justice of the comparison we have made; for we think that a few short extracts will make it apparent, that, so far as his literary labours are concerned, Mr. Leeson may be regarded as the legitimate type of those *stunkeys*—in Carlyle phrase—who don the livery of a great man, and then strut about with airs of arrogance which they think conformable to his dignity, betraying their ignorance of every thing that constitutes true gentility in every word and movement.

Let our readers scrutinize the following brief quotations (the italics in which are our own), and say if we have judged too harshly.

"The theories propounded by Liebig, although not altogether new in many instances, are nevertheless put forward *under appearances of greater perspicuity than was accomplished by any other philosopher.*" (p. 24.)

"There can be no other way of explaining the laws of life and sustenance with any thing like certainty, than by chemical analyses; as the resulting phenomena determine without much ambiguity the nature of the one and the manner of the other." (p. 24.)

What a profound conception is here manifested, of the nature of the relation between chemistry and vitality?

"Heat is *generated out of the union of carbon and oxygen, and also out of hydrogen and oxygen; nutrition from the food, out of which are produced reparation and excretion.*" (p. 35.)

"The *action of oxygen upon the living body is a demand for food or for death. The food discloses all the elements, by which the destructive influence of the oxygen is neutralized, and every part and tissue of the system maintained.*" (p. 26.)

"The *gases derived from the food, and by which animal life is protected, are carbon, hydrogen, and nitrogen,—they act in all the chemical combinations with the oxygen of inspiration, the human body itself instinctively regulating all their*

*movements and adaptations* in such a manner, as, in some instances, to be beyond the comprehension of our ablest philosophers." (p. 27.)

"However the alcoholic solutions may approximate in their amount of carbon and hydrogen to fats and oils, the difference of composition in each class is yet sufficient to maintain the most distinctive characters between them . . . . The *rugged cares of life* are said to vanish before the influence of the alcoholic solutions; while the *fully ones*, in small proportions, often produce nausea, sickness, and biliary derangements." (p. 30.)

Here we have a glimpse of a new and hitherto unexplored region in the wide domain of a sublunary sorrow. "O reader, if that thou canst read," didst thou ever before read of the *adipose cares of life*? Perhaps not: but doubtless we all have witnessed such, with their "nausea, sickness, and biliary derangements," in the portly aldermen and jolly Falstaffs of our corporation.

One of the most apposite references to the great master, whose pupil he desires to be accounted, is the following:

"Liebig wisely says that 'the deprivation of food soon puts a stop to all manifestations of vitality.' It would be difficult to dispute the soundness of this aphorism, when we can so readily discover in ourselves the depressive effects of even a temporary suspension of the ordinary means of nutrition." (p. 36.)

Wisely, indeed!—almost as wisely as if our own Tuson had spoken. Difficult to dispute!—so difficult, that the fact was known, we will venture to say, almost as soon as there were men on the earth to know it; and has continued to be a matter of the most familiar experience ever since. We scarcely think that Professor Liebig, although it might suit his argument to remind his readers of a fact so trite, ever dreamed of being cited as the author of the "aphorism." And we suspect he would scarcely own the following, although the statements profess to be but expositions of his doctrines:

"Animal and vegetable fibrine, albumen, and caseine, or the proteinaceous compounds, must all be resolved into their original elements, before the system will admit them to become parts of itself." (p. 37.)

"The urine consists principally of water, which enters the stomach as food and drink; after which it is taken into the circulation, where it becomes impregnated with the oxygen of inspiration, together with the various gases or vapours which are mixed or mechanically suspended in the atmosphere with which we might be surrounded; from thence it is transmitted to the kidneys for secretion into the bladder, from which it is expelled as necessity dictates." (p. 61.)

"Large quantities of the *ammoniacal gases* are taken in by the inspiratory process; and after having traversed through the circulatory system, are exhaled by the skin, as well as passed in the urine with little alteration, or combined with other gases, and with them form those ammoniacal salts which often appear in the urinary excretions. Large quantities of *carbon* are known to be constantly floating in a London atmosphere; and while it is the business of the system to expire carbon and oxygen, the same system is constantly taking in carbon, which is as often expelled through some or other of the excretory channels, and particularly in the urine under some or other of the *ammoniacal combinations*." (p. 63.)

Save me from my friends, may well be Liebig's exclamation, if he should chance to meet with such perversions of his meaning as are contained in the foregoing extracts.

So much as to the correctness of the exposition, which Mr. Leeson gives, of the *principles* of Liebig. The motive which dictates this enunciation

of his *practice*, will at once appear from the following extract, which relates to no slight disease, but to nothing else than *consumption* !

“ This view of the subject has often led me to the best method of treating this fearful disease, and with the most wonderful success. Cases for illustration I consider to be needless, as [the] soundness of the principles must appear obvious to the simplest understanding,” (p. 98.)

There seems no other disease, among those to which allusion is made in this treatise, in which Mr. Leeson's practice does not claim equal success ; and the question, therefore, naturally presents itself—Do Mr. Leeson's patients *ever* die ? We have carefully scrutinized his account of his treatment ; and whilst we have seen not a single idea worth anything, that is not already familiar to the mind of every well-educated practitioner, we observe continual omissions and errors, in regard to points as well understood as those just now cited. Consequently, the assumption of superiority, evident throughout the work, is so far from being justified by its contents, that Mr. Leeson's worst enemies—if enemies he has—could scarcely desire a more complete exposure of his ignorance and arrogance than he has here volunteered to present to the public. We denounce his book as disgraceful to any “ Practitioner of Medicine,” of whatsoever rank or name, and experience a sort of horror at the thought of its title making it be sought after and read by some of Liebig's learned countrymen. What a dainty dish of British medical literature and science to set before the stranger !

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#### ART. XXIV.

*On the Analysis of the Blood and Urine, in Health and Disease ; and on the Treatment of Urinary Diseases.* By G. OWEN REES, M.D., F.R.S., F.G.S., &c. Second edition. London, 1845. 8vo. pp. 218. With a plate.

WE have too long delayed noticing this very useful treatise, which may almost claim to be considered as a new work, rather than as a new edition of an older one, so great is the amount of alteration and addition it has received. These are in part occasioned by the advances which the science of animal chemistry has made since the first edition was published ; but they partly consist also of the introduction of an essay on the treatment of urinary diseases, which nearly equals the original treatise in bulk. It is not the author's desire that his work should be regarded as an elaborated survey of the blood and urine in all their relations, but merely as a guide to proximate animal analysis, and to the practical application of the results of such analysis. His object has been throughout, he states in his preface, “ simply to supply to the medical practitioner the means of analysing blood and urine, and thus to assist him to comprehend and appreciate with more defined ideas the experiments of those animal chemists whose results may have reference to the important subject of humoral pathology.” A description of the microscopical appearance of urinary deposits, with illustrations, has also been added.

We are well assured that the analytical portion of this treatise is of peculiar value to the medical practitioner, supplying him with all the information he can require for ordinary purposes, without distracting his

attention and burdening his memory with an extensive code of directions, which have reference to substances of rare occurrence, and which are consequently adapted for the exclusive benefit of those who make organic chemistry their special object of pursuit. We do not hesitate to say, that by following out the course which Dr. Rees has thus cleared, any medical man possessed of but ordinary skill in manipulation, at the expense of a little time and attention, may render himself so far conversant with the processes described as to be able to perform them all, as occasion may arise, in those intervals which occur in the professional avocations of almost every one; thereby securing a greatly-increased accuracy of diagnosis in a large and most important class of complaints, and also adding in no trifling degree to his power of discerning the true value of the researches of others, and of applying their results.

In regard to the practical portion of the work, we think Dr. Rees would have done wisely in not connecting this essay with a treatise which had acquired for itself the reputation of giving almost everything that it is desirable for the practitioner to know, on the subject which it professes to discuss; for its chief fault is its meagreness of detail, both in regard to the symptomatology of the diseases of which it treats, to the constitutional states with which they are connected, and to the treatment which will be most appropriate under different conditions. It appears to us that Dr. Rees underrates the influence of diet upon lithic-acid deposits, and in diabetes; and that he neglects an important principle in the treatment of the phosphatic deposits. Doubtless it is not in every case that these dietetic principles can be rigidly carried out, which the pathology of the two former of these disordered conditions would dictate; but where the system *can* accommodate itself to them, we feel assured, from evidence we have been at much pains to collect, that great relief may be given, and, in certain cases, even a permanent cure effected, by these means alone. Dr. Rees does not feel disposed to assent to the opinion of those who would connect the phosphatic deposits and alkaline urine with disorder of the nervous system; and he considers that, in by far the largest proportion of cases, the alkaline character is only acquired by the urine, after it has left the kidneys, in consequence of an unduly irritable state of the mucous membrane of the urinary passages, occasioning an excessive secretion of alkaline mucus. As a test of the correctness of this view, he adduces the benefit derived from the use of alkaline remedies in rendering the urine acid, and relieving the other symptoms; his idea of their *modus operandi* being, that they render the urine less acid and irritating at the time of its passage from the kidney, and consequently remove the cause for the undue secretion of mucus. Without denying that such may be the true pathology of certain forms of phosphatic deposit, we would take this opportunity of stating, that the result of many inquiries made subsequently to our review of Dr. Golding Bird's work, has fully confirmed the opinion we then expressed,—namely, that any excessive *waste* of the nervous system is a direct cause of phosphatic deposits in the urine. And we understand that the researches of Dr. Bence Jones, lately communicated to the Royal Society, and shortly (we hope) to be published, afford much additional evidence to the same effect.



## PART SECOND.

## Bibliographical Notices.

ART. I.—*Memoir on the Field Carriage of Sick and Wounded Soldiers in the Bengal Army.* By J. S. LOGIN, M.D., Surgeon to the British Residency at Lucknow, Superintendent of H. M. the King of Oude's Hospital, late Surgeon to the Commander-in-Chief in India, &c. &c. Printed at H. M. the King of Oude's Lithographic Press. With coloured drawings.—*Lucknow*, 1844. pp. 36.

THE object of this Memoir is to bring under the notice of the authorities in India a new litter for the transport of sick and wounded soldiers, invented by the author. The objections to the *doolie* at present in use in the Bengal army are pointed out by Dr. Login to be;—the large number of bearers required—the establishment for an European corps of 1000 men being 630—the consequent expense, and the inconvenience of encumbering the army with followers; the inapplicability of the doolie to mountain passes, or to uneven ground; its great bulk; the delay to which this necessarily gives rise in defiling through a narrow pass, or crossing a river in boats; the necessity for having bearers accustomed to the work: these, by existing arrangements, are not under strict discipline as soldiers, and frequently desert at times when their services are most required.

Dr. Login proposes to substitute for the doolie a litter constructed of bamboo, of which he has given several drawings in the Memoir. The advantages it possesses over the common doolie are, that it is little more than half the weight; that it forms an hospital cot, if required; that it can be carried by two men instead of four, or by camels, mules, elephants, ponies, &c., or on carts; when not in use it can be packed up in a small space and carried till required, and in this form could be transported much more rapidly through passes or across rivers.

Dr. Login also proposes that corps of hospital Lascars should be raised to carry these litters, instead of hired doolie-bearers, and who might be employed, when in cantonments, as hospital orderlies and servants, and be required to furnish sentries for the hospital. This arrangement would be productive of saving, compared with the present system, when a regiment took the field; but as this is the exception, and as regiments in India change their quarters, under ordinary circumstances, only once in three years, the expense of keeping up these corps when in cantonments will prove, we fear, a fatal objection. There can be no doubt, however, that such an establishment would be attended with many advantages.

The subject of the transport of sick and wounded soldiers is one of

very great importance in India; and at the present time, when it is requisite to assemble such armies as that of the Sutlej, any arrangement for reducing the number of camp followers is deserving of consideration, especially when it promises at the same time to prove beneficial to the soldier. Dr. Login is entitled to great praise for having, in the midst of his other duties, devoted to it so much time and attention. The arguments he has advanced in favour of the proposed system are such as to establish a clear case for affording it a fair trial; and we trust the military authorities in India will give it one on an extensive scale, and under his superintendence.

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ART. II.—*Lettre sur la Syphilis ou Vues Nouvelles sur la Nature, et le Traitement de la Maladie Vénérienne*. Par F. S. RATIER.—Paris, 1846. *New Views of the Nature and Treatment of Syphilis*. By F. S. RATIER.—Paris, 1846. 8vo., pp. 100.

WE cannot discover in this brochure of M. Ratier that the contents in any way correspond with the title. Our author admits one species of primary sore, and one variety of constitutional disease only (the papular eruption) as characteristic of syphilis properly so called. These are considered specific, and the treatment recommended is that which English surgeons are commonly in the habit of employing. Other symptoms the result of sexual intercourse, as the various forms of gonorrhœa, balanitis, vegetations, bubos, &c. &c., are due to a variety of causes, but are not purely specific, nor is the treatment suited to the first forms at all applicable to the second. In all this there is nothing to detain us. We are sorry we cannot extract from M. Ratier's letter any thing either in reference to the pathology or treatment of venereal diseases new or useful to the English reader.

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ART. III.—*Collectanea Medico-Chirurgica; or, Cullings from the Case-Book of a General Practitioner in Medicine, Surgery, and Midwifery*. By W. MACLURE, Esq., Surgeon, &c. &c. &c.—London, 1845. 8vo. pp. 192.

THIS collection of cases, the author in his preface informs us, is given to the public with a view "in some degree to exhibit the nature and extent of the labours of the general practitioner," and to show the injustice, as Mr. Maclure believes, "of the recent attempts of the Council of the College of Surgeons of England to degrade him" [the general practitioner] "below his recent level." The author makes the singular announcement, that in his miscellaneous volumes, he does not consider it necessary to "load his pages and swell the size of his book" with the "uninteresting details" of ordinary practice, "*such as*" the protean forms of gastric and hepatic derangements, of gout and rheumatism, of the operation for hernia, stone, tumours, &c.; yet these and the other diseases enumerated by Mr. Maclure (p. vi.) are at once the most important and the most common, and in regard to which no man would consider "details," if judiciously selected and stated, as "uninteresting" or unneeded.

The volume is multifarious in its nature. One or two of the subjects

discussed have little bearing on practical medicine, and others have nothing very unusual to recommend them. There is one case, however, extremely creditable to Mr. Maclure's surgical management. It is headed "Partial obliteration of the urethra cured by the caustic potash;" and although it has been already elsewhere published, a slight recapitulation of some points in the treatment may here be given.

The patient when he came under the author's care, had been for sixteen years a sufferer from a fistulous aperture in the perineum, through which his urine was discharged instead of by the urethra. The disease was originally caused by chancrous sores which spread downwards from the groin. The orifice of the fistula, with its glistening lining membrane, was situated rather less than half the distance between the scrotum and anus. A probe could be introduced by the fistula backwards into the bladder, but not forward to the glans penis, being stopped by a cul-de-sac, in which the anterior portion of the canal terminated. The obstruction was situated five or six inches from the orifice of the urethra, and *seemed to occupy an inch and a half of the canal*. The treatment consisted in the application of an armed bougie, which was first introduced on the 15th of November, 1822. On the 17th of January, 1823, a catheter could be passed into the bladder. On the 20th, the urine was withdrawn by means of a gum-catheter, introduced along the urethra, instead of flowing by the fistula. On the 26th of February, the patient regained the natural use of the canal.

There are several other papers of some interest, though few of them are sufficiently so to justify their being resuscitated from the pages of the journals in which they originally appeared.

ART. IV.—*An Atlas of Anatomical Plates of the Human Body, accompanied with Descriptions in Hindustani*. By FRED. G. MOUATT, M.D., assisted by MOOUSHI NUSSEERUDIN AHMUD.—*Calcutta*, 1846. Fol.

It gives us much pleasure to notice this book, which is printed in Hindustani and edited by Dr. Mouatt. This attempt to diffuse the medical knowledge of the West amongst the inhabitants of the East must meet with the warmest approbation not only of us, who watch over and hail with pleasure the progress of medical knowledge—not only of all well-wishers for the improvement of the Hindoo, but of all philanthropists. This publication is addressed to the military class of the College of Calcutta, and is designed to remove, through the education of the native doctors, so to speak, the ignorance and prejudice of the people. Hitherto Europeans have, for the most part, given their translations of medical works in characters that were not understood by the lower grades; but the editor of the present work has attempted to teach the people by publishing in a language that is understood by them; therefore, the book has been printed in Urdu, "the vernacular language of the people, the vulgar mother-tongue of the mass." From the want of books for the native students in the college, Dr. Mouatt and others have been led to undertake the labour of compiling and translating the following works: a *Manual of Anatomy and Physiology*, one of *Surgery*, one of *Practice of Medicine and Midwifery*, and one of *Materia Medica*.

The work on anatomy and physiology has been undertaken by Dr. Mouatt, who is assisted by a native, Mooushi Nusseerudin Ahmud. It will consist of an atlas of plates, with accompanying description, which will be followed by a manual of anatomy and physiology; the first part of the Atlas now sent to us from Calcutta, contains drawings of the bones, which are copied from Cheselden's very excellent plates. In the other parts—in all five—it is proposed to give the vessels, the nervous system, the viscera, and organs of sense. There is not any mention made in the preface of illustrations of the muscular system, and the ligaments joining the hard parts of the skeleton. Surely this omission must be from oversight; for what kind of knowledge can be gained of the vessels or nerves without a previous acquaintance with the muscles?

Besides the utility of these works to the native student in medicine, their appearance will be scarcely less beneficial to the junior members of the medical staff in service in India; and we trust that the medical officers of the East India Company's service will use them as a means of familiarizing themselves with the language of the Sepoy committed to their charge. We feel strongly the truth of the remark of the editor, that "no surgeon in charge of a jail or a regiment can be perfectly efficient, or discharge his duties with credit to himself and benefit to his patients, who is entirely ignorant of their tongue, and incapable himself of ascertaining the exact nature and extent of their complaints."

It is because we wish well to this undertaking in the East that we would venture to suggest to Dr. Mouatt the necessity that there will be for his most unremitting attention to the execution of the illustrations. In the progress of a work the artist is so prone, as the novelty wears off, to relax in the ardour necessary to the perfection of the plates. Indeed it is because we notice now some slight disposition to omit the clearness and precision so remarkable in Cheselden's plates that we are induced to offer these remarks. To Dr. Mouatt's name we shall look for security for the fulfilment of the terms of the contract; and we are sure Dr. Mouatt is more desirous of reputation than of the *éclat* of having produced a book.

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ART. V.—*The Sanative Influence of Climate.* By SIR JAMES CLARK, Bart., M.D., F.R.S., Physician in Ordinary to the Queen and to the Prince Albert. *Fourth Edition.*—London, 1846. 8vo., pp. 412.

THE almost unexampled event of a genuine medical work—neither a school-book nor a book written expressly for the public—coming to a *fourth edition*, is assuredly a striking indication of its character and the estimation in which it is held, and may well justify our dismissing it with brief notice. In our number for July, 1841 (Vol. XII., p. 160), we gave an outline of the contents of the Third Edition, and an analysis of its principal sections. The present edition is substantially the same work, the former plan being preserved; but the book is much enlarged by the addition of a great deal of new matter of importance, and is moreover much improved by alterations and emendations in every chapter. Among the improvements we may mention new articles on Bournemouth on our south coast, Egypt, and various places in the southern hemisphere; great additions to the article on Madeira; and an entire remodelling of the

meteorological tables—now the most extensive and accurate series which have ever been constructed.

Sir James, with characteristic modesty, tells us, in his preface, that “with all the improvements which he has been able to effect in it, the work is still to be regarded only as an essay which future and more extended observation will be required to perfect.” We venture to express our belief that it is as complete now as the present state of our knowledge admits of its being made. He adds, that “although he has seen no reason to change his opinions on the characters of the different climates treated of, yet that the information he has continued to receive from others, added to his own increased experience, has enabled him, with more confidence and precision, to lay down rules respecting the adoption of certain climates to the cure of particular diseases.” This is the natural and necessary result of time; and we hope the author may yet find leisure enough to give us yet more, and more complete, editions; but as Sir James has already, we believe, got somewhat beyond the *mezzo del cammin*, and is, moreover, one of the busiest of doctors, we may reasonably enough presume that the work has at length received the author’s last touches, and will go down to posterity in its present form—one of the most original and most classical productions of the medical press of our time. It is, indeed, in every way a classical work; and as it is the result of long and accurate observation and experience, and has no connexion with our transitory opinions or professional fashions, it stands a fair chance of being one of the few medical books of the present day which will be familiar to the readers and practitioners of other days. For ourselves, we owe to the work a debt of especial gratitude, as having in its earliest form of “Notes,” full five-and-twenty years ago, directed our attention to the curative influences of Nature in chronic diseases, and thus led us to the adoption of views and plans of treatment, which have been to ourselves the source of infinite gratification, and, through us, may—we would fain hope—be of some slight benefit to the profession and the public. Of the rational system of treating diseases, in accordance with Nature not in opposition to her, the author of this volume has always been the enlightened and consistent advocate; and we believe that for the improved plan of management in many chronic diseases, now more prevalent in this country than formerly, the profession is not a little indebted to his writings and his example.

We need scarcely add any formal opinion of the volume before us. There are few whom its perusal will not both gratify and instruct. To all who desiderate a knowledge of the sanative influence of climate, both at home and abroad, it is indispensable.

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ART. VI.—*On the Temperature of the Earth and Sea, in reference to the Theory of Central Heat: a Lecture delivered at the Royal Institution.* By ALFRED S. TAYLOR, F.R.S., &c.—London, 1846. 8vo, pp. 32.

THIS is an excellent Lecture on a very interesting point of scientific inquiry. It contains all the knowledge we possess on the subject of which it treats, and this knowledge disposed clearly and in an agreeable form. We think there are few readers whom it will not interest, and not many whom it will not instruct.

ART. VII.—*Phrenology—Its Nature and Uses: an Address to the Students of Anderson's University, at the opening of Dr. Weir's first Course of Lectures on Phrenology in that Institution, Jan. 7, 1846.* By ANDREW COMBE, M.D.—*Edinburgh, 1846.* 8vo, pp. 32.

It will interest some of our readers to learn, from the title page of Dr. Combe's Address, that a Chair of Phrenology has been instituted at Glasgow, in that flourishing school of medicine known as "Anderson's University." Respecting the propriety or the judiciousness of such a proceeding persons will differ, there is no doubt, according to the views which they entertain of phrenology itself. As on several former occasions, we have supported its claims to scientific investigation, believing it to embody much that is true, whatever may be the imperfections characterizing many of the details, we frankly avow ourselves to be gratified at the liberal step which the managers of the school in question have taken.

If we had not more substantial reasons for recommending this subject to the notice of the profession, one might yet be found in the fact that phrenology offers to the physiologist, at the very lowest on *plausible* grounds, a rational exhibition of the functions of particular parts of the brain. This is a want in physiology that has long been felt; and if Gall have not, in some degree, supplied this desideratum, the labours of other physiologists have certainly not been more successful; for although theories have been propounded, none of them have exhibited any solid foundation in fact, nor yet, by their antecedent probability, have they been enabled to attach to themselves any considerable number of adherents. Phrenology, however, has for many years, and in many countries, been received and advocated by numerous able and estimable physiologists, as furnishing a true physiology of the brain; and we hold that, under such circumstances, it cannot be fairly rejected without careful examination of the natural facts upon which it is stated to rest. Observation alone can determine what is true and what is false in phrenology; but we much wish, in reference to this point, that inquirers would seek to determine, as a preliminary step, the just *method of investigation*, and the *value of evidence* in relation to cerebral physiology; for as observed, apparently with justice, by Dr. Combe, "it has been the want of a sound method of investigation, and not any inherent difficulty in the subject, or any marvellous complexity of function, which has hitherto constituted the chief obstacle to success. Nature's laws and operations rarely remain wholly inaccessible to well-directed and persevering inquiry, and they seem to be a maze of confusion and contradiction only when considered in a wrong point of view, or when examined apart from their natural relations to each other."

There are many reasons, indeed, why Gall's physiology of the brain should no longer experience neglect from the members of our profession; amongst others may be adduced the circumstance that, if true, it must of necessity form the basis of all rational treatment of the insane; and already many experienced and successful practitioners in this department testify to the advantages they derive from this physiology. The whole question is fundamentally one of fact, and as such it should be regarded; all mere *opinion* is obviously inadequate to the formation of solid conclusions. Dr.



Combe says, "Where mere opinion is brought against what I know, from direct, careful, and repeated observation, to be *clear* and *positive facts*, no matter how eminent the source of the opinion may be, I stand firm and unmoved, because Nature is at my back, and I have the fullest assurance that she commits no mistakes, and is never inconsistent; and I know that, on appeal being made, she will be found to speak the same language to-day as yesterday, or as a thousand years ago, and to bear out all I have advanced, if I have really been accurate in my observations." This language expresses sound philosophy, it must be admitted, whatever be thought of the particular subject to which it is applied.

Dr. Combe, in maintaining the necessity of inquiry, observes, "You cannot with safety continue to neglect this inquiry; because the truth is advancing while you are inactive, and you are not in possession of any other knowledge which can warrant you in condemning the claims of phrenology untried. In common fairness you are bound, at least, to make yourselves acquainted with both sides of the question, and to suspend your judgment until you have done so." We readily adopt this sentiment.

The address itself needs no other commendation at our hands than that, in thought, style, and illustration, it is characteristic of Dr. Combe.

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ART. VIII.—1. *Outlines of the Arteries, with short Descriptions*. By JOHN NEILL, A.M., M.D.—Philadelphia, 1845. Royal 8vo.

2. *Outlines of the Nerves, with short Descriptions*. By JOHN NEILL, A.M., M.D., Demonstrator of Anatomy in the University of Pennsylvania, &c.—Philadelphia, 1845. Royal 8vo.

THESE two books are abridged descriptions of the vascular and nervous system of the human body, with illustrations, and belong to a class of works that have for their object the making anatomy easy. Now, much as we should desire to see that end attained, we must assure our readers that the royal road to it will not be found in these books. We think the writer must hold that common but erroneous opinion, that an abridgment of the science of anatomy is made by the omission of important facts, and the disregard of common accuracy in statement; so that brevity and generalities are used by the authors referred to as the recipe for the manufacture of the several 'Outlines,' 'Manuals,' 'Guides,' &c. for the use of students and beginners. Fully conscious of the advantage of a good elementary treatise to guide and encourage the learner in his labours, we must protest against such books as these. We generally look suspiciously at first books of young authors, published for the purpose of simplifying difficulties, for such are commonly found to be intended to bring the writers into notice, rather than to assist the inquirer after knowledge. We have examined with some care the books of Dr. Neill, and are forced to state that, as publications, they are neither suitable to the present time, nor calculated to bring honour to the author: they are 'Outlines' made after the worst type.

The pamphlet on the arteries,—it contains but 30 widely-printed pages,—is certainly not fitted to advance the knowledge of the vascular system of the body. The description is loose, unprecise, and often inaccurate,

giving in fact little more than what might be inferred from the name of a vessel. All recent observations on the anatomy and surgical anatomy of the vessels are entirely omitted; and the badness of the illustrations exceeds any thing we have seen. With one or two exceptions that seem to be copies from dried parts of the body, the figures are merely imaginary, and therefore full of errors; arteries that should be covered by muscles are represented altogether on the surface of the body, as in Plate I; and in many places the descriptions and the drawings do not correspond.

The part containing the nervous system has been cast in the same mould as the other; it contains the same number of pages—almost the same number of lines. If both treatises were not so faulty as to render it difficult to say which is most imperfect, perhaps that on the nerves, though more aspiring in the way of illustrations, might at once be pronounced the more erroneous of the two. Regardless of the progress that has been made in the anatomy of the nervous system within the last few years—we will not say unacquainted with it—the author adopts the old, and now considered erroneous views of the sympathetic and the ganglia in connexion with the fifth cranial nerve. And there is moreover omission of fact and inattention to accuracy all through the book. It would appear as if the writer had taken some old copy of a system of anatomy, and compiled his ‘*Outlines*’ from it without discrimination.

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ART. IX.—*Percussion und Auscultation des Herzens im gesunden und kranken Zustande*. Von LIBERAL GÜNZBURG, M.D. Zweite Auflagen. —Wien, 1844.

*A Treatise on Percussion and Auscultation of the Heart in the Healthy and Diseased States*. By LIBERAL GÜNZBURG, M.D. Second edition. —Vienna, 1844. 8vo., pp. 179.

ALTHOUGH the soundness of the novel notions which Skoda has put forth concerning the mode of production (and, indeed; in some cases, the pathological signification) of various phenomena of auscultation and percussion, may very fairly be questioned, and though those notions have not been admitted, nor are likely to be admitted, by any but his immediate pupils and those whom local influences may be expected to convert, yet it cannot be denied that his writings have been of very material service. In truth, the Germans now feel a sort of property in auscultation, and are stimulated to make its mysteries more and more completely their own; whereas, before the advent of Skoda, their ignorance of and indifference generally to the subject, were matters of remark on the part of all who visited their schools. There is now a glut of manuals, treatises, &c. of auscultation in the German market, even greater than in our own or in that of France.

It is not sufficiently known (ingenious devices have in this country been employed to throw the fact into the shade) that Dr. Carswell was the real proposer of the theory of the second sound of the heart, ascribing the phenomena to flapping of the semi-lunar valves. It is, on the other hand, well known that, after that theory had been put forward by Carswell, Billing, and Rouanet, several persons refused to accept it; Dr. Williams, for example, (in 1833) held that the second sound was produced by ten-

sion of the parietes of the ventricles. Such being the facts, it scarcely surprised us to find, in this manual, no mention of Dr. Carswell's name in connexion with the point. Skoda's fantastic theory of the cardiac sounds (as fantastic in its way as any product of any German brain) is not accepted by the author.

Dr. Günsburg makes no reference in his description of the origin of pericarditis to the statement of M. Gendrin to the effect that bulging of the pericardial regions occurs previously to the production of fluid effusion in the pericardium. We do not quarrel with him for this; we believe the appearance in question to have existed very much in the "mind's eye" of M. Gendrin, and nowhere else. The discovery was in every way worthy of that remarkable acuteness of vision whereof M. Gendrin gave such marvellous proof in times of yore by "*seeing*" (this was the word,) the *red corpuscles of the blood* converted into *pus-corpuscles within* the capillary vessels, and transuding thence ready-formed into the circumambient textures!

Besides the exposition of the physical characters of diseases of the heart, this work contains full tabular views of pulmonary and cardiac diseases "in their diagnostic and anatomico-pathological relations, after Skoda and Rokitansky, with an appendix on their treatment." The title of this portion of the work gives a fair insight into its general character and pretensions: it is, in truth, got up "after" the labours of others,—a simple and correct compilation, but nothing more.

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ART. X.—*Urologie. Traité des Angusties ou Rétrécissements de l'Urètre, et de leur traitement rationnel.* Par le Dr. LEROY-D'ETIOLLES.

*Urology. Treatise on Strictures of the Urethra and their rational treatment.* By Dr. LEROY-D'ETIOLLES.—Paris, 1846. 8vo., pp. 88.

UNDER the above fanciful title is ushered into the world a work by a man who is no doubt well acquainted with the subject of which he treats. It is one of a class written not because the author had anything new to say, but because he conceived that particular circumstances rendered it advisable that the work should be published. In 1838, the Marquis d'Argenteuil left by will to the Académie Royale de Médecine 30,000 fr., "to be placed with the interest that it will produce from the day of my death in the French Rentes, and of which the accumulated revenue shall be given every six years to the author of the most important improvement introduced during that time into the curative treatment of stricture of the urethra. In case this part of the art of cure shall not have been the object of any improvement sufficiently notable to merit the prize which I have instituted, the Academy may award it to the author of the most important improvement, during the same period, in the treatment of other diseases of the urinary organs." The Academy required that all papers should be delivered by the 21st of September, 1844, and declared the value of the prize to be upwards of 8,000 fr.

Twenty-seven memoirs were received, among which we conclude was that of M. Leroy-d'Etiolles. On the 25th September, a commission was chosen to examine and report. It was composed of MM. Jobert, Amussat, Villeneuve, Bérard, Lagneau, Begin, Ségalas, Civiale, Jourdan. On the 17th of December, the Academy decided that the number of competitors

was too great to admit of the adjudication of the prize at that public sitting, and that it would be adjudged in the course of 1845, when its value will be 10,430 fr. 50 cent.

Such, it would seem, is the circumstance to which we owe the work now before us.

We have not found in it anything new or important which would, in our judgment, entitle it to the munificent bequest of M. d'Argenteuil. And we have to complain of the mode in which everything is dilated: for instance, we have 45 pages, closely printed, on the nature of stricture; 27 pages on the seat and causes; 45 pages of diagnosis; and 250 pages of treatment; and by way of make-weight, if the stricture part should fail, 35 pages on vesico-vaginal fistulæ: besides all this, we have interminable cases of barons, deputies, diplomates, princes, councillors, lords, poets, counts, &c., some occupying five or six pages, and in number amounting to near 100; and what to us is still more unpleasant, constant squabbles with M. Civiale, M. Velpeau, &c. This is not the plan on which a good practical work should be written; though we presume that is what M. Leroy-d'Etiolles desired to do—and what, in our conscience, we believe him quite capable of doing. He has had much experience; is a man of good sense, and generally expresses himself well; and we think his book contains most things that need be said on the subject; but these things are mixed up with so much that was not wanted, that they are not easy to find. Sir B. Brodie required only 72 smaller pages to say all that he thought necessary on the subject.

There is one point on which we are less satisfied with our author,—we refer to his views on the curability of stricture;—of cured cases he gives us upwards of 50, and as he does not employ any other means than those in common use, we wonder that the result of his treatment is so different from that of men of most experience among ourselves.

The observations on vesico-vaginal fistulæ, as well as the instruments for carrying them out, are ingenious enough. With reference to these cases it is fully clear that the apparently diverse results are owing to a want of accuracy in description; some are cases in which the fissure extends little beyond the urethra; those are very manageable; in others it extends along a great part of the septum; these are very difficult of cure.

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ART. XI.—*Some Observations on Organic Alterations of the Heart, and particularly on the beneficial employment of Iron in the treatment of such cases.* By S. SCOTT ALISON, M.D., M.R.P.L., and Physician to the Northern Dispensary.—*London*, 1845. 12mo., pp. 62.

THIS is a modest and sensible monograph. It is written with much moderation. There is no undue eulogy of the effects of iron in cases of heart disease: but the circumstances are carefully discriminated in which recourse to this agent promises benefit. There is much judiciousness in the doctrine, several times inculcated in these pages, that palliative treatment, even where curative is out of the question, is and ought to be a great object. The author likewise shows that some forms or modifications of organic change of the organ, are not so totally insusceptible of cure as has hitherto been by many supposed. We recommend this little volume as useful and opportune.

## PART THIRD.

## CONTRIBUTIONS TOWARDS THE ADVANCEMENT

## OF THE

## Natural History and Treatment of Diseases.

I. ON THE RECENT PROGRESS AND FUTURE PROSPECTS OF PRACTICAL  
MEDICINE.

BY ELISHA BARTLETT, M. D.,

Professor of Medicine in the University of Maryland.

[The following observations are so excellent in themselves, and in such perfect accordance with the views recently promulgated in this Journal, on the subject of therapeutics, that we feel it a duty to lay them before our readers. They are extracted from the author's excellent work on the 'Philosophy of Medicine,' which was briefly noticed in our Twentieth Volume, p. 140. The observations are the more important because we have reason to know from other sources that they express the sentiments of the best and most experienced men in the United States. This return to a milder, more rational, and more natural system of practice, has been mainly brought about, we believe, by actual experience of the vast mischief produced by the heroic treatment formerly in vogue, which treatment was carried to an extent of boldness, or violence, unknown even in England. We know, on the best authority, that, not many years since, it was the practice of a professor of medicine, at one of the American Universities, to recommend and to prescribe calomel in *tablespoonfuls*! Even in this book Dr. Bartlett, reprobating this system, tells us: "It [calomel] is constantly administered—on all occasions—in all diseases—and in all their stages. It has, literally, in some instances, been made an article of *daily food*—sprinkled upon buttered bread, and mixed with it before baking! I suppose it is no exaggeration to say, that there is more calomel consumed in the valley of the Mississippi and its tributaries, than in all the world beside."]

"The history of practical medicine, especially, during the last twenty-five years, and a right appreciation of its character, and the conditions and means of its progress, furnish us with very positive assurance that many of its most important laws will gradually, but steadily and certainly, be carried forwards to their entire and final establishment. The foundations of many of these laws,—and of those, too, most difficult of determination,—have been already broadly and securely laid; and although many years must elapse, amidst earnest, unremitting, and conscientious toil, before these laws can be *definitively* and *fully* settled, it is not possible, in the nature of things, that we can be deceived, or disappointed, in this consummation, so devoutly to be wished.

The minute and thorough study of diseases, in all their aspects, phases, and relationships, which is now prosecuted, with so much zeal and fidelity, cannot fail of leading to the result of which I have spoken. The great laws of pathology and its relations,—of etiology, and therapeutics,—are sure to be ascertained; each successive year will add something to their development, in the steady accumulation of legitimate and authentic materials, and in their disposition and analysis, so that, in the end, the *entire natural history of diseases* will be made out and written.

“In this progress of medical science, which we thus confidently anticipate, some of its branches will take precedence of others. Diagnosis, for instance, will be in advance of therapeutics; and this for two reasons. In the first place, the elements of the former are fewer, and less complex in their relationships, than those of the latter; and in the second place, diagnosis is an *essential pre-requisite* of therapeutics. These are amongst the reasons why improvements in the treatment of disease, especially for the last twenty-five years, have not kept pace with the advances, which have been made in our knowledge of disease itself. After our knowledge of pathology, and our nosological diagnosis growing out of this, have reached their highest attainable point of accuracy and positiveness, there is still left an almost interminable field of investigation, in the study of the relationships between the morbid condition, thus ascertained, and the substances and agencies in nature, which can in any way affect or influence this condition. Let us look, for a single moment, at the extent and the complexity of these relationships. They are almost infinite. Look at any single disease, even of the simplest and best settled character; and let us suppose that all its elements, as far as this is possible, in the nature of things, have been accurately ascertained. Before our therapeutical knowledge of this disease can be said, in literal strictness, to be *complete*, we must know the effects and influences, which *all the substances and agencies in nature are capable of producing upon it*; and we can know this only by direct observation of the effects themselves. We must know how it will be modified by each and all of the different vegetable productions of the earth; by each and all of the mineral substances, in their manifold forms of chemical combination; by changes of temperature, and other meteorological conditions; by light; by electricity; by food; by drink; by exercise; by the state of the mind, and so on. The doctrine, thus stated, sanctions the constant introduction and trial of new remedies; since until any given substance is tried we do not and cannot know what properties of a remedial nature it may be endowed with. *All* substances, in their remedial characters, were once new; calomel, antimony, opium, Peruvian bark, were once, and some of them not very long ago, new remedies; and any philosophy that would reject the trial of a remedy now, *because* it is new, would of course have rejected the trial of these on the same ground. But, let me say, there is no man, anywhere, who regrets more sincerely than I do the multiplication which is constantly taking place of the so-called articles of the *materia medica*. There is probably no man more entirely sceptical in regard to their alleged properties and virtues than I am. There is no man who has been in the habit of using a smaller number of them. There is nothing in the whole range of medical history, which shows so miserable a logic, and so false a philosophy, as the introduction of this multitudinous assemblage of new remedies, *with the properties which are so confidently assigned to them*. But then the fault and the error are,—not that the remedies are new,—but that the evidence of their value and efficacy is so utterly wanting. My own opinion is,—an opinion founded upon the history and experience of all the past,—that the number of substances endowed with active, and peculiar or characteristic, remedial properties is small. But whether this number is small or large can be determined only by observation and experience, or *trial*.



The true course of the philosophical physician is,—not to reject the medicine *because it is new*, but for the reason, abundantly sufficient in regard to nineteen-twentieths of the articles of the official pharmacopœias, that there is no satisfactory evidence that it is worth anything; and one of the most certain and beneficial results of a correct medical philosophy will be the final expulsion and banishment of these aliens and impostors from the domain of our science.

“Now, when it is remembered that these substances and agencies are, many of them, acting together,—that it is exceedingly difficult, in many cases, to separate the influence of one from that of another in our own endeavours to estimate the real agency of each; and, furthermore, that the elements of the disease itself, so far at least as its therapeutical relationships are concerned, are more or less fluctuating and changeable,—it must at once be seen how true it is, as I have already said, that positive therapeutical knowledge is more difficult of attainment than any other in the entire circle of medical science.

“But, notwithstanding all these formidable and inherent difficulties, this knowledge has made, within the period of which I am speaking, great and positive advances. The effects of many remedies are much better understood, and their value much more accurately appreciated than formerly. And I believe, that hereafter, this department of our science and art is destined to a more rapid and positive advancement, when compared with the other departments, than has hitherto been its lot. The first essential condition of this advancement—the accurate and positive diagnosis of disease,—has to a good degree been fulfilled. The first element in the problem to be solved has been ascertained; and we accordingly find, that the attention of many of the best minds in the profession is now turning in this direction. This is the natural course of events. The seat, the character, the regular march, and the tendencies of the disease, having been first ascertained, the next thing to be done is to find out the best methods of preventing, of modifying, and of curing it. This is what many of the great pathologists of the present day are actively and zealously engaged in endeavouring to do. This is the great mission which now lies immediately before us; this is to constitute the great work of the next and succeeding generations.

“I should be doing great injustice to my subject, if I did not mention, as prominent amongst the therapeutical improvements of the last quarter of a century, the change which has been gradually taking place, in the use of violent and dangerous remedies. I am inclined to regard this change as one of the greatest blessings which modern medical observation has conferred upon the human race; and it is but fair to admit, that absurd as the *system* of homœopathy is, and unsupported as its pretensions are, so far as its *peculiar treatment* of disease is concerned; it has, nevertheless, done great good *by its practice*,—its scrupulous adherence to a strict regimen, and its avoidance of all injurious remedies,—in the furtherance of this revolution. ‘It has been sarcastically said, that there is a wide difference between a good physician and a bad one, but a small difference between a good physician and no physician at all; by which it is meant to insinuate, that the mischievous officiousness of art does commonly more than counterbalance any benefit derivable from it.’ (Sir Gilbert Blane.) The conviction has been steadily gaining ground, and spreading itself abroad in the medical community, not only that *heroic* remedies, as they are called, are often productive of great mischief, and should never be lightly or questionably used; but that in very many cases of disease, all *medicines*, using this word in its common signification, are evils; and that they may be dispensed with, not merely with negative safety, but to the actual benefit of the subjects. The golden axiom of Chomel, that it is only the *second* law of therapeutics *to do good*, its *first* being this—*not to do harm*—is gradually finding its way into the medical mind, preventing an incalculable amount of positive ill. The real agency of art is more generally appreciated than for-

merly; and its arrogant pretensions much more truly estimated and understood. It is coming every day to be more clearly seen, that perhaps its most universal and beneficent function consists in the removal and avoidance of those agents, the action of which is to occasion or to aggravate disease; thus giving the recuperative energies of the system their fullest scope and action, and trusting to them, when thus unembarrassed and free, for the cure of the disease. 'This, I apprehend, is so well understood among well educated physicians, that the word *cure*, as applied to themselves, is proscribed as presumptuous, and rarely, I believe, escapes the lips of any practitioner, whose mind is duly tinctured with that ingenuous modesty which characterizes the liberal and correct members of the profession.' (*Sir Gilbert Blane.*)

"It is melancholy to think what an enormous aggregate of suffering and calamity has been occasioned by a disregard of the axiom which I have quoted. Our means for the direct removal of disease are limited in extent, but it is not so with our power to augment and to cause it; this is unlimited. Difficult as it may be to cure, it is always easy to poison and to kill. We may well congratulate ourselves and society, that the primary and fundamental truths, of which I have been speaking, are finding their right position, and producing their legitimate results; and that long abused humanity is likely, at no very remote period, to be finally delivered from the abominable atrocities of wholesale and indiscriminate *drugging*."

"I cannot forbear remarking, by way of parenthesis, that this evil, in addition to the many others which I have already had occasion to enumerate, has been greatly aggravated, and in many instances wholly produced, by the influence of *à priori* medical doctrines. The whole history of medicine will show that the most flagrant abuses of this character have always been the direct results of these mischievous influences."

## II. ON THE NATURAL HISTORY AND SIMPLE TREATMENT OF WOUNDS.

BY ISAAC GILCHRIST, M.D.

[Although we hope the practice described and recommended in the following paper is that which, in its main features, is now adopted very generally by enlightened surgeons in this country, its renewed promulgation in a more formal shape cannot fail to be useful. The natural treatment here illustrated is far from being universally followed; the old system of impertinent interference with Nature in all her ways being that still adhered to by many practitioners.\*

In the present medical crisis, we, moreover, reckon it of especial importance that the attention of physicians should be directed to the curative activity of Nature in surgical cases, where the whole of the processes are subjected to the senses. If we find Nature capable of doing such great things, alone, or with that little help which scientific art deems it right to apply, on the surface of the body, we might reasonably infer that similar results would ensue under similar circumstances in the interior of the body; and such, assuredly, is the fact. But we see also, in the one case as in the other, that, although the actual worker of the cure, Nature, stands often in need of the assistance of art to put her in the right road, to remove obstacles from her path, and, occasionally, to supply instruments which she does not herself possess. And in doing this the physician and surgeon have their high and true calling, and may

\* For a graphical account of this "old system" see Mr. Liston's admirable "Practical Surgery," p. 31, fourth edition. And it is but justice to this distinguished surgeon to say, that much of the modern improvement in this department of surgical treatment is owing to his precepts and example.

always find ample and legitimate employment. All that Nature demands in such cases is, that Art should not, like Dr. Johnson's patron, "encumber her with help."]

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*To the Editor of the British and Foreign Medical Review.*

Woodside, Aberdeen, 26 May, 1846.

SIR,—Allow me to express to you the extreme satisfaction I have recently enjoyed from the careful perusal of two articles which appeared in the two numbers, for the present year, of the *British and Foreign Medical Review*, on the present state and anticipated reformation of the Medical Profession. I have been fortunate enough to have been, by certain appointments, very extensively engaged in the practice of our art during the last seven years, and my experience in the observation of disease has convinced me of the soundness of your views. In fact, I have for a considerable time been putting them to the test of experiment. The accompanying paper embodies the result of that experiment in a particular branch of the science. At the request of some of my brethren, who have witnessed the success of the simple treatment therein detailed, I had intended preparing a paper for some of the other journals; but, after reading your articles on the general subject, I thought that my observations on one division of it might with more propriety be sent to you. I am, Sir,

Yours, most respectfully,

ISAAC GILCHRIST.

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In Sir John Herschel's Discourse on the Study of Natural Philosophy, it is stated that, "Art is the application of knowledge to a practical end. If the knowledge be merely accumulated experience, the art is *empirical*; but if it be experience reasoned upon, and brought under general principles, it assumes a higher character and becomes a *scientific art*." It is humiliating indeed to consider that, in this age of boasted scientific advancement, the practice of medicine and surgery must be allowed to belong more to the former than the latter division of art. Has the progress of disease in the human body been studied in the same philosophical spirit as the processes observed in the external physical world? Have we kept in view that the laws of Nature comprehend in their dominion the course of a fever and the reparation of a wound, as well as the fall of an apple? Why have we long since ceased to struggle in the pursuit of the alchemists; and why do we exercise our ingenuity in paths of discovery different from that followed in the search after the perpetual motion? It is because the study of the laws of Nature has shown us how to avoid impossibilities.

Let us remember also that a knowledge of these same laws enables us "to accomplish our ends in the easiest, shortest, most economical, and most effectual manner." It is only when the natural history of disease is studied philosophically that we can arrive at any real principles of therapeutics. The treatment of disease will only then be conducted upon sound principles when we know what can be "accomplished by Nature, and under what circumstances her operations may proceed with the greatest facility." When this is the case, there will be no more room for the craft and mystery of empiricism. Disease will not be considered an entity which must be driven out of the system by this medicine or by that.

The more unexplained any grievance is, the more numerous are the remedies usually proposed for its removal, and the greater the opportunity offered for the excitement of the strongest and blindest faith. Hence the reliance in

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former days upon charms, amulets, and incantations, and in our own, upon doctrines and systems equally baseless and absurd. Hence the expectation on the part of the patient that his medical adviser should *do a great deal* towards the removal of his disease, not knowing that certain salutary, natural processes may, by rash interference, be counteracted. Bodily disease doubtless produces impatience, and impatience too often produces imprudence; and hence Shakspeare's reproof:

"How poor are they that have not patience!  
What wound did ever heal but by degrees?  
Thou know'st we work by wit, and not by witchcraft,  
And wit depends on dilatory time"

Thus the powers of Nature being overlooked, and all sorts of remedies being proposed as capable of combating with the unseen and unknown evils that oppress us, what else can be expected but that the boldest and most presumptuous treatment shall be accepted on the part of the impatient sufferer?

In another department of art man is content to prepare the ground and throw in the seed, relying implicitly on the laws of Nature that in due season the earth shall display its fruits; but he cannot, it would appear, exercise his faith in the same laws, when flesh and blood are revealing the processes of growth and reparation in the human body. Instead of watching, in the patience of hope, the operations of Nature, and learning with how very few medicines—or it may be with none—disease may be treated, we have been busy in the use of innumerable remedies (so called) and every variety of appliance, blindly believing that when a disease has been removed, the result was owing to the medication adopted. The various kingdoms of nature have been ransacked, every substance which she presents or which chemistry can form from her materials, has been tried in the practice of medicine. Each of these articles, moreover, must be tortured into a number of different forms and preparations; and, after all, they must be combined together, to form pills, powders, mixtures, &c., without end. Of course, all this adds to the mystery of the treatment of disease; and is not such mystery considerably enhanced, by clothing our prescriptions in an unknown tongue and unknown characters?

It has been remarked that "the whole tendency of empirical art is to busy itself in technicalities, and to place its pride in particular short cuts and mysteries known only to adepts; to surprise and astonish by results, but conceal processes." Has the profession itself then but been supplying lessons to the acknowledged quack? Alas, we fear we must confess ourselves herein guilty. We have not been prescribing medicines like philosophers, we have been only rudely and blindly experimenting with drugs. We have been engaged in what Bacon calls the "anticipation of nature," instead of the "interpretation of nature." We have forgotten his wise and weighty aphorism, which, however trite, cannot be too often sounded in our ears: "Man, the minister and interpreter of nature, can act and understand in as far as he has either in fact or in thought observed the order of Nature; more he can neither know nor do."

These remarks are intended to introduce a few observations which I have to make on the Natural History and Treatment of Wounds. In the case of an external injury, we are invariably met with the demand, what application will heal or cure it? In former days they had *sarcotic* or *flesh-creating* ointments, and in our own days we have *healing cerates*, and other similar preparations without number.

Let us first inquire what Nature can accomplish in the matter of wounds; and then, how she may be aided in her operations. Dr. Macartney states, that as to the effects of injury in the different classes of animals, he found, "that the powers of reparation and reproduction are in proportion to the indisposition or incapacity for inflammation, and hence that inflammation is so

far from being necessary to the reparation of parts, that in proportion as it exists the latter is impeded, retarded, or prevented; and that when inflammation does not exist, the reparative power is equivalent to the original tendency to produce and maintain organic form and structure; that it then becomes a natural function, like the growth of the individual or the reproduction of the species." This is quite different from the doctrines formerly taught under the terms *adhesive*, *suppurative*, *ulcerative inflammations*. There is no countenance here given to Sir Astley Cooper's statement—"No wound can be repaired without inflammation."

Dr. Macartney describes the modes of reparation as follows:—1. Immediate union without any intervening substance such as blood or lymph. 2. The union by the medium of coagulable lymph, or a clot of blood. 3. The modelling process, or reorganization without any medium of lymph or granulations, the cavity of the wound becoming obliterated by a natural process of growth. 4. The reparation by means of a new, vascular, and organized substance, called granulations. In the treatment of wounds, therefore, the great object of the surgeon must be to prevent inflammation, and thereby secure reparation by any of the first three modes; if he is successful in this object, granulation and suppuration, which go together, will be obviated. The following simple rules seem to embrace all that is necessary to facilitate nature's operations:—approximate the edges of the wound gently and without much traction (after having cleaned it and removed foreign bodies); use as few stitches as possible; use as little adhesive strap as possible; apply a pledget of cloth soaked in cold water, and bandage loosely; inculcate absolute rest; preserve the part moist and cool, by the assiduous changing of cloths wrung out of cold water, and applied over the bandage; the part must not be allowed to become heated, so that for the first few days the cloths must be changed every two or three minutes, or a minute continuous stream must be directed on the part, by any of the simple processes recommended for the purpose. By the use of the cold water dressings, incised wounds heal immediately, and lacerated wounds detach sloughs, and are repaired by the modelling process without suppuration, at the same time presenting the most excellent cicatrix. In the latter kind of wounds, when poulticing is used, profuse suppuration is established, inflammation being excited by the hot, rancid, oppressive, irritating poultice, much of the previously sound tissues are wasted away, and the resulting cicatrix is rigid, puckered, and contracted.

We read that Hippocrates himself used water dressing most successfully, but that afterwards Celsus introduced a variety of absurd and complicated medicines. In the 14th century, the system of secret dressing was in fashion, each practitioner having a remedy which he considered universally applicable. When at a still later period water dressings were used, they were accompanied with incantations, to which the good effects were attributed. It is stated that Ambrose Paré, a pious but superstitious man, used the same application, but astonished at his extraordinary success, deemed the remedy nothing less than miraculous, and therefore not to be used by mortals, and accordingly he abandoned it.

This mode of treating wounds has received at my hands a very extensive trial, and has been followed with great success. I must, however, confess that I have had no inconsiderable difficulty in overcoming the prejudices of the people against so simple a method; and, in ordinary private practice, it is not unlikely I might have been obliged to discontinue it, or at any rate, substitute the usual mere formal perfumed lotions; but the nature of my appointments in connexion with the extensive manufactories in this district, has enabled me to carry forward the simpler practice, and that too, at last, to the entire satisfaction of the people themselves. Such prejudices are not confined to our locality, and have been, I fear, too much fostered everywhere



by the more mysterious proceedings of the scholastic and orthodox practitioners. If this is so, we need be less astonished at the success of quackery, which is conducted upon similar principles. When we let all our patients see and comprehend that we are treating them upon scientific and simple principles, then may empiricism prepare for its downfall without any interference on the part of government.

I shall conclude with a brief note of a few cases which have occurred recently, in illustration of the foregoing observations.

I. A man received an injury by the machinery in a large paper mill, which laid open the wrist-joint. The hand was half separated from the fore arm, the tendons were torn, and the inferior end of the radius, which is naturally related to the carpus, was exposed. The arm and hand were placed straight upon a pillow, the wound was cleaned, and two stitches taken; a pledget of cloth soaked in cold water was applied, and a bandage rolled, not too tightly, round the hand, wrist, and fore arm; a large basin of cold water placed conveniently by the bedside, and directions left to apply freshly-soaked cloths over the bandage every two or three minutes, to prevent any heat or inflammation ensuing. No inflammation took place; the modelling process was uninterrupted, without suppuration, and an excellent cicatrix formed in little more than a fortnight.

II. A girl had the whole of the soft parts on the palm or surface of the four fingers, as it were, scraped off by the machinery in a flax mill; the tendons were torn, and the phalanges exposed at different places. Each finger was dressed as follows every day: being first bathed in cold water, a piece of soft cloth was placed round the finger, and a narrow roller to keep it applied; when the fingers were all thus dressed, a larger cloth soaked in cold water was wrapped round them together, and changed as frequently as the slightest tendency to become heated appeared. The modelling process advanced steadily without suppuration, and cicatrization was completed in about four weeks. The fingers gradually acquired flexibility.

A great number of similar accidents have occurred among boys and girls employed in the cotton and flax factories in this district during the last six or seven years; and the same simple treatment has been adopted, so that, although obliged occasionally to amputate fingers in part or in whole, cases of very remarkable injury of soft parts and bones have recovered, and members have been saved, which, in all likelihood, would have been sacrificed by a treatment less calculated to prevent inflammation and suppuration. Flabby granulations are seldom seen, unless where the prevention of inflammation is carelessly attended to; so that caustic applications, astringent lotions, and stimulant ointments are not used.

III. A little boy had scrofulous disease of the bones of the ankle-joint, on account of which I amputated, by the flap operation, below the knee. Two stitches were used for two days; a strip or two of plaster and cloths wrung out of cold water were the sole applications. The wound was whole in a week. Other amputations have been similarly treated, with equal success.

IV. A girl received a sharp instrument into the ball of the eye, at the Woodside works. The cornea and sclerotic coat were ruptured, the iris was lacerated, and prolapsus followed. Rest in bed, continued persevering use of cloths wrung out of cold water, and simple laxative medicine, constituted the treatment. The treatment was effectual in preventing inflammation, which was clearly the only indication in the case. The termination was as favorable as could be under such circumstances.

A multitude of cases might be recorded in this place in which the same simple natural treatment was adopted; but these instances suffice to show what Nature can accomplish herself, and the little we have to do to facilitate her operations.



### III. EXTRACTS FROM CORRESPONDENCE.

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[The following pages consist of extracts from a very voluminous and interesting correspondence with which the editor has been favoured since the publication of the article on homœopathy and allopathy, in the January number of this journal. They are made public, in the hope that they may tend to promote the object with which the article was written—viz. the improvement of practical medicine. Emanating, as they almost all do, from men not only of reputation, but of long experience—from men in every rank of the profession, and living in different countries, they will show that the opinions promulgated by the editor are neither novel nor singular, but are in strict accordance with those entertained by the most eminent members of our body. All the merit the writer of the article in question has ever sought to claim for it, is, that it openly avowed what the writer knew to be the sentiments of the wisest and best among his brethren. The correspondence and the other documents now published will, it is hoped, spread more widely the knowledge of the fact—that such are in reality the sentiments of a large portion of the physicians and surgeons of the present day. Such a knowledge cannot fail to give greater confidence to the younger members of the profession, not merely to declare their opinions, but to modify their practice according to the views they may conscientiously entertain. It is mainly to the younger members of the profession that the writer of the article in question looks for the consummation of the reformation in therapeutics, which he is desirous of promoting, and which he believes to be absolutely necessary—and inevitable.]

In his desire to forward the cause in which he has embarked, the editor has entirely disregarded the imputation to which he has probably laid himself open in printing these extracts—that, namely, of publishing his own praises. In sending them to the press he has certainly omitted much that was complimentary: but if he had omitted all, he would obviously have defeated the object he had in view in publishing them,—the corroboration of his own views by the high authority of others. The editor will only further add, that as none of these communications were written with a view to publication, he has not only omitted the writers' names, but left out every indication of their precise locality. He trusts that his excellent friends and correspondents will forgive him for making the profession generally partakers in the great satisfaction and advantage which he himself has derived from their communications.]

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“ ——— (America), January 30th, 1846.

“I thank you much for having written the article. The fulness of time has come in which all this matter should be exposed fully and clearly. You have not said a thing which I have not thought of and agreed to beforehand. I know how presumptuous this may seem; but you know that it is one thing to entertain correct thoughts on a subject, and quite another thing to bring them out clearly and in proper order, so as to claim the assent of all good judges. This last is what you have done most satisfactorily.

“Some of the opinions you have expressed I have entertained for many long years; others I have arrived at more lately. I first longed for a good natural history of diseases, to decide how far remedies had an effect. As connected with this I early learned the primary importance of diagnosis, and this in reference to the different stages of diseases as well as in reference to diseases themselves. It is many years since I was satisfied, in respect to acute diseases, that it was only on the first days (principally the first three days) that medicine

(drugs) could be of much service. At this period I am still satisfied that acute diseases can, ordinarily, be much mitigated and somewhat shortened. But an exception must be made in regard to such as we call malignant (yellow fever, Asiatic cholera); and, as to shortening, in reference to the exanthemata. I have been getting more and more of the opinion, that in most chronic diseases, diet and regimen will often have a great influence,—drugs rarely any very decisive good effect, and often an injurious effect only. I have long deprecated the idea (and this I have done in communication with my intelligent patients) that medicines (drugs) are necessary in the treatment of all diseases. I have urged that it was the business of the physician to take care of the sick, pointing out that *cure* and *care* were the same word originally,—and that in taking care, it was much more important that he should endeavour to control the influence of the common agents, than that of the occasional ones, called medicines;—that he should attend to the non-naturals,—those things which nature does not decide, but leaves to our choice,—that he, the physician, should do it, and not leave it to the nurse or the grandmother. In consonance with these views, I have been unwilling to say that my patients who recovered were cured by me; for I endeavoured to cure all of them, and claimed to have done it, even when they died. In our hospital, opened twenty odd years ago, I would not allow the record books to say that so many patients had been cured, as is a common practice; but that so many were discharged well, so many improved in health, &c. I have often urged upon my brethren that we should never get the better of quackery so long as we attributed the recovery of our patients to medicine, on the *propter hoc* principle,—that is, *propter hoc* because *post hoc*. Our proper ground is, that, having studied the subject and had personal experience, we know better than others how to direct the cure of the sick; and that in doing so we may use drugs, or may not, as the case may require. These drugs may sometimes be directed against the principal disease; but oftener they may be used to counteract or guard against the accidents which would aggravate that disease.

“I have found no difficulty in all this. In some respects I have, perhaps, had advantages which you do not yet enjoy. . . Though we have good apothecaries, by whom alone medicines are ordinarily put up,—they are strictly men of the shop. They never visit the sick, and do not pretend to practice. They make their charges for medicines alone, and are paid as grocers or other tradespeople are. The physicians (and practitioners generally) charge first for their services whether they order medicine or not. Thus, there are none of the inducements to employ drugs which exist among a large class of practitioners with you. In our country towns the physicians supply the drugs; still the influence of city customs prevents their resting their emoluments on these; they charge distinctly and mainly for their visits, &c. Again, among the regular practitioners we have not any distinction of classes; though age, &c. give to some men a certain rank, it is very different from what exists with you. We all of us have therefore a chance to get patients at the onset of disease. . . . .

“I have said that I have found no difficulty in satisfying patients without medicines. I began life with a good confidence in certain articles, and more especially in the alterative powers of mercurials. The doctrine now prevalent with you as to the power of calomel in acute disease, especially in inflammation, was familiar to me, and fully believed, when I attended St. Thomas's, in 1799-1800. I used to say to my young friends there, with the true juvenile conviction of superiority, that it was singular Dr. Saunders (William) could be so blind; in his book, and in his lectures, he advised the use of mercury in acute as well as in chronic hepatitis, yet did not see that it ought to be equally useful in other acute inflammations, as I knew that it was. It was some years later that one of your Dr. Hamiltons (in England, not Scotland) first brought

forward the general doctrine; at least, so far as I know. At the present day, I find it maintained by Dr. Watson in his delightful lectures. Now, I am not ready to say that the practice is all wrong. There is perhaps some good in it in some cases; but when pursued heroically it does a wonderful deal of mischief. Whether it often *causes* a fatal result I will not say; I think not often; but it often adds much to the suffering of the patient and to the duration of his *sickness* if not of his *disease*. I began to perceive this early in my career, but not in its whole extent for more than twenty-five years. In acute rheumatism I gave up the mercurials within six or eight years after I began; though it would seem to be the disease to which it should be most applicable. And as early as this I became very careful not to produce a bad sore mouth in all cases of disease. But I was a pioneer here; and I can boast (for I may seem to be boasting) much more of what has been done by younger men—whose modes of philosophising in medicine I have influenced in some measure—than of my own doings. . . . .

“I must finish by saying, that I am often consulted by people from the country, as well as from the city, in chronic diseases, though I have ceased to be a busy practitioner; and that I find no difficulty in sending away a patient, after an hour’s lecture upon his mode of life and conversation, without any prescription for medicine. In epilepsy and in phthisis, except so far as symptoms may require to be alleviated, I always tell the patient at once not to employ any drug to remove his disease. But in epilepsy, not evidently from organic disease, and in phthisis at an early period, I venture to say that much alleviation and benefit may be derived from care as to diet, exercise abroad, &c. Do not understand that there is a precise formula which I follow in these cases. I try to communicate principles rather than rules, and I find a large proportion of patients can understand me.” . . . . .

(B)

“ ——— (America) March 26, 1846.

“A few weeks since I had the pleasure to receive your remarks on Homœopathy, &c., with an accompanying note, requesting the opinion of those to whom it was communicated, whether harmonizing with, or discordant from it.

“The remarks I read with the deepest interest; they open to the eye a new and beautiful view of medical practice; beautiful, because it harmonizes with natural laws which we are well acquainted with, but greatly neglect.

“For forty years and more I have been called to attempt the relief of suffering by medical and surgical administration; the result of my observation has been that drugs are frequently more injurious than beneficial, particularly where the course of disease is established; the glimpses of truth which have occasionally enlightened me now shine out broadly in your philosophical treatise. I heartily congratulate you on your having the courage to be its author, and sincerely believe it will do more service in the improvement of medical practice than any publication of the present day.”

(C)

“ ——— (America) Feb. 28th, 1846.

“The article has created quite a sensation here, and knowing well that it could be laid hold of by the homœopathists, and garbled as it has been, I was myself anxious that it should be reprinted in full, so that no permanent misrepresentation might exist. The favorable portion of your remarks has already been extracted by them (the homœopathists), yet they have not concealed that you are no homœopathist, and have endeavoured to show that you are not consistent, by contrasting your admissions in regard to the reform

produced by the practice of Hahnemann with your exposition of its absurdities. The whole article accords signally with my own views. In regard to the 'agenda, cogitanda, &c.,' I have scarcely any objection to make. Whilst I lived in ———, I was generally regarded as an 'inert' practitioner, because I did not practice the energetic and heroic treatment universal there; and since then my remedial agencies have been considered to belong to a 'masterly inactivity.' I apprehend that in the progress of life, every one becomes less and less active; is more and more disposed to attend to the 'divinity that stirs within us;' is less and less disposed to believe in the special adaptation of drugs to special morbid conditions; and more and more in the great principles of hygiene and therapeutics. With one single admission only would I hesitate to accord. You ascribe immense influence to Hahnemann as a reformer of regular practice. In this country, his doctrine and course of treatment have had but little effect on the 'regulars.' In the cities they have long become less active; but if any one is entitled to the credit more than another here, it is Broussais. Nowhere, not even in France, were his views so extensively embraced; and under their adoption, the excessive bleedings of the Rush School and the hyper-catharsis in use every where were abandoned, and a more rational and milder system introduced. The good sense of observers of the day has also, I apprehend, had much to do in bringing about this salutary reformation."

(D)

" ——— (Germany) April 15th, 1846.

"I beg that you will allow me to thank you for the article on Homœopathy and Allopathy, contained in the January No. of your Review. It must be hailed with the greatest satisfaction by all the members of our profession who have its welfare truly at heart, and have searched for the solution of the contradictions and riddles presented by all the orthodox works on the different branches of our science, either when studied in books only, or when compared with the statements of unorthodox schools or the results of practice. Your article must necessarily produce not only a great sensation, 'but it will lead to the most positive and the most beneficial results,' since it contains the enunciation of a principle which has before been hinted at by others, and which many, and myself among the rest, have had an 'Ahnung' of, (to use a favorite German term,) but which none have dared to give clear and decided utterance to. The article in question made upon me the impression of an outburst of matured thought and long-suppressed conviction, which has broken forth with all the vehemence of an explosion, but it is not the explosion of gunpowder, but the powerful ejection of the first steam from the boiler, which at once proves the strength and workmanship of the engine, and gives promise of a long and useful career. The works of Dr. Combe, to which I with so many others owe much—I might almost say, the entire direction of my professional creed and practice in the path of hygiene—have prepared the way and rendered the adoption of your views a less difficult matter than it might have been, had they been published ten years earlier; and though I am convinced that you yourself would soften some of the expressions used in the above-named article, and remove stumbling-blocks which may offend tender consciences, the profession are indebted to you for the clearness and precision and courage with which you have pointed out what ought to be our aim, what must be the guiding principle in all our labours, if we are to be useful to humanity and satisfied with ourselves. . . . .

"I have learnt to look upon the prevention of disease—upon hygiene in its most extended meaning—as the true aim of the medical man. I have seen enough both in hospital and private practice to feel disgusted—I may admit as

much to you—at the authorized quackery even of intelligent and highly educated medical men. I have felt the opprobrium severely, which must be the lot of the profession so long as they shut their eyes to the true working of natural laws, and as long as they wilfully refuse to admit inferences, which though necessarily the permanent basis of the “curative” art, clash with received notions and traditionary prejudices. Truly has it been said that the real object of our science is less the healing of disease than the correct guidance of those that are healthy, and the interpretation of those laws by which healing *κατ’ ἐξουσίαν* is rendered unnecessary. What more melancholy fact can be presented to the mere prescriber when he first enters upon the duties of his benevolent profession, with the enthusiasm of unsoured philanthropy, than the continual assurance of the Nestors of the profession that the greater our experience the more positive the conviction that we can do nothing? and it only proves the immense force of habit that, with such convictions, we do not more frequently see men quit a profession which, under such circumstances, requires a constant exercise of hypocrisy and a sacrifice of principle. But Sir, thanks to you, and to men like Combe, Chadwick, Clark, the young generation see the radiancy of a new light, that warms the heart while it illumines the intellect; and though their path still continues beset with dangers, they feel a firm footing, and the slough of despond is passed.

“One more remark on your essay, and I have done. The profession are deeply indebted to you, not only for your advocacy of the physiological school, but also for the truly conservative manner in which you have given vent to your ideas. I do not hesitate to say that there has been intellect enough among the members of the medical profession to have given birth to similar views at an earlier age; but the authors of new theories have invariably felt too much vanity, and consulted their passing fame too much, and have raised party feeling, and the baneful effects of party spirit. What is there in a name? We might almost say everything; and thus it has been that Galenism, Brownism, and all the other isms that have proved an ignis fatuus to many, have at least retarded the due development and progress of medical science, more from the factious opposition or factious adherence to the principles individualized by the name, than by any inherent faults of the systems.

“I beg to apologize again for intruding myself upon your notice, but it is to me almost an act of devotion to express my opinion and my gratitude to one who has conferred a most signal benefit upon the whole profession, and more especially upon its junior members. Whether or not I shall ever have an opportunity to avow my adhesion to ‘the natural system’ publicly, will depend upon a variety of circumstances. If you meet me in the field of literature, I trust that you may not have reason to refuse me recognition as one of your earliest disciples; at all events, I shall endeavour to follow out those principles which I feel convinced afford the only true standard and rule to guide our actions by. They are the moving power in my private practice, and I am guided essentially by them in my labours in the hospital.”

(E)

“———— (Germany) May 1st, 1846.

“One of the first points that the junior members of the profession will anxiously wish to receive advice upon in reference to the Natural History School, I conceive to be, ‘how they are to observe,’ in how far they are to continue to act according to the hereditary doctrines, and in how far they dare trust their own discretion in treading in the footsteps of Nature. It is no trifling matter for a beginner to be cast upon the sea of doubt without a sure beacon to steer his skiff by, and though you may assert, and perhaps rightly, that the public will not suffer, still some regard ought to be had for the mental



distress of those whose energy is not sufficient to carry them through the dangers that beset all innovators, and still are possessed of sufficient 'ingenium' to feel that they are not in the right path. There is a great additional difficulty in England, from the unfortunate and disgraceful fusion of the apothecary and the medical practitioner; for let the 'pures' boast as much as they list of their influence and power, they do not, nor can they ever gain that general influence that is enjoyed by the surgeon-apothecary. The mere apothecary, (in the continental sense of the word,) cannot possibly interfere with the march of intellect in the medical profession, but so long as the medical man is expected by the bulk of the population to give his quid pro quo in a more tangible shape than that of a prescription or advice, he must prefer poisoning others to starving himself. The universal complaint of the apothecaries in Germany is, that much less medicine is sold now than formerly, and it was only the other day that a lady made the characteristic observation to me that, whereas, in former times, the apothecary was in the habit of sending her family presents of lozenges in elegant boxes at Christmas, he now scarcely had a bill to send."

(F) ●

"———— (Germany) March 8, 1846.

[*Translation.*]

"Being compelled to write in haste, I avail myself of the German preferably to my bad English . . . . The good fortune I have had, ever since the commencement of my medical studies, of residing in large hospitals, necessarily familiarized me early with the natural history of disease, a thing somewhat different, it is true, from what we read in many, and not all uncelebrated works. I have arrived at the conviction that in inflammation and fever our drugs prove rather mischievous than useful; and that Nature has then to overcome both the disease and the evil effect of the said drugs. I need scarcely tell an experienced physician that, under this term 'drugs,' I do not comprehend simply-mucilaginous, gently-resolvent, mildly-aromatic, or very slightly-astringent decoctions or infusions. My objection is to the frequent employment of emetics, purgatives, drastic resolvents, mercurials; of cuprous, cinchonous, æthereo-resinous preparations. It has been, is still perhaps, imagined, that with such remedies, inflammation and fever are to be 'cut short,' 'advanced to a crisis,' 'to resolution,' &c. Calm observation at the bedside, an unbiassed review of circumstances, long practical study of pathological processes at the dissecting table, demonstrate the untenable nature of all these fancies which have, alas, been handed down from generation to generation. Let not Nature be thwarted; above all, let external influences be properly regulated, the instincts of the patient judiciously ministered to. Under this kind of treatment diseases are assuredly less complex in their course, and more fortunate in their termination; whilst the patients themselves are spared the distress inseparable from the use of substances for the most part so little german to the organism. With all this, however, I do not intend to reject all drugs; in their application I am guided by a regard to physiological processes and conditions, and in the very few instances in which I know of specific remedies, I gladly avail myself of them; e. g. quinine, strychnia, belladonna, hyoscyamus, digitalis, — opium and morphia. — iodine, — mercury, — ipecacuanha? — tartarized antimony?? — &c. &c. ???

"With respect to external remedies—in fever and inflammation, where we have to combat local evils, I allow none but physical indications to be my guide. In chronic disease, the dietetic treatment, in its most comprehensive sense, should unquestionably take the first place, although the empirical use



of certain remedies must be retained, until a more intimate knowledge of the composition of the blood, of the nutritive process, of the metamorphosis of matter, of neuro-physiology, either warrants the older methods, or holds out new.

"The employment of the lancet I have almost entirely abandoned, ordering only, say, amongst 1500 patients, three or four venesections. The inference here may, perhaps, be simply this,—that in our time, for our nation, for my particular patients, phlebotomy had not been required; I can only say that I have cured all kinds of inflammation as well, nay better, without its aid; and I am entitled to make this declaration, and to appeal to experience, inasmuch as I see, annually, in my public practice alone, from 1400 to 2000 patients, besides a considerable number in my private practice; whilst during a period of fifteen years, I have had the opportunity of comparing the various methods of treatment practised in the different provinces of our Empire. Leeches I hardly ever employ, except—and that mostly to gratify the patient—for purposes of mere local depletion. Cupping I order sometimes, but upon the whole, very rarely. My rate of mortality will bear a comparison with that of my—perhaps more active—colleagues, without discredit; nor do my patients remain longer under treatment; in acute cases, in particular, I certainly do not lose more than the others. If I choose, I might perhaps be able to adduce proofs of the superiority of the simple treatment, but this I should deem inappropriate. Enough if it show no inferiority. . . .

It is easy for an hospital physician to register splendid diagnoses, to describe severe cases, to record a series of violent symptoms; but for reasons easily intelligible, there is seldom in such cases a rigorous certainty as to the diagnosis. In like manner, the fixing of the cure or amelioration of the patient rests upon the personal judgment of the physician, and the books of the hospital written by himself; and he, doubtless, often loves his patient and himself sufficiently well to dismiss the case as cured, the moment the most striking and distressing symptoms are removed; but merely to receive the patient back again after a brief interval. . . .

"What do I think of *Homœopathy*? Ever since I began to *practise*, I have regarded *allopathy*, *homœopathy*, &c., as historical facts. From both I have derived instruction; from the former perhaps most. I believe that a good physician, in earnest both with humanity and his profession, cannot swear allegiance to any one standard only. The pharmacology, and more especially the pharmacodynamics of *homœopathy*, have given the impulse for a thorough reconsidering and weeding of the *Materia Medica*. Without entering upon the absurd mystifications of the ultras, both deceivers and deceived, I affirm that we have not a little to thank *homœopathy* for, more especially in relation to the so-called specifics, and also as to the lessening of our doses. . . .

"The foregoing will show you the sort of value which I attach to the reports of hospital physicians, who contend exclusively for this, or any other system; *unconditionally, they are absolutely valueless.*

"The world—the crowd—the ignorant, (and how many physicians, some of high renown, might count among the number?) never can and never will judge correctly; nor will they ever be taught by us. We must leave the finishing of the battle—the triumph, and the union, if achievable—to time. I, for my own part, have resolved to let my creed be corroborated, more by deed than word; by actual experience, and not by written dogma."

(G)

“ ————— April 29th, 1846.

"Lord Bacon says that 'It is not St. Augustine's, nor St. Ambrose's works, that will make so wise a divine as ecclesiastical history thoroughly read and

observed;’ and it was wisely said—for there he will have a history of the effects of ignorance and weakness, as well as of the art and ambition of human nature; and if a review of them does not make him a more charitable and wiser man, he must have a weak and highly prejudiced mind, or has it contracted and blinded by interested motives. An equally important lesson may be learned from the history of medicine; for there the physician will see the folly, credulity, and impositions which have been connected with the healing art; how the opinions and practice of its professors have fluctuated; and the conflicting testimonies of men all equally honest and zealous to promote the advance of medicine, and all equally positive as regards the effects of opposite modes of treatment in diseases. One would naturally conclude that the review of such a history would excite in the mind of a reflecting man a spirit of caution and modest doubt, in all cases where he had not the guide of uncontroverted facts and sound experience to justify confidence, decision, and assurance. There is nothing in the history of medicine, either in ancient or modern times, calculated to excite more surprise than the absurd statements connected with homœopathy and the astonishing credulity of the advocates of it. But if, as you conclude, and I believe justly, there are really honest men who sincerely believe in the whole of homœopathy, what a lesson should it teach us of humility and charity, that honest minds in the search of truth should embrace such follies!

“The practical importance of the statements made by the homœopathists is, that they furnish some aid in solving the interesting questions—how far the use of drugs and other active agents have tended to promote the cure of diseases? and how far, and in what respects, the present practice of medicine is preferable to that of Hippocrates, who chiefly relied on regimen and what is called the expectant treatment? Every medical practitioner, who is a friend to truth, and who wishes that medicine should be based upon rational principles, will court for its practice the strictest investigation and scrutiny, and rejoice to see it cleared of every thing which is injurious or useless, that ignorance, prejudice, or credulity have introduced into it. It will be only persons to whom the words of Bacon are applicable who will fear the light of truth: ‘Doth any man doubt that if there were taken out of men’s minds vain opinions, flattering hopes, false valuations, imaginations as one would, and the like, but it would leave the minds of a number of men poor shrunken things, and unpleasing to themselves.’

“Nothing can be more injurious to the true interest of medicine than holding out false hopes in attributing too much to its powers. The power we possess in what are termed surgical cases is very different from that which we have over internal diseases; the changes which take place in our internal organs in disease are often of a mysterious nature, and out of our power to discover or control; and it is happily ordered that, in most cases, much more depends on the powers of Nature than the knowledge and skill of man, or else, woe unto the poor and helpless, and those who depend upon the judgment of the unskilful practitioner. But when the Healing Art has been stripped of all its unjust pretensions, much will be left to prove its importance. No one will deny the great improvements which, in modern times, have taken place in the treatment of cases which belong to surgery, and the immense benefit which surgery has conferred on mankind. It is equally certain that there have been great additions to our knowledge in regard to the remote and exciting causes, as well as the nature and treatment of internal diseases. If medical treatment only saved one life in a hundred, medicine would be a science of vital importance; and the man who does this surely should be as much valued as the pilot who, in conducting ships through a dangerous passage, preserved one in a hundred from destruction.

“There is no doubt that many practitioners of medicine have presumed too much on its efficacy, and, by an officious interference with Nature, instead of

affording relief and benefit, have often been the cause of torment and injury, and thereby lowered the just estimation of the profession of medicine. There is as much truth as wit in Molière's description of the orthodox physician in his time: 'C'est un homme tout médecin depuis la tête jusqu'aux pieds; un homme qui croit à ses règles plus qu'à les démonstrations des mathématiques, et qui croirait du crime à les vouloir examiner; qui ne voit rien d'obscur dans la médecine, rien de douteux, rien de difficile; et qui, avec une impétuosité de prévention, une roideur de confiance, une brutalité de sens commun et de raison, donne au travers des purgations et des saignées, et ne balance aucune chose.' It will be allowed that the character of physicians has improved since the days of Molière, but if a little more 'modest doubt,' which has been called the beacon of the wise, had influenced their opinions and practice, the stability and dignity of medicine had not been left so much at the mercy of homœopaths and other theorists.

"One great cause of the present imperfect state of medicine is the ignorance of the public as regards the important principles which ought to guide the medical practitioner; they are pleased with what appeals to their senses rather than to their reason. The *opus operosum* of medicine has, in every age, been more captivating to all classes, than the recommendation of simple means which reason and experience may dictate as best adapted to their case. When Elisha told Naaman the leper to wash in the Jordan for the cure of his disease, he was wroth at the simplicity of the remedy, and went away and said, 'Behold! I thought he would surely come out to me and stand and call upon the name of the Lord his God, and strike his hand over the place and recover the leper;' and such unfortunately is the state at present of the public mind, that, in the majority of cases, a medical man cannot do justice to himself or give satisfaction to his patients, if he does not practise a little of the 'Art and Mystery' of Medicine. . . .

"Much has been said and written about medical reform, but in my opinion none is so desirable or would contribute so much to the welfare of the medical profession, as an elevation of the mental character of its members: this, I think, would be the best, if not the only remedy for the present defects and follies connected with the practice of medicine. There should be required from all those who enter the medical profession, a knowledge of authors whose works tend to develop the reasoning powers—such as those of Bacon, Locke, Paley, &c., as well as a knowledge of the history of medicine, and of subjects immediately connected with the preservation of health—public and private hygiene, in addition to those on which candidates are now accustomed to be examined. The first examination, (that qualifying for practice,) might be of such a kind as any one of average talents, with common industry, may undergo with facility. Every one might follow that branch of practice which his talents, his taste, or circumstances may dispose him to cultivate; but surely no one will deny that a knowledge of surgical subjects is highly desirable, if not necessary, to the 'pure physician.' Professor Alison, of Edinburgh, says, that many physicians of high eminence, both those employed in consulting practice as well as those attached to hospitals, have few opportunities of witnessing the effects of bleeding in the early stages of violent inflammatory diseases (*Pract. of Med.* p. 221), and this observation is equally applicable to other important diseases. When a medical practitioner has had opportunities to acquire experience, and seen what may be called *the active service of medicine*, he would in the course of years, say ten, have some claim to the title of doctor, which he should acquire by undergoing a more extensive examination on all those subjects he had been previously required to have some knowledge of as a licentiate; and he might take his degree either as doctor of medicine, or of surgery, or both. If a competent knowledge of philosophical subjects, including mental and moral philosophy, was required of every one who took rank higher than a

licentiate in medicine and surgery, it would give a motive for and a stimulus to the study of subjects the knowledge of which necessarily tends to elevate the character and accomplish what Hippocrates says ought to be done, 'to bring medicine to wisdom and wisdom to medicine, so that the physician should be a divine philosopher.' (De Decent. Habit.) Such a reform in my opinion would remove what are now thought to be unjust privileges and invidious distinctions, and tend to place the practice of medicine on the basis of sound experience and philosophy."

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(H)

"——— Hospital, 3d March, 1846.

"The general error into which English practitioners are falling is the empirical use of powerful remedies, with a strong disinclination to be quiet, even when the diagnosis is obscure. The general scope of your paper I take to be to combat this mistake, and I hope that it will be the means of bringing about an improvement in our practice. The table (Fleischmann's) you have given is curious, and could it be implicitly relied on, would prove to me at least that we had better, in some diseases, give up prescribing altogether. Thus we have 188 cases of rheumatism, all of which are cured; and even of articular inflammations, 203 are cured out of 211. Now we have no success at all approaching to this at our hospital. I treat about 100 cases of these diseases annually; but I should be ashamed to place the results in a tabular form by the side of the homœopathist's table.

"In regard to pneumonia, used as a general term, the results of our hospital practice would differ little from the table, or even appear more favorable. I studied under a physician of the old school, who was fully impressed with the virtues of Seneka in cases of inflammation of the respiratory organs, and seldom prescribed venesection; yet he was as successful as his neighbours. But I have so often witnessed the sudden extinction, as it were, of incipient pleuritis, or pneumonitis, by a timely bleeding, that I should consider myself culpable in neglecting such a remedy. The fatal cases of pneumonitis are those in which the disease has been silently going on for a week or more, without pain or much uneasiness, and the patient has been living in his accustomed manner, and perhaps taking more strong drink than usual to relieve the languor which he feels. We receive five or six such cases annually, mostly men who are living with their families, and are not seen daily by a medical officer. They are sent to us either as cases of typhus, or of pneumonia, with typhoid fever; but, in fact, they are cases of hepatized lung, proceeding to the gray or suppurating stage, and the great debility, which generally comes on suddenly, is caused by the want of oxygenization of the blood. They do not bear bleeding at the arm at all; and the blood drawn has a dark venous colour. This form of the disease is, I believe, more frequent with spirit drinkers.

"Again, of dysentery, all are cured in the homœopathic table, except two that died. Now, I have very many cases of dysentery from the coasts of Syria and China, and experience leads me to believe that permanent recovery is an exception to the general rule in cases sufficiently severe to require hospital treatment, after two or three months' duration. The cases discharged from the hospital are merely relieved, for the complaint recurs again and again, or alternates with irregular remittent; yet, judging from the result of dissections, there is no ulceration in the intestinal canal in the majority of fatal cases.

"Pericarditis, too, is set down as cured, in the only two cases mentioned in the table. It is a frequent disease with us, as might be expected, since

rheumatism is our most common acute disease. Now, unless it can be speedily cut short, we find that the functions of the heart are permanently injured, and life shortened. A considerable number of men are invalided annually for this cause, and many of them return to the hospital in a year or two with the usual dropsical symptoms that attend dilatation of the ventricles of the heart in its advanced stage. In regard to continued fever, perhaps the homœopathic treatment is equal to any. When the skin is kept cool, thirst allayed, the bowels cleared out, and air freely admitted, the *vis medicatrix* must do the rest. . . . .

"I trust that your paper will have a beneficial effect, by causing medical men to weigh the facts well before they come to decision as to the effects of the medicines prescribed. I firmly believe that the cure is much oftener retarded by the medicines administered than it would be safe to say in these times of advanced medical knowledge, and the mischief would be still greater were it not that much of the stuff sent to the patient is not swallowed by him. Could the public mind be so far influenced that a post-mortem inspection of the body took place in every case of death, by experienced anatomists, the foundation of rational medicine would be laid. The opportunities we have of testing our diagnosis in hospitals, though not so good as they might be made under better arrangements, are of much service to the individual practitioners in charge, and might be made of more general use by a system of reporting."

(I)

" ——— February 18th, 1846.

"During above thirty years of extensive practice, consequent upon eight or nine years of studentship, I have really attended as unremittingly as most men to my profession; and am arrived at the period when I feel no hesitation in stating, that the best part of medical science consists in observing symptoms, so as to form a correct diagnosis, and in ascertaining the *restorative powers* of the system, so as to aid without interrupting *them*. I have long taken leave of the worrying excessive method of treatment, by which the juvenile practitioner expects to knock down every disease, as systematically and surely as the soldier the enemy's stronghold in a bombardment. I suppose most men, as they go on in years and experience, leave off the over-active dosings and depletions in all ordinary medical cases, and find themselves proportionately rewarded with success. I feel convinced that, in my own circle, I have witnessed patients evading successfully from a protracted and dangerous disease, owing to forbearance in the treatment of its early stages, who would, under an opposite plan, have shown no powers of recovery, or have sunk under diseases induced by the treatment.

"In proportion as a physician respects the natural *vis medicatrix*, does he attend to diet, exercise, fresh air, cheerful impressions, &c., which are admitted to be beneficial under all the theories and vagaries of practice. I have been so impressed with the effect of the mind upon a great proportion of the ordinary ailments claiming our attention, as to be led to remark, that the practitioner of great renown often gives a fillip to recovery by the confidence he inspires, and which the familiar and daily medical attendant has no chance of inspiring. You will readily conceive, my dear sir, that imbued with these sentiments, I can readily appreciate your explanation of the only merit and efficacy of homœopathy—the most nonsensical and wild doctrine that ever took possession of the human mind, not excepting the darkest period of our history—and when the practice is successful, this can only be attributed to the natural *vis medicatrix* having fair play in the absence of treatment by medicines, or to the favorable influence of the mind under the confidence



inspired in the patient, or to both these, aided however by rules of diet, exercise, &c., which the homœopathists, I believe, pay sufficient attention to.

"I trust that your prediction, as to the homœopathic practice causing the restorative powers of the living system to be better understood and more respected, will be realized, and shall be glad to find that your pen continues to be exercised towards the fulfilment of so desirable an object. I am sure we want something to help us, and to right us, in this respect—for excessive bleedings, continual raking of the bowels by purges, and indiscriminate mercurialization of the system, cause more diseases than they cure. Injurious as these measures prove, unless temperately and very carefully applied, they are nevertheless regarded by those fresh from the schools as the great and chief resources against the most frequent diseases. I involve my own past errors and early notions in making this statement; and am free to confess that what I now know, in regard to the proper and successful treatment of many chronic diseases, and particularly such as require a tonic plan of diet or medicine, I have found out by experience, and was ignorant of them at first starting in practice."

(K)

“————— (Scotland) 18th Jan., 1846.

"I have not ventured to bore you with my opinion of your 'Young Physic' article, because abundance of more valuable opinions would be poured in upon you for some weeks. Now, when probably the inundation of letters is diminished, or diminishing, allow me to say that I think it a right bold, and right good essay. On this side of the Tweed we all belong much more to the school of 'Young Physic' than our Southern neighbours, with all their gigantic drug-loving, drug-giving, and drug-swallowing propensities. Still, however, the article will be of great use even in old Scotland. Some here are in great wrath at it. Never mind. *Fiat justitia, &c.*

"Pardon me if I say I think you would have made the article as useful as it will be, and less offensive to the 'some,' if you had done two things—first, not *contrasted* Homœopathy and Allopathy, but merely adduced Homœopathic results to show (as they do) medicines of no use in many recoveries; and, second, I think you should have brought out more forcibly the fact that we have some **POSITIVE** agents in allopathy—that opium does and can act as an anodyne, antimony can sweat, ipecacuanha vomit, jalapp purge, &c., &c. What we want is more precision as to when and where we should purge, vomit, sweat, &c., or where we should *not*."

(L)

“————— Feb. 7. 1846.

"... I do not imagine that my opinion will be of any consequence to a veteran writer like you, but I hope I may be excused for indulging myself in the pleasure I feel in expressing to you the great satisfaction with which I have read the article in question. It is long since I read anything embracing so many opinions on different subjects, to which I could give so hearty and unreserved an assent. It is a bold, a well-timed, and a classical exposition of most momentous truths; and, if it be appreciated, it may lead to most important results. I wish the spirit which dictated that article could be deeply impressed on the mind of every medical man in existence. Many of the opinions you have expressed I have often inculcated in my daily public duties; and I have so earnestly longed to see them more widely disseminated that, if my pen had been as ready as yours appears to be, or my engagements less pressing, I might have robbed you of the honour you have nobly achieved in thus giving them to the world.



"I expect the article will be extensively misunderstood, and your first return for it may not consist of unmingled praise; but you have raised a standard about which all right minds must sooner or later rally.

"Apart from lessening the mortality of diseases, we allopathists *may* do much good by shortening their *duration*. What you say about the cure of pneumonia (one of the most fatal internal inflammations) by the natural powers, I have often verified. I can also assent to what you say about the want of evidence of the powers of mercury in this class of inflammations."

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(M)

"——— (Scotland) 18th Jan., 1846.

"Your article in your last Number has stirred us like a trumpet. Many are terrified and enraged, as well as awakened, and, like 'Demetrius and the craftsmen' of old, are 'crying some one thing and some another;' for they are 'confused' as well as confounded. Our friends (the minims) are quite jocose and lively at being handsomely killed, decently buried, and having a sort of funeral service read over them. They are preparing, however, Phoenix-like, to rise from their own remains. You have done a great service to the profession and to mankind by giving them (in their present form) their quietus. But as to create is better than to destroy, you must go on to the *instauratio magna*, and give us, or help us to the giving of, a new *Organon*, in which Bacon's (because Nature's) rule will be followed, and the *end* of the healing art will be stated to be, not the knowledge of diseases, but the cure of them, by *knowledge* (of a certain kind, of course); but the knowledge as a means; the cure as the *end*, and the only end, though not the only result.

"Some of our better men, and who have been *lying awake, waiting to be called*, think your notice rash, and rough, and exaggerated. I say to them, a trumpet sounds an alarm, gives a challenge, calls attention, and signifies a position. It must be loud, it must be startling; it can hardly fail to be a little too loud, or a little too rough-voiced. I for one acknowledge it as a true and certain sound, and am ready to join the ranks; and my object in using the liberty I now do with you is first, to thank you most sincerely as a man and as a healer, for what you have done to *me* in this matter; and, secondly, to offer my services to you as a fellow-soldier and a free man, and one who serves the same queen (the Vis. Med. Naturæ). . . . .

"We are all against the young giant being called 'Young Physic.' He must not be *nicknamed*, and by his own father too! We have got the *thing*—the *name* will come in due time.

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(N)

"May 21st, 1846.

"With regard to the soundness and the importance of the step you have taken, and been the first to take publicly in this our 'reformation,' I have even a stronger opinion than when I first wrote you, and feel personally grateful to you for the good I have already got from its effects upon my mind.

"I am delighted with your second explanation, and with the wise and pregnant letters of Dr. Combe; but I have a special affection for the original blast. Its true value will be better known, and more fully acknowledged, when you and I have been a hundred years in our graves. . . . .

"I wish some one would do justice to Sydenham, and show how truly he was a 'minister and interpreter' of the Vis Medicatrix. I have long thought that something valuable, and at the same time entertaining, might be made of the three greatest practical healers we know, Hippocrates, Sydenham, and

John Hunter—men who combined the speculative with the practical, the deductive with the inductive, in rare quality and measure. Their lives and their doctrines and practice might be made to illustrate three great epochs of our art, and there might be a prognosis attempted of what is likely to be the fourth epoch, and what sort of man the fourth, should and is likely to be. But this subject, to be well handled, must be a work of time, and much labour and judgment." . . . . .

(O)

" ——— Jan. 11, 1846.

"....The conclusions you have arrived at meet with my cordial concurrence. For several years past I have been very much of an expectant practitioner, and I have no reason to be dissatisfied with the result, so far as my patients are concerned; but in private practice it is no easy matter to follow always your own judgment—so wedded are the *laity*, generally, to an opposite and *telling* system. I was educated by Dr. ——— in the strong-remedy method; but long previously to his death he was aware of the total change that my experience had effected upon me, and he did not express himself as if he believed I had gone wrong. When my views were a little strengthened by a still longer experience, I relinquished the plan of keeping medicines and preparing my own prescriptions; and I cannot but recommend my friends to do the same—so much comfort is there in this, and it removes the charge of tradesmen from us, and undoubtedly has a tendency to add to our respectability in the eyes of the public."

(P)

" ——— (Ireland) 13th Jan., 1846.

"I have carefully read your 'Young Physic' with much interest, and have taken every opportunity of eliciting the opinions of my brethren respecting it. Many quite coincide with your views; many deprecate, not—so far as I could make out—your *views*, but your *article*; their disapprobation summing itself up in the complaint of the Ephesian of old, 'Sirs, our craft is in danger,' and bitterly complaining of the mischief and danger that might accrue should the *public* become indoctrinated with such principles. For my own part, I think all your positions are in the main correct; I only doubt that you have extended some of them too far. I verily believe, and have long believed, and have long taught, that the greater portion of our so-called curative methods are, to say the least, of doubtful efficacy; and, in my mind, the instances in which giving medicine and recovery from disease are satisfactorily connected as cause and effect are few compared with the mass of cases of disease, or rather of diseases, treated. But still, I think that in many and important instances such connexion does exist, and is known. The only question is to determine to what extent this is the case, and how such determination is to be effected. It may be that the only way is to make a *tabula rasa*, and begin the study of disease afresh—first, by ascertaining from observation the natural history of disease, and then determining, by experiment, how far we can modify disease by interference. This may be the case; but I am strongly inclined to believe that by consulting the records of experience we shall find the practical question decided, or at least we shall find the elements for deciding it, in some cases at least, or at all events, get a starting point, whence we may go on without commencing absolutely *de novo*. Thus, for example, take any acute disease which has been treated according to three, or four, or more methods, each of which has yielded the same, or *quam proxime* the same results. Let us, then, examine what these several methods of treatment possess in common with each other, and, ex-

cluding everything but what is common to them, treat the disease in a certain number of cases by this common term. If the result is the same, the problem is, I think, solved for this particular case. If it is better, the problem is solved in a still more satisfactory way. If it is worse than the different modes, its treatment must contain in the parts of each omitted, some one common perturbing influence—common, that is, in its effects; and it would remain to inquire what that perturbing influence was, and to apply it in its simplest form. As to chronic diseases, I almost despair of anything very definite being ever known respecting the true value of curative proceedings in most of them, because of the great length of time over which the experiment would extend in very many of them. . . . .

"I think you must 'go on.' If you, or some one else, does not go on in this direction, medicine is, I think, in danger of being utterly prostrated as a science and as a profession, and must inevitably descend lower and lower. It is now much, and promises to become still more, in the hands of the drugging and drenching branch of the profession; but the next slide downwards has already not only commenced, but has made considerable progress—the slide, namely, of the practice of medicine into the hands of professed nostrum vendors, on the one hand, and mesmerists, hydropathists, homœopathists, &c., on the other. If the regulars do not in time adopt the 'common term,' so very clearly indicated by the results of the practice of the latter-mentioned race, and adopt, in the matter of 'drugs,' *ne quid nimis* for their motto, *c'est fait de nous*."

(Q)

"Jan. 16, 1846.

" . . . . I think you have expressed accurately the sentiments which prevail amongst the older practitioners, at least, you have expressed mine. It appears to me that under the new order of things which must evidently become established, the homœopathist will have a considerable advantage over the allopathist; his positive faith in his own remedies, if he has it, will enable him to sustain the confidence of his patient throughout a long and trying illness, whilst the negative faith of the allopathist will make it difficult to inspire his patient with such confidence as shall render him willing to trust to nature, and able to resist his own prejudices and those of his friends in favour of more active treatment. How is this to be remedied?"

(R)

" ——— Hospital, Feb. 15, 1846.

"I have read your 'Young Physic' and detected your hand at once. I have gathered the opinions of most of the profession here upon it. Their amount of approval has been in about an exact ratio with the degree of their knowledge and ability, as far as I am able to judge of the latter. There are some who think that you undervalue the powers of medical treatment, and that they have saved numbers of lives by various plans which others consider injurious. But all the best informed men here are delighted with your exposition of doctrines to which their own reflections had for years been leading them. As to myself I regard the article as just what I should have wished to write. I was for the first three years of professional life with Mr. ———, who had an immense number of patients, and, with my principal, placed most implicit confidence in the powers and virtues of the medicines we ordered. This was very pleasant if the patients got well, but I can answer for most painful feelings and anxiety if they did not; often attributing to

our want of knowledge in treatment what I now know to be inevitable doom. I then was assistant for a year and a half with a thoroughly educated and clever man, Mr. ———, of ———, who first opened my eyes to the natural course of disease and the trivial effects of remedies generally used. Then I went to Dublin, and returned a little to former views in polypharmacy. Then I came to London, and under the late Dr. ———, of St. Thomas's Hospital, began to watch disease in, I hope, a more philosophical spirit, and have ever since been advancing towards the doctrines of 'Young Physic.' I need not say more than that I am sure your paper will be appreciated by the better class of the profession, and by most thinking men out of it."

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(S)

"———— February 5, 1846.

"I have read your article on homœopathy with unmixed pleasure. It has created no small astonishment among the polypharmacists of the provinces, but all well-educated physicians will I am sure agree with you. You hit the homœopaths hard, and the more so because your eyes are so evidently open to the miserable drugging system, which ruins our practice on this side of the border. Of all countries, England is that which requires such a reaction as homœopathy, to bring the drug-doctors to their senses, and I rejoice to find you fully alive to the negative benefits thus bestowed by homœopathy, while you utterly destroy by your arguments that most absurd and baseless theory."

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(T)

"———— January 23d, 1846.

"With regard now to your proposed reformation of physic practical, in contradistinction to physic political, I hardly know how to begin. The subject is so extensive, and my own ideas so crude and undigested, that I shall now attempt to say little more than that I feel there is abundant room for reform in allopathy, and that you have certainly strongly and strikingly pointed out its most glaring defects, and by thus affording food for reflection and consideration, have given a clue for the elaboration of some clear and definite ameliorations. In doing this I think you have been too sweeping in your general condemnation of practitioners. I cannot think that the extreme practice which you so strongly and justly condemn, is in this day adopted by well-informed and sensible men, and it is hardly fair to include them in the same category with the illiterate and unthinking mob. Still I am not sorry to find you dealing an unsparing measure, because nothing rouses a thoughtful man to self-examination and reflection more than a home charge, even though in great measure unfounded as regards him individually. After all, to the question *cui bono*? What should we be at? I am at a great loss to say in what direction the reform is to begin, for this simple reason, that though we have no right grounds for rejecting *in toto* the accumulated observations of past times, still the very foundation of our fabric requires investigation and thorough repair; and where are the minds to be found to do this? Where is the simple love of truth to be found which shall rightly and justly discriminate between fact and fiction—between truth and error—between prejudice and observation? Find your man or men who shall be both capable and willing to undertake such an investigation, when so many and powerful temptations are against them. Then comes the almost more difficult question—where is to be the field of their observation? Private practice never could afford it; and where is the hospital which would be sufficient as the theatre of experiments, the utility and real benefit of which, to the patients themselves and to the public, would

be too remote and uncertain to be appreciated by the latter, or allowed by the former? I despair entirely of any general plan of co-operation, where the labourers are so generally, almost universally, unfitted for such a species of investigation. Those who can do much are fettered. Those who are willing for the race have no legs for it.....

(U)

"———— Feb. 2, 1846.

"On the subject of the present state of therapeutical knowledge and practice, the great point seems to me to inculcate the necessity of giving up empirical prescribing, and as far as possible prescribing and treating upon the general broad principles which have been laid down and ascertained as truth. We want men who can take a comprehensive view of diseases and their management, who will make diagnosis a more prominent feature in the training of their minds for practice, and who, when for want of an accurate chart they are obliged to sail without rule, will deduce rules of their own from their previous knowledge of general principles. It appears to me that really correct practice can be found only with men who have enough scope of mind thus in some degree to generalize. Now here, to my mind, lies the obstacle, and as far as I see the insuperable obstacle, to any extensive therapeutical reform. Medicine is essentially a science of observation, and each case demands all the powers of observation that the individual practitioner can bring to it. You cannot reduce the practice of it to a mathematical formula—in a word, you can never make medicine one of the exact sciences. Neither is it to be desired that it should, since its very standing as a science depends upon its complex and uncertain nature. Divest it of this, and reduce the practice of it to the mathematical precision and certainty which the homœopathic humbug pretends to, and it at once becomes degraded to a mere art, a trade, an occupation unworthy to be the object of the higher powers of the mind. I, therefore, not only seem to see the impossibility of a reform in practice to the extent which you appear to contemplate, but I cannot desire it. What then? Are we past amendment? Have we arrived at that state of therapeutical knowledge at which we ought to rest content? Very far from it. I should like to have all our present knowledge accurately retested by competent persons; but I greatly doubt if this is possible. Could this be done, a much greater degree of certainty would attach to our practice. But the really and, to my mind, only feasible plan of reform and improvement will consist in elevating the standard of our professional character, morally and intellectually, and teaching men the value of general principles as a guide through what must, in every individual case, be more or less an experimental procedure."

(V)

"———— June, 1846.

"..... In common, I believe, with every one who has at all studied and tested practically the conservative and reparative powers of Nature, the effects of medicines, and the often incalculably vague and doubtful grounds on which the presumed virtues of many or most drugs rest,—the announcements in your article did not entirely take me by surprise..... You have, I conceive, achieved a professional, scientific, and philanthropic duty, of which you may be justly proud; and you must not consider that I say so by way of compliment, but simply to mark my sense of the importance of your labours. In accomplishing any great object or end, such as that which you proposed to yourself, there are two modes, the less creditable of which, alas, is the one too

usually adopted. This consists in a compromise, or attempted compromise, of truth with error, of reform with abuse, an introduction of some useful emendations, a retention of many existing imperfections, a fear to take one's stand on broad and ultimate ground. The other mode—that alone worthy of great minds and honest natures, the mode by which the animosities, confusions, sufferings, which unavoidably accompany reforms, are made the least, because the soonest over,—is to occupy at once and from the first, ground coextensive with truth and fact, and nothing short of this. This you have well done in your article. You certainly have not overstepped the due limit. That the projected reformation is directly calculated to affect the interests of mere apothecaries, and also of all quacks in and out of the profession—nay, that it is calculated to curtail the numbers and limit the services of qualified medical men,—cannot be concealed or denied. But whoever would cavil at your article, at your statements, and at the reform you suggest and advocate, on the ground that the adoption of the proposed plans would affect the pecuniary interests, present or future, of himself, his profession, his trade, is, to say the very least, a man whose opinion or opposition may be easily disregarded.

“Will you allow me to state to you my opinion of Dr. Henderson's printed answer to your article? I believe I express the general impression of the profession, when I characterize it as most unsatisfactory and sophistical. I should, moreover, think that homœopaths must be equally dissatisfied with it. Dr. Henderson's homœopathy is certainly not Hahnemann's. His lower dilutions are very little weaker than those now employed by many of *our* way of thinking and prescribing. He adds nothing whatever to the evidence already adduced,—or, perhaps, I should rather say, to the evidence still wanted—to establish the homœopathic principle. The self-complacency—I shall not say, the dogmatism—of his style, in reasoning against a system he so recently held, is somewhat amusing, and confirms the long-made remark that there is no zealot so unmitigated as the proselyte or apostate.”

(W)

“ ——— January 12, 1846.

“The opinions you have offered respecting both homœopathy and allopathy I have long entertained. With regard to homœopathy, I have, publicly and privately, constantly advocated a similar mode of explaining its apparently successful results. My confidence in the allopathic cause, by drugs, was greatly shaken by Louis's ‘*Recherches*.’ . . . . In speaking or writing respecting the curative or therapeutical powers of drugs, I have a thousand times felt the want of a correct knowledge of the natural history of diseases. How readily do we allopathists detect the want of this knowledge in the homœopaths, yet have overlooked our own ignorance on this point. How is this knowledge to be attained?”

(X)

“ ——— 8th January, 1846.

“It is long since I waged war under the banner of ‘*Young Physic*,’ and I shall most gladly join your ranks and follow your generalship. I have long argued the point with ———, and begged him to try the milk-globules unimpregnated, but without success. I think, in the general opinion of the profession, you will be thought to have gone too far, especially in drug-giving and bill-making England. But in the opinion of the best men, I am sure you will be approved a just judge.”



## PART FOURTH.

## Original Reports and Memoirs.

REPORT ON THE PROGRESS OF HUMAN ANATOMY AND  
PHYSIOLOGY IN THE YEARS 1844-5.

## PART II.

BY JAMES PAGET,

Lecturer on General and Morbid Anatomy and Physiology, and Warden of the College,  
at St. Bartholomew's Hospital.

## NUTRITION.

*In its Chemical Relations.—Formation of Fat.* I cannot find, in the long course of experimental pig-fattening described by M. Boussingault,\* anything that is both new and interesting to human physiology. He and M. Persoz† who has fattened geese, do not appear to have found out more than was already known, namely, that the presence of fatty matter in the food is not essential to the formation of fat in the body; and that a certain quantity of nitrogenous principles in the food is essential to that end; the mere truth being that for an animal to grow soundly fat it must be in tolerable health, and that for this it must have some of its natural diet.

In relation to the well-proved formation of fat from the saccharine principles of the food, an interesting observation is made by H. Meckel.‡ He finds that when grape-sugar and bile are mingled fatty matter is formed from the former; thus, in his chief experiment, performed under the guidance of Marchand, 440 grammes of ox-bile were divided into four parts: the first, while recent, was treated with ether; the second was so treated after exposure for twenty-four hours to the heat of an incubating machine; to each of the third and fourth portions, there were added 4 grammes of grape-sugar obtained from starch; and the third was exposed for five hours, and the fourth for twenty-four hours, to the heat of the incubating machine. Then, from these third and fourth portions, all the fatty matter was extracted by ether. The quantities of fatty matter thus obtained were, from the first portion, 48 grammes; from the second, 54; from the third, 87; from the fourth,

\* *Annuaire de Chimie*, 1846, p. 789, from the *Ann. de Chimie et de Physique*, t. xiv.

† Report from the Acad. des Sciences, 16 Decembre, 1844, in the *Gazette Médic.*, 21 Decembre. Three of the principal papers on the formation of fat, by MM. Edwards, Boussingault, and Persoz, which have been noticed in the last two Reports, are fully reprinted in the *Annales de Chimie et de Physique*, Août, 1845. They are all admirably criticised by Liebig, in the *Annalen der Chemie und Pharmacie*, Juni, 1845; and in the *Lancet*, of the same month.

‡ *De Genesi Adipis in Animalibus*; Halis, 1845, 8vo. I did not obtain this treatise in time to mention the fact as a contribution to the physiology of the liver.

184. The much larger quantity of fatty matter in the third and fourth portions can be assigned only to the sugar which was added to the bile; and the greater quantity in the fourth than in the third appears to show the gradual progress of the transformation, which was not completed in the last portion even after twenty-four hours; for sugar unchanged could still be detected in it.

It is thus made probable that the process of transformation of the amylaceous principles of the food into fat consists in their being, first, by the saliva and pancreatic-fluid, transformed into grape-sugar, of which some is converted by the bile into fatty matter in the intestines, and the rest, absorbed by the vessels leading to the portal vein, is carried in its branches to the liver, and therein is also converted into fatty matter. This is confirmed by an experiment of Trommer, who, having fed animals on grape-sugar, detected it in the blood of the portal vein, but not in that of the hepatic veins. Neither is it impossible that a similar series of transformations should be effected in carnivorous animals; the gelatine of their food being, perhaps, converted into sugar of gelatine, and this into fatty matter.

*Quantity of Nitrogen in Food.* Drs. Schlossberger and Kemp\* have constructed a table of what they suppose to be the comparative proportions of nutriment in our several organic aliments; but it is scarcely more than a table of the comparative proportions of nitrogen in them. It is too long to be extracted, and cannot be analysed.

*Proportions of Elements discharged in the Excretions.* Some experiments on chickens, by Dr. Sace,† show the respective quantities of the elements of their food which are discharged by the cloacal excrements, and by transpiration. Two chickens consumed in a week, in barley, chalk, and sand, 211.544 grammes of carbon, 30.1551 of hydrogen, 10.6123 of nitrogen, 197.468 of oxygen, and 15.4695 of substances which would have remained as ash after combustion; and their cloacal excrements yielded of carbon 50.3946 grammes, of hydrogen 6.7275, of nitrogen 4.3524, of oxygen 45.9836, and of inorganic matter remaining as ash 121.6128. Their joint increase in weight was 19.18 grammes; allowing for this, the quantities of the several constituents which were transpired and retained in the tissues may be easily reckoned.

#### NUTRITION IN ITS RELATIONS TO STRUCTURE.

*Theory of Cell-Development.* A very lucid exposition of this theory, and of the principal facts concerning the history and nature of the nucleated cell in the structures of animals, has been published by Kölliker.‡ The subject has also been thoroughly discussed by Reichert§ in his 'Report on the Progress of Microscopic Anatomy in 1843,' his observations being included in an examination of essays by Karsten,|| Kölliker,¶ and Nägeli.\*\* The general tendency of the whole is to show that we are yet very far from the knowledge of the true mode of development of the nucleated cell in animals. There is indeed in all these essays, as well as in the personal knowledge of most anatomists, an abundance of facts bearing upon the subject; but many, perhaps

\* Lond. and Edinb. Philos. Mag., Nov. 1845; Medical Gazette, Dec. 12, 1845; and Annalen der Chemie, Oct. 1845.

† Annalen der Chemie, Oct. 1844. He seems to think his experiments show what proportion of food is assimilated, and what is at once excreted without being first assimilated; but they do not do this.

‡ In Schleiden and Nägeli's Zeitschrift, Heft ii., 1845; another part is announced for publication, but I have not been able to obtain it.

§ In Müller's Archiv., 1844, No. vi. Jahresbericht, pp. 148-172.

|| De Cella Vitali; Berol. 1843. See last Report.

¶ Entwicklungsgeschichte der Cephalopoden; Zurich, 1844.

\*\* Zur Entwicklungsgeschichte des Pollens; Zurich, 1842; and in Schleiden and Nägeli's Zeitschrift für Botanik, 1844, Heft i.

the majority, of these facts cannot be brought within the expression of Schwann's theory of cell-development; neither can there be yet traced in them any single, uniform, and constant mode of development of the nucleated cell. From the very nature of the case, it seems most probable that one law and one mode must be always observed in the development of the cell and its parts; if it be so, the one mode is unknown; if it be not so, then, in the place of the fair and comprehensive system of Schwann we have a crowd of unconnected facts such as no memory can contain, and of which it would be useless, even if it were possible, to give a brief report.

The observations of Reichert, as well as those in the other works just referred to, relate only, or principally, to the genesis of the nucleated cell and its several parts; and he implies that there is much less room for doubt concerning the metamorphoses of the cell itself, by which, of it or through it, all the more highly organized animal tissues are supposed to be formed. It appears to me, however, that we can be as little sure of many of the changes which nucleated cells are said to pass through in the formation of other tissues, as we are of the process by which the cells themselves are formed. The development of all the fibrous tissues appears especially doubtful. For the investigations of every year show the great difficulty or impossibility of confirming the observations by which Schwann explained the development of these and some other tissues, and the equal facility of finding appearances which cannot be reconciled with his theory, or any other single theory yet proposed concerning it.\*

I have found ample reason for expressing these doubts of the sufficiency of the accepted theories of development in recent examinations of tumours and other morbid growths. Their structure seems peculiarly adapted for testing a theory of cell-development; for they are, doubtless, obedient to the same general laws of formation as the healthy structures are, and, in the unequal and often rapid growth of their several parts, it could hardly happen but that in many specimens all the phases would be seen through which their structures pass towards their fully developed state. But in very numerous examinations I have not found a single example in which a cell has appeared to be forming or formed around a pre-existing nucleus; or one in which fibres have appeared to be formed out of nucleated cells; or one in which nucleated cells have appeared to constitute a stage towards any form of higher development. On the contrary, I have found many instances of rapidly growing structures composed of large collections of fibres without a nucleated cell among or near them; others with abundant nucleated cells, but scarcely any free nuclei or granules, and nothing like a cell incompletely developed round its nucleus; and, again, others (and these of especially rapid growth) with no cells at all, but composed almost entirely of corpuscles like nuclei or cytoblasts.

From these and other observations I am disposed to think that the ordinary (and not the exceptional) mode of development of fibres is, not through nucleated cells, but from a structureless or dimly granular substance which is first *marked*, and then broken up, into fibres. There is good evidence that the cytoblasts which are usually or always imbedded in this substance, influence the development of the fibres; and though I cannot tell how they do so, yet it is certainly not by conversion of themselves into fibres; they shrivel and disappear as the fibres increase and become more perfectly formed.

I think it will be found that, in morbid growths, the nucleated cell is always a terminal, not a transitional, form; for in certain tumours in which the best formed nucleated cells are formed, e. g. the epithelial tumours and some

\* A good evidence of this is in the fact that the most original observers, when they speak of the development of the tissues, almost always cease for the time to be original, and copy both the words and drawings of Schwann or Valentin.

examples of medullary cancers, there are no higher forms found, not even imperfect fibro-cellular tissue, except in small quantity about the blood-vessels. Corpuscles having the characters of nuclei or cytoblasts (to adopt still the usual names) appear to be the really energetic bodies in the growth and determination of these morbid structures; they are found in some tumours so abundantly, and so unmixed with nucleated cells, that their power of multiplying and assimilating cannot be doubted; and it is in some of these tumours also that, apparently under the influence of the cytoblasts, the most perfect fibro-cellular tissue is ultimately formed. What I have seen also of the development of these cytoblasts, leads me to agree with that view of the development of nuclei generally, according to which they are described as formed, not on a pre-existent nucleolus, but out of granules collected in a dark or dim mass of the proper size and shape, which then clears up by the formation of a membranous wall and transparent fluid contents with, in some cases, one or more persistent granules holding the position of nucleoli.

*Anatomy of Nucleated Cells.* Although, if the doubts just expressed are well founded, we may have lost the thread for weaving the facts concerning cells into a system, still they must be collected with peculiar interest; for they must at some time be the basis of structural physiology.

In the last Report, several observations were mentioned concerning the molecular movements of particles within cells. Additions have been made during the year to the most interesting of these, namely those, such as Dr. Sharpey first observed, in which the movements are regular and in currents analogous to the currents of particles in the chara and other vegetables.

Professor Czermak\* has described peculiar rotatory movements of particles in certain vesicles attached to the fine extremities of the seminal tubes of the black salamander. The vesicles are either attached to the peritoneal folds connecting the seminal tubes, or are imbedded in the tubes themselves; and the rotatory movements begin as soon as water is brought into contact with them. The vesicles are spherical or oval, from one fiftieth to one seventieth of an inch in diameter, and are covered by an outer layer of polygonal, mutually-flattened, granular cells. They contain, 1. round, oval, or pear-shaped corpuscles, some of which are not unlike blood-corpuscles, and which vary much in size, but have an average diameter of 1-2000th of an inch. 2. In many of the larger vesicles there is one large corpuscle, not granular, and occupying from one tenth to two thirds of the cavity, the rest of which contains corpuscles like those last described, or crystals. 3. The largest vesicles contain crystals of uncertain nature, alone or with the corpuscles before mentioned.

The rotatory movements vary according to the contents of the cells. The smaller corpuscles (of the first kind) move as if ciliæ within the vesicles impelled them. In some they move from side to side; in others, round their axes; in some, up and down; in some oval vesicles they move along the middle, from one end to the other, backwards and forwards. The movements usually continue for several hours, are accelerated by warmth, and are stopped by drying, or by completely soaking the vesicles in water. The vesicles which contain the larger (second kind of) corpuscles, show a different movement of their contents. No ciliæ could be discerned on the large corpuscles, yet the small ones move along their margins just as if ciliæ seated there impelled them; and the large corpuscles themselves move slowly round their own axes in one constant direction.

The nature and mode of generation of the vesicles is quite uncertain; they at first were taken for the ova of entozoa, but the author gives reasons for thinking that they are not, and holds them to be analogous to those which Remak† found in the mesogastrium of frogs and has described under the name of ciliary vesicles.

\* Oesterr. Med. Jahrbucher, Jan. 1845.

† Müller's Archiv, 1841, H. v.

Kölliker\* also has observed movements analogous to those within the cells in two lower animals, namely, in the cells of the seminal filaments of *Polyclinum stellatum*, and in large cells in the sprouting arms of a young-medusa-like radiate animal.

**Centres of Nutrition.** Mr. Goodsir† believes that the theory which he holds‡ of the existence of "germinal spots" in the secreting glands, i. e. of "a number of points from which acini are developed as from so many centres," may be extended to the process of development and nutrition of all the organs and textures. In an obscure exposition of his theory, he calls the points whose office corresponds to that of the germinal spots in glands, the "centres of nutrition," of the several textures. Each of these centres he considers to be "a cell, the nucleus of which is the permanent source of successive broods of young cells, which from time to time fill the cavity of their parent, and then carrying with them the cell-wall of the parent, pass off in certain directions and under various forms, according to the texture or organ of which the parent forms a part." He names that a "germinal membrane," in which "the nutritive or germinal centres are arranged at equal or variable distances, and in certain directions in the substance of a fine transparent membrane." Such a membrane, identical with that which Mr. Bowman has named the basement-membrane, forms the tubules of glands, and the secreting epithelium is situated on its inner surface; its nuclei are the germinal spots, or centres of nutrition. "A germinal membrane is occasionally found to break up into portions of equal size, each of which contains one of the germinal centres;" showing that it "consists of cells with their cavities flattened, so that their walls cohering at their edges form the membrane, and their nuclei remain in its substance as the germinal centres." The secondary cells developed from the germinal spots or nutritive centres of such a membrane are always attached on its free surface; they are at first contained between the two layers of the membrane, (these two layers being formed by the opposite walls of its component cells,) and when fully developed they carry forward the superficial layer, leaving the nuclei or germinal centres in the substance of the posterior or deeper layer in contact with the blood-vessels.

The theory is illustrated by the example of serous membranes.§ Their germinal membrane is the layer immediately below the epithelium. It does not, in general, show the lines of junction of its component flattened cells. These, its cells, appear to be elongated in the form of ribands; their nuclei, or the germinal spots, being also elongated, expanded at one end, elongated at the other, somewhat bent, and directed, in general, parallel to the subjacent blood-vessels, in the neighbourhood of which they are most numerous. These flattened riband-shaped scales or cells, and the bright crystalline nuclei, appear identical with Henle's nucleus-fibres. (In aged and inflamed serous membranes, they appear to break up into areolar tissue.) It is assumed that the nuclei are the sources of all the [epithelium] scales of the superficial layer of the serous membrane, each being the source of those in a certain compartment of its own; and that, in the development of these epithelium-scales, the necessary nutritive material passes from the blood in the adjacent capillaries to the several centres, from each of which the scales of a compartment derive their origin and their nourishment, till they are detached.

Again, another example is referred to in the bones;|| in which, as already stated, the mass of soft cells in each bone-corpuscle is considered to be the nutritive centre or germinal spot for all the cells within the range of the canaliculi of that corpuscle.

\* *Entwick. der Cephalopoden*, p. 156; in *Reichert's Jahresbericht*, u. s. p. 171; and in *Schleiden u. Nägeli's Zeitschr.* u. s. p. 101.

† *Anatomical and Pathological Researches*; Edinb. 1845, p. 1.

‡ *L. c.* p. 29, and *Trans. Roy. Soc. of Edinburgh*, 1842.

§ *L. c.* p. 41.

|| *L. c.* p. 65.



*Development of Tissues.* Mr. Owen\* thus describes the development of dentine. The cells at the base of the dentine-pulp fall into linear series, directed towards the periphery of the pulp; while those at and close by the periphery, being already similarly arranged in series, become more closely aggregated and enlarged, and change as follows. The larger granular nucleus of each increases, becomes more opaque around a pellucid point in its centre, and then divides through its long axis. The division is succeeded by a further transverse subdivision of each nucleus; and the subdivisions of each become elongated, with their long axes vertical to the plane of the pulp, and then attach themselves to the corresponding elongated and subdivided nuclei of cells in advance. The attached extremities of the nuclei become confluent, and form a linear, or rather, a wavy, series of granular matter.

While these changes are going on, the calcareous salts begin to be deposited—1st, they accumulate within the cells; 2d, they are aggregated in a semitransparent state round the confluent elongated granular nuclei, which now appear as secondary cells; and 3d, they occupy, in a still clearer and more compact state, the interspaces of the cells. The nuclear tracts, i. e. the series of granular nuclei, receiving a smaller proportion of the salts than the other parts do, constitute the *aræ* of the dentinal tubes; the nucleolar membranes, or secondary cells, become the proper walls of the dentinal tubes; the indications of the proper walls of the parent cells are retained through a modification of the arrangement of the calcareous salts in and between them; and the salts deposited within the parent-cells and around the secondary cells convert the intermediate spaces into the intertubular substance. The primary curves of the dentinal tubes depend on the primary linear series of the parent-cells; the secondary curves are determined by the angles at which the subdivided and elongated nuclei unite when their extremities coalesce in the wavy series already mentioned. As the calcification proceeds in its centripetal course, the pulp decreasing in size, fewer nuclei are developed in the cells, and these are smaller. Thus it happens that the linear tract formed by the nuclear matter in a smaller cell unites with the converging extremities of two tracts of a cell in advance. Thus the bifurcation of a tube is produced; and the repetition of this, which becomes more frequent as the calcifying process approaches the base of the pulp, gives rise to the dichotomous division of the main tubes.

The tissues which Mr. Owen has named osteo-dentine and vaso-dentine, acquire their peculiar structure from the change which takes place in the central smaller cells of the pulp being different from those just described. In the formation of osteo-dentine the cell retains its nucleus undivided, and the salts are imparted around it within the cell, but enter only partially into its granular substance. And in the formation of vaso-dentine many of the cells lose their nuclei, which appear to be dissolved. In both these substances the blood-vessels of the pulp remain; in true dentine they wholly disappear.

Mr. Owen's account of the development of enamel agrees with that generally received. He suggests that the transverse striæ of the human enamel-fibre may be caused by the remains of multiplied nucleoli subdividing or modifying the walls of the elongated enamel-cells.

In the development of the cement, the blastema of the capsule acquires a fine granular structure, in which the calcareous salts are imparted in a comparatively clear state, constituting the framework of the cemental tissue. They penetrate the cavities of the nucleated cells, which are arranged in concentric rows around the part already calcified and which rest in cup-shaped cavities in its periphery; but their progress is arrested by the large granular nuclei, which maintains an irregular area, partly occupied by the salts in a subgranular

\* *Odontography*, p. xliii.-lxi. The account given by Mr. Tomes, in his excellent *Lectures on Dental Physiology*, now in course of publication in the 'Medical Gazette,' is in all its principal parts nearly similar to Mr. Owen's.



opaque condition. From these nuclear cavities are subsequently developed the minute radiated tubes (calcigerous canals). But these cavities are not formed in the layer of cement which covers the crown of the human and some other simple teeth; the layer of capsule in which it is developed contains no nucleated cells.

*Growth of Parts.* A series of micrometric observations has been made by Professor Harting,\* with the view, chiefly, of determining the changes of number and dimension which the elements of each tissue and organ undergo in their development and growth from the early period of foetal life to adult age. His results show that the elements of the tissues may be thus placed in two classes: 1st, those elementary structures which, from their first existence to adult life, increase in size either very little or not at all; so that the growth of the tissue which they form must be ascribed to their multiplication, not their enlargement; and 2d, those which constantly increase in size, till the tissue or organ which they form has gained its full dimensions, while their number does not increase after birth; the growth of the whole organ depending on their enlargement, not on their multiplication. In the first class he places the cells of epithelia, the fibrils of fibro-cellular tissue and tendons, the primitive fibrils of voluntary muscles, the cellular cavities of bones, and the blood-corpuscles. In the second class are the cells of the black choroidal pigment, of the fat, and of the liver, the primitive fasciculi (fibres) of voluntary muscles, and the fibres of involuntary muscle, the primitive nerve-tubules, the cells of cartilage, the urine-tubes and Malpighian capsules; and, probably, the fibres of elastic tissue and the ganglion corpuscles. In a third class he says we might place a few elementary structures the number of which appears even to decrease after birth, e. g. the primitive fibres of muscles and the cells of permanent cartilage.

#### STRUCTURE AND FUNCTIONS OF THE URINARY ORGANS.

The *Structure of the Kidney* has been studied by Drs. Gerlach,† Bidder,‡ and Kölliker,§ who all confirm the description of Mr. Bowman in nearly every particular—only they all find that the Malpighian body or tuft of vessels does not lie naked or bare within the capsule. The first two describe it as covered by a layer of epithelium, reflected on it from the walls of the capsule, like the reflected or visceral layer of a serous membrane; and they admit a space existing between this reflected layer of epithelium and that by which the capsule is lined. Kölliker thinks there is no such space, but that the Malpighian body is imbedded in one continuous layer of epithelium, which on the one side covers and fits into all the spaces between its vessels, and on the other is attached to the structureless membrane which forms the wall of the capsule.

Gerlach also says that the Malpighian capsules (which he has examined in the injected kidneys of sheep) are not at the ends of the tubules, but are attached like diverticula to their sides—[but they are certainly terminal in the human kidney; and Bidder and Kölliker assert that they are so in the kidneys of the frog and the triton: they may be lateral in the sheep; but it is more probable that they only appear so when two tubules lead to one capsule. Whenever this happens, as it does sometimes in other than sheep's kidneys, the capsule which is really at the junction of the ends of two tubes may appear like a lateral diverticulum on one]. Gerlach, moreover, holds (as Valentin does) that the ciliary epithelium in the frog's kidney extends over the whole internal surface of the capsule; but neither Kölliker nor Bidder agrees to this.

Kölliker's observations were made in connexion with others on the primordial kidneys or Wolffian bodies, the structure of which he has found to be

\* *Recherches micrométriques*, 4to; Utrecht, 1845.

† *Ibid.* Heft v, p. 508.

‡ *Müller's Archiv*, Heft iv, p. 378.

§ *Ibid.* Heft v, p. 518.

almost identical with that of the kidneys. They consist of tortuous tubules, which are formed by structureless membrane, lined by three or more layers of laminated epithelium-cells, the innermost layer of which bears long ciliæ. The ends of these tubules are dilated into Malpighian capsules, which are lined by a more delicate epithelium without ciliæ, and in each of which there is a Malpighian body, a tuft of capillary vessels, entering and leaving at the part opposite the connexion of the capsule with the tubule, and covered by a layer of cells.\*

*Excretion of Urine.* In a boy affected with extroversion of the bladder, Mr. Erichsen† has observed the mode in which the urine flows through the ureters, and the rapidity with which substances pass from the stomach through the blood into the urine. His observations on the mode of excretion agree with others already made. When a drop of urine has collected within the termination of the ureter, which is elevated on a small papilla, the orifice of the canal opens to a diameter of two or three lines, and then, when the drop has passed it, contracts with a sphincter-like action. When the patient has neither eaten nor drunk for twelve hours, about three drops of urine pass every minute through each ureter, but at no regular intervals; neither is the action of the two ureters simultaneous, or regularly alternate. When the patient lies down, the urine does not flow at all for some time, and then flows slowly and gently, with a less distinct opening and shutting of the orifice of the ureter. Then, if he stands erect, after having been some time recumbent, it flows in a full stream till the ureters have emptied themselves of what had collected in them. During deep inspiration or straining, the flow of urine is suddenly increased, and it escapes in a small stream or in several rapidly succeeding large drops. Its excretion is made twice as rapid by violent exercise as it is during rest, and is peculiarly influenced during digestion. For fifteen or twenty minutes after a meal its flow is much diminished; at the end of this period it begins with increased rapidity, and so continues till the digestive process is nearly completed; and this increase occurred (though to a less extent) when no drink was taken with the meal.

The periods which elapsed between the taking of various substances into the stomach and their detection in the urine were various. Soluble saline substances passed, *cæteris paribus*, more rapidly than vegetable substances. The earliest period at which prussiate of potass was detected was about one minute after it was swallowed; the longest time that elapsed before it was found was thirty-nine minutes; and the chief source of this diversity appeared to be in the state of the stomach at the time. If the digestion of the last meal were finished, and the stomach empty when the solution of the salt was drunk, its passage to the urine was effected in one or two minutes; if the stomach were still digesting, its passage was retarded. The time required for the passage of vegetable infusions was from sixteen to thirty-five minutes; it also varied, but in a less degree, according to the condition of the stomach.

Some other experiments were made to ascertain how soon after taking alkali-salts the urine would become alkaline. Solutions of the citrates and tartrates of potass and soda were given; and the time required for their decomposition and the appearance of their bases making the urine alkaline varied from twenty-eight to forty-seven minutes. In one case, after taking two drachms of bicarbonate of soda saturated with lemon juice, the urine did not till the sixth day recover its natural acid reaction. Its quantity was rapidly and greatly increased by taking the salts of soda.

\* In a paper published in the Medical Times, April 4, 1846, from a Vienna Journal for March, Professor Hyrtl, maintains that the Malpighian capsules have no connexion at all with the urinary tubules, but open into the lymphatics. His account is said to have been drawn from most industrious, careful, and varied researches; but it is contradicted by observations which have been so often repeated and appear so secure from the danger of error, that I must for the present believe him to be thoroughly mistaken.

† Medical Gazette, June 27, and July 4, 1845.

[The publications on the chemical properties of the urine have been, in the last year, so numerous, that nothing less than a special report on them would give a sufficient account of their contents. I shall therefore only give the titles of the chief of them, and a brief notice of the nature of the contents of those which I have read. Many of them treat of things which are at present so far from being applicable in human physiology that the omission of a larger notice of them will not be regretted.]

E. v. Bibra. Ueber den Harn einiger Pflanzenfresser; in the *Annalen der Chemie und Pharmacie*, Januar, 1845; containing analyses of the urine of the horse, pig, ox, goat, and hare.

Golding Bird. On the mode of ascertaining the proportion of solid matters in the urine; in the *Medical Gazette*, May 23, 1845; giving a very useful table for ascertaining this at sight; a short rule being that if the sp. gr. of a sample of urine be expressed in four figures, (water being 1000,) the last two figures will nearly express the number of grains of solid matter in a fluid ounce of the urine; the quantity thus estimated being always rather less than the true quantity.

Boussingault. Recherches sur l'urine des animaux herbivores; in the *Annales de Chimie et de Physique*, Sept. 1845; analyses of the urine of the pig, horse, and cow, and an account of the relation which it bears to the food of each.

Chambert. Sur les sels et la densité des urines chez l'homme sain; in the *Recueil des Mém. de Médecine militaire*, t. lviii, and in the *Annuaire de Chimie*, 1846, p. 693. Data, but no definite conclusion, for estimating the relation between the sp. gr. of urine and its saline and organic contents; with new modes for certain parts of the analysis.

Heintz. Ueber die harnsauren Sedimente; in *Müller's Archiv*, 1845, p. 230; an explanation of the formation of the amorphous precipitates of urate of ammonia in cooling urine. The same chemist has published an essay on the mode of determining the proportions of urea, potash, and ammonia in urine, in the *Annalen der Physik und Chemie*, t. lvi, p. 114.

Bence Jones. On the variations in the alkaline and earthy phosphates in the healthy state, and on the alkalescence of the urine from fixed alkali; in the *Proceedings of the Royal Society*, Jan. 19, 1845, and in the *Philosophical Transactions*, Part I, for 1845; showing the increase of the earthy phosphates after taking food, and their decrease after long fasting; also the increase of alkaline phosphates after feeding on bread alone, and after exercise; with other similar facts.

Laveran and Millon. \* Sur l'élimination de l'antimoine par les urines; in the *Comptes Rendus*, p. 237, t. xxi, and in the *Annuaire de Chimie*, 1846, p. 715; showing the intermittent and very slow elimination of the metal through the kidneys.

Marchand. On the composition of the urine of a tortoise; in the *Journ. für prakt. Chemie*, t. xxxiv, p. 244.

Möller. Ueber das Kystein; in *Casper's Wochenschrift*, Januar. 11, 18, 1845; a summary of his own and other's observations to show the little value of the presence of this principle in urine as a sign of pregnancy.

Pettenkofer. Ueber das Vorkommen einer grossen Menge Hippursäure im Menschenharn; in the *Annalen der Chemie und Pharmacie*, Oct. 1844. The urine of a girl of thirteen years old, with chorea, contained 1.2886 per cent. of hippuric acid: a quantity equal to one-fourth of all its solid constituents.

Pettenkofer. Ueber einen neuen stickstoffhaltigen Körper im Harn; in the *Journal* last quoted, p. 97.

Rabsky. A new mode of determining the proportion of urea; in the *Annuaire de Chimie*, 1846, p. 699.

Krukenberg. On the uniform influence of sweet fruit in making the urine

alkaline, and the production of alkaline urine by the morbid secretions of the bladder after injuries of the spine: in the Oesterr. Medicin. Wochenschrift, 11 Januar. 1845, from Henle u Pfueffer's Zitschrift, Bb. iii. [On all that relates to the progress of animal chemistry, or of chemistry in any other department, the reader may best consult the admirable *Annuaire de Chimie* of MM. Millon and Reiset. The Reports, also, by Dr. Day, in the "*Lancet*" of February, 1845, and in Dr. Rankin's Half-yearly Abstract, contain the substance of many of the best papers on animal chemistry.

#### ORGANS AND FUNCTIONS OF ANIMAL LIFE.

*Structure of Joints.* Mr. Goodsir,\* points out the highly vascular fringes and processes of synovial membrane as more active in the formation of epithelium, and therefore more closely allied to the secreting organs, than other portions of these membrane are. The pulpy nature of their serous covering, their vascularity, and their position where they do not interfere with motion, but hang into the parts of the cavities which may be reservoirs of synovia,—all, he thinks, favour this opinion.

*Anatomy of the Knee-joint.* Dr. Gruber† has dissected 160 knee-joints to determine how, and how often, the bursæ near the joint communicate with it, and what are the usual arrangements of the recesses formed by the synovial membrane. The chief things he has noticed are—1. The sac which the membrane forms between the outer and posterior part of the joint and the tendon of the popliteus, and which covers the outer margin of the external semilunar cartilage, is often divided by a septum into two parts, placed one behind the other, and communicating by one opening with the joint. 2. The bursa under the quadriceps femoris is found in about every sixth or seventh person; and communicates with the cavity of the joint in about every ninth person. In 47 males it had a separate cavity in only 9; and in 33 females in only 3. 3d. The bursa between the tendon of the M. semimembranosus, the inner condyle, and the inner head of the M. gastrocnemius, is often locular or divided by septa into two or three cavities. In robust persons it is often, though not diseased, two and a half inches long and three-fourths of an inch broad, so that it may be felt externally, and in them it generally communicates with the joint by an opening half an inch in diameter. 4. The bursa under the lig. patellæ was once found communicating with the joint; that under the inner head of the gastrocnemius never was: a communication between the femoro-tibial and tibio-fibular articular cavities was very rarely seen.

*Muscles of the Larynx.* Dr. Gruber‡ has also found (but only once in a hundred dissections) a muscle which he names the M. thyroideus transversus anomalus. It is placed transversely over the upper two-thirds of the middle crico-thyroid ligament, between the angle of the thyroid cartilage and the crico-thyroid muscles. Its greatest breadth is four and a half lines; its greatest width seven and a half. It has both its origin and insertion on the inferior angle and margins of the thyroid cartilage. The upper fibres are transverse and straight; the lower are longer and arched, with their convexities downwards. They are all fleshy at their attachments, and all have additional points of insertion on the crico-thyroid ligament. The action of the muscle is supposed to be assistant to the crico-thyroid muscles; to make the crico-thyroid ligament tense when the thyroid cartilage is fixed, and, when in full action, to approximate the alæ of the cartilage; or, when the cricoid cartilage is fixed it may draw the thyroid downwards and forwards.

\* Anatomical and Pathological Observations, p. 42.

† Prager Vierteljahrschrift, 1845, B. i; and Oesterr. Med. Wochenschr., 25 Jänner, 1845.

‡ Oesterreich. Medic. Jahrbucher, Mai, 1845.

## NERVOUS SYSTEM.

*General Structure, Origin, and Course of the Nerve-fibres.* The most important contribution to the physiology of the nervous system, perhaps, indeed, the most important physiological production of the year, is from Kölliker.\* The main design of his essay is to discuss the question of the independence and speciality of the sympathetic nerve. This, it is known, has been long disputed; especially between Bidder and Volkmann on the one hand, and Valentin on the other. The former maintained, as the anatomical evidence of the independence of the sympathetic, that there belonged to it a peculiar set of nerve-fibres, characterized by their fineness, (they being only about half or one third as large as the cerebro-spinal fibres), their paleness, the absence of a double contour, their nearly uniform contents, and their yellowish gray colour when in bundles. The latter held that the sympathetic fibres are neither in structure nor in relations peculiar. The several statements on both sides have been nearly all inserted in former Reports.†

In the discussion of the question, Kölliker decides—1st. That the fibres described by Remak as peculiar to the sympathetic nerves, and which are commonly called Remak-fibres, are, as Valentin has always held, not nerve-fibres at all, but neurilemma, consisting of imperfectly developed fibro-cellular bundles. 2d. He determines that Bidder and Volkmann are right in their description of the structure of the fine nerve-fibres, or, at least, of the well-marked examples of them; and that these are not (as Valentin maintained that they were) Remak-fibres. But he denies that these fine nerve-fibres are peculiar to the sympathetic system, or even so different from the common larger cerebro-spinal nerve-fibres that they ought to be regarded as of a kind distinct from them. To justify this denial he shows that the characters assigned to these fine nerve-fibres as distinctive, by Bidder and Volkmann, are neither definitely marked, nor constant, nor essential; that there is no real difference between these fine fibres and those of the brain, spinal cord, and nerves of special sense; that, commonly, the large fibres assume near their peripheral ends the size and some other characters of the smaller ones; and that many fine fibres are found in all nerves, though it is generally true that there is a smaller proportion of them in the cerebro-spinal than in the sympathetic nerves.

But, although it thus appears to be an error to speak of sympathetic and of cerebro-spinal nerve-fibres as if they were two different *kinds* of fibres, yet the differences which do exist between them, and the various proportions in which the fine fibres occur in different nerves, make it important to discern their origin and course. On these points, Kölliker first proves the most important fact that these fine fibres have their origin not only in the ganglionic or nerve-corpuscles of the sympathetic ganglia, but in those also of the ganglia on the cerebral and spinal nerves, and in the corpuscles of the brain and spinal cord. In this, his observations fully confirm those of Helmholtz, Will,‡ and Hannover,§ who like him have seen this mode of origin, and of Bidder and Volkmann, who from another mode of investigation concluded that fine fibres must thus arise. Kölliker has seen this mode of origin of nerve-fibres in the spinal cord and in the spinal and sympathetic ganglia of the frog, in the spinal ganglia of the tortoise and cat, and in the Casserian ganglion of the cat and guinea pig. Hannover has found it in all classes of vertebrata and in many invertebrata, in the brain and spinal cord, and in ganglia of all kinds; neither has he observed any other mode of origin besides this. The description given

\* Die Selbständigkeit und Abhängigkeit des sympathischen Nervensystems; Zurich, 1844, 4to.

† Report on Microscopic Anatomy, pp. 33-5; Report for 1842-3, p. 18; Report for 1843-4, pp. 43-4-8. All that Bidder and Volkmann have maintained may also be found in the art. Nervenphysiologie, by the latter, in Wagner's Handwörterbuch.

‡ See last Report, p. 45.

§ Recherches Microsc. sur le Système Nerveux; Copenhagen, 1844, 4to.



by Kölliker of the spinal ganglia of the frog is, that they contain one form of ganglion-, or nerve-corpuscles, which are of simple shape and give off no processes; and many other corpuscles, more or less pyriform, which at their smaller ends are drawn out into a process. This process, like the corpuscles, is pale and finely granular; it is from 1-10,000th to 1-7000th of an inch in diameter, and after proceeding about 1-1000th of an inch, it rather suddenly acquires a dark contour and slightly granular contents; it, in short, becomes a fine nerve-fibre. And, in regard to those cases in which he has not *seen* this mode of origin of the fine fibres, Kölliker so far confirms or admits the truth of Bidder and Volkmann's observations respecting the relative number of fine fibres which enter and leave the ganglia, that he considers it proved that a great number of these fibres have their origin in the ophthalmic ganglion, and in the ganglion of the vagus of fish; and considers it as highly probable that the ganglia of the cerebral and spinal nerves of all the higher animals are also sources of origin for similar fibres.\*

To these observations may probably be added those of Dr. Todd and Mr. Bowman.† For although they do not demonstrate it, yet it is, as they state, most probable that one (or more?) of the processes of those which they name "caudate nerve-vesicles" is prolonged into a nerve-fibre. Their description of these vesicles or corpuscles is that they possess the general characters of the common nerve-vesicles, but have one or more long processes from their central parts or bodies. These processes, like the interior of the vesicle, are very delicate, frail, and finely granular. When unbroken, one or more of them may be traced, extending far from the vesicle, then dividing into two or three branches, which again divide, and give off extremely fine transparent fibres, such as may either connect distant nerve-vesicles, or become continuous with nerve-fibres. Such vesicles, they say, are best found in the locus niger, and in the gray matter of the cerebellum and spinal cord; and the tissue in their vicinity is freely traversed by delicate filaments which appear to be the ramifications of their caudate processes.

It has been already said that these fine fibres proceeding from the nerve-corpuscles are not peculiar to the sympathetic nerves (commonly so-called); yet all the branches of these sympathetic nerves contain a larger proportion of them than the common cerebro-spinal nerves do, and some of them contain no other fibres besides these. And assuming that the origin of these fibres is proved, the next question concerns their mode of distribution. Kölliker shows that there are much greater difficulties in the way of tracing these fine fibres from their origins than Bidder and Volkmann supposed. All that can be certainly said is that, 1st, the fibres arising in the sympathetic ganglia go partly to the viscera, and partly through their communicating branches, (which have been often called origins, or roots of the sympathetic, and are composed almost entirely of fine fibres,) to the anterior branches of the spinal nerves, in which most or all of them pass peripherally; 2d, some of the fine fibres which arise in the spinal ganglia pass through the communicating branches to the sympathetic, and are distributed in the viscera, and others go to the posterior branches of the spinal nerves; 3d, the fine fibres arising in the ganglia of cerebral nerves probably pass out from them with the nerves that are proceeding to their peripheral distribution. But it is yet uncertain whether the sympathetic ganglia‡ send fibres into the posterior branches of the spinal

\* The most striking instance in which more fibres leave than enter the ganglia is seen in the septum of the auricles of frogs' hearts, which is so transparent that the ganglia and nerve-fibres may be counted in it. Here Bidder has often seen more fibres in one than in the other of the two branch from a ganglion—e. g. five in one, and seven in the other. Volkmann, in *Art. Nervenphysiologie*, l. c.

† *Physiolog. Anatomy*, p. 314, fig. 55-6.

‡ i. e. the ganglia of the sympathetic system commonly so called; the sympathetic system of Volkmann includes *all* the fine nerve-fibres, wherever they originate.



nerves; whether the spinal ganglia send fine fibres to their own anterior branches; whether the fine fibres arising in the spinal cord go to the sympathetic or to the spinal nerves, or to both; and what course the fine fibres which run with the spinal nerves take, though, from the large number of them in the branches of sensitive nerves, one may conclude that they are chiefly distributed with these.

As to the relative proportions of large and fine fibres in the nerves distributed to various parts, Kölliker concludes from his own and other observations that, 1st, the nerves of voluntary muscles contain in their trunks a majority of large fibres, but in their peripheral distribution either only, or a majority of, fine fibres; 2d, the nerves of the skin contain (for the most part) equal numbers of both; but in some of them, one or other size of fibres greatly preponderates, and in all of them the fine fibres greatly preponderate in their peripheral distribution; 3d, the nerves of sensitive mucous membrane are, in this respect, like those of the skin, except that in the nerves of the teeth-pulps and the gums there is a great majority of large fibres; 4th, in the nerves of involuntary muscles, and of the less sensitive or insensible mucous membranes, there is a great predominance of fine fibres.

To these important conclusions respecting the general anatomy of the nerve-fibres, a few less considerable facts may still be added.

*Structure of the Fibres.* Stadelmann\* describes the axis-cylinder of the nerve-fibres as very distinctly visible in transverse sections of them. Its outline has commonly the same form, and is nearly half as large, as that of the nerve-fibre itself, but sometimes it looks like a mere chink or a central point.

Harting† has once noticed (but long after death) some peculiar fibres in the neurilemma of a nerve on the abdominal muscles of a Molge (Triton?) punctata. They were narrow, spirally twisted, band-like, with sharp outlines, and each of them had on its outer edge a row of very short ciliæ, which ended in minute round knobs, and were set at regular distances from one another. The average width of the fibres was about 1-1600th of an inch; the average length of the ciliæ 1-1100th; the diameter of their knobs 1-2300th; their distance apart about 1-800th.

*Central Terminations of Nerve-fibres.* Probably we must not conclude from the foregoing observations by Hannover, Kölliker, and others, that the fibres in the nerve-centres have no other mode of termination (if it may be so called) than that by connexion with the nerve-corpuscles. In the last Report, I mentioned the observations of Dr. Lonsdale,‡ who found, in two cases of anencephalous monsters, that the nerve-fibres in the truncated portions of the fifth and other nerves, which hung unattached in the base of the skull, formed loops; a fact which seemed confirmatory of the theory of central terminal loops of the nerve-fibres in the brain. I have recently had occasion to confirm this fact in a mature foetus, whose cerebro-spinal axis was truncated at the medulla oblongata. In the loose hanging ends of the fourth and fifth nerves, all the fibres appeared forming loops, exactly like those figured by Dr. Lonsdale. There can be no doubt therefore that this is the usual arrangement of the nerve-fibres in these cases, whatever may be the import of the fact.

*Peripheral Terminations of Nerve-fibres.* In doubt concerning the terminal loops of nerve-fibres, Volkmann§ tried the experiment of dividing half the

\* Sectiones transversæ partium corporis humani, p. 17.

† Tijdschrift voor natuurl. Geschied. en Physiologie, 1845, d. xii., st. 1. In another examination he could not find these fibres. There is some similarity between this observation and that of Renak, in Müller's Archiv., 1841, p. 30. and Valentin's Repertorium for 1841, p. 108.

‡ In the Edinb. Med. and Surg. Journal, No. 157.

§ Wagner's Handwörterbuch, art. Nervenphysiologie, p. 565.

trunk of the infra-orbital nerve, thinking that if there were terminal loops, at least several of the fibres divided might be connected at their peripheral parts with some of the undivided fibres; and that thus, on irritating the peripheral portion of the divided nerve, the impression might be conveyed centrifugally through its fibres, then through the loops and through the undivided fibres to which they led, and thus to the brain. But no pain was produced by the irritation; indicating either that sensitive nerves do not convey impressions towards the periphery, or else that there are no such peripheral loops as can convey impressions from one nerve-fibre to another.

*Termination of Nerve-fibres in the Pacinian Corpuscles.* The discoverer of the Pacinian corpuscles has continued his observations\* on these bodies, and confirms the account of Henle and Kölliker recorded in the last Report. The nerve-filament within the corpuscle has, he says, a single contour, like the sympathetic filaments; up to the base of the corpuscle its contour is double. At its termination it presents a granular swelling like the common ganglion-corpuscle.

*Anatomy and Physiology of the Nervous Centres in general.* The researches of Hannover and Kölliker already reported, confirming and much extending those recorded in the last Report, render it necessary to admit the existence of many more nervous centres than are commonly reckoned. If under this title we may include all those bodies in which nerve-fibres have their origin or termination, and of which it can be made probable that they are centres or co-ordinators of the actions of many nerves, we must now include not only the brain and spinal cord, but, probably, all the ganglia on the spinal and cerebral nerves, and all those of the sympathetic system; for in all these we may believe, on the grounds already adduced, that there are nerve-corpuscles giving origin to nerves; and in many (as will presently appear), there is evidence of the same kind of action as in the acknowledged nervous centres. Not indeed that all these nervous centres have the same powers—they are not, like the brain, in direct communication with the mind, nor have all so wide a range of action as the cord; but each is a nervous centre to its own district, receiving from, and sending to, its own parts, the impressions which pass along the nerve-fibres which enter it. Each also is in communication with other centres, and, probably, through one or more of these, with the spinal cord; through which also, if not more directly, all may be connected with the brain, the head of the whole nervous system, through and beyond which no impression can be conveyed.

*Centres of the System of the Sympathetic Nerves.* The same researches of Kölliker and others show that this nerve, or system, may now be described as mainly composed, 1st, of ganglia, which (like other ganglia) contain (a) nerve-fibres traversing them; (b) nerve-fibres originating in them; (c) nerve-corpuscles giving origin to nerve-fibres; and (d) free nerve-corpuscles; and, 2dly, of various nerve-fibres, comprising (a) those which arise in their own ganglia; (b) those which it receives from the ganglia of the spinal and cerebral nerves; and (c) those which it receives from the brain or spinal cord, or both. Among the last are, probably, the large fibres, which are contained in some of the branches of the sympathetic, and through which the occasional influence of the brain and spinal cord upon the viscera is probably exercised.

Now all these same elementary structures occur in different proportions in the cerebro-spinal nervous system, and some of them pass from it into the sympathetic. Kölliker's conclusion is, therefore, that the sympathetic nerve is independent and peculiar, not by peculiar elements which are not found in

\* *Annali Univ. di Medicina*; Luglio, 1845, p. 208.

other parts of the nervous system, but by its very numerous ganglia, by the fine fibres which proceed from their corpuscles, and by the general complexity of its composition; and that it is dependent on the other parts of the nervous system, in that it receives fine fibres from the ganglia of the cerebral and spinal nerves, and both fine and large fibres from the brain and spinal cord. Probably, however, it is in this respect more dependent in the higher than in the lower vertebrata.

And, as for the function of the sympathetic, these facts prove that its ganglia are so many centres of origin for some of its fibres; and they thus afford the anatomical evidence of what analogy and the physiology of the system had already rendered nearly certain, namely, that each ganglion of the sympathetic is, in its own sphere, a nervous centre, receiving, transmitting, reflecting, and, perhaps, even originating, the impressions on which the harmonious movements of the parts to which its nerves are sent depend. It is not necessary here to enumerate the organs thus dependent on nervous centres of the sympathetic, or the degrees in which they are severally subject to the influence of the brain and spinal cord. The best examples of movements governed by the sympathetic are afforded by the actions of portions of the heart, mentioned in a former part of this Report; by the many days' continuance of all the organic functions in frogs, after the removal of the brain and cord (saving the medulla oblongata);\* and by the wide-extended influence of irritation of the intestines, if they are cut out with the mesentery (in which are their ganglia) still attached.

The influence of the sympathetic on the nutrition of the parts to which it is distributed is commonly known; such an influence appears to me to be constantly in exercise, affecting not only the quantity, but the mode, of nutrition in parts. I am surprised that so acute a physiologist as Kölliker should think this influence is exercised only in the power which it may have of determining the size of the blood-vessels; as if the vessels were not enlarged in many conditions, the results of which are all different; but I repeat what was stated in the last Report, that there is at present no evidence that the sympathetic system (i. e. the system commonly so called) exercises an influence on nutrition different either in degree or in kind from that which is exercised by the cerebro-spinal system.

*General Physiology of the Spinal Cord.* The general tendency of the investigations of the last year has been to prove that the spinal cord is neither a mere collection of tracts of nerve-fibres nor a single nervous centre, but (if I may use the most popular language of the day) a collection or series of *central stations*, each of which has its own lines of nerve-fibres terminating in it, and serves to receive, and to transmit on numerous lines and in various directions, the impressions which are conveyed by the centripetal nerves abutting on it. The chief evidence for this, which, though not a new view, has hitherto been a very doubtful one, is as follows:

1. Volkmann† has submitted the question whether the nerve-fibres of the spinal nerves remain and end in the cord, or go on to the brain, to the test of a kind of measurement. He weighed four pieces of a horse's spinal cord, each seven centimeters long, and taken respectively from below the 2d, the 8th, the 19th, and the 30th pairs of nerves. Their weights (in the order above named) were 219, 293, 163, and 281 grains; the areas of the transverse sections of the gray matter in them (in the same order) were 13, 28, 11, and 25 square lines; and those of the white matter 109, 142, 89, and 121 square lines. Thus, the quantity of white matter of the cord is absolutely less at the cervical

\* See last Report, and Volkmann's Art. Nervenphysiologie, p. 500.

† Wagner's Handwörterbuch der Physiologie, art. Nervenphysiologie.

than at the lowest part of the lumbar portion, and much less in the lower than in the upper cervical portion. The contrast was more marked in a comparison of the sum of the areas of transverse sections of all the spinal nerves of a serpent (*Crotalus mutus*) with that of a section of the upper part of the spinal cord. The former (purposely estimated below the truth) might be reckoned at  $\cdot 0636$  of a square inch; the latter only  $\cdot 0058$ . The total size of the nerves, therefore, is at least eleven times greater than that of the cord—a difference which cannot be explained on the supposition that the nerve-fibres, when they pass into the cord, become smaller.

2. The almost necessary deduction from these facts is that many or all the nerve-fibres terminate in or very near those regions of the cord into which they penetrate; and this is strongly confirmed by the observations of Hannover and Kölliker, already often referred to, both of whom have demonstrated the fine nerve-fibres as prolongations of the processes of some of the nerve-corpuscles of the gray matter of the cord.

3. A step further is made by the remarkable observations of Volkmann, which have determined at least two examples of small portions of the cord having absolute and uninfluenced control over the movements of parts, to which parts they are the true and sole nervous centres. I refer here to the governance of the rhythmical movements of the lymphatic hearts by the two definite portions of the cord, of which an account has been already given. The evidence is complete that these portions of the cord are as truly the nervous centres for the two hearts as the portion of the medulla oblongata is for the respiratory movements.

4. Something of the same kind as this influence of the cord on the lymphatic hearts is indicated by an observation of Budge.\* If a piece of the cord of a frog, scarcely two lines wide, be removed from the place at which the great brachial nerve goes off, it constantly occurs that the pulse of the heart decreases in frequency within two hours after the operation, and this does not happen when all the rest of the cord below this portion is removed.

But if it be thus proved that there are in the spinal cord many central stations, the question still remains, how an impression is conveyed from one to the other, or from any of them to the brain?† It is evident, that there are other modes of conveyance besides that through the continuous course of the fibres first impressed; it is not certain that any fibres pass uninterruptedly from the periphery to the brain, yet the impressions are precisely conveyed both to and from the brain; and there is no support in all these facts for the erroneous experiments of Van Deen,‡ which would have made it appear that not only the nerve-fibres, but the impressions also, stop short in or near the part of the cord on which they fall. Some of his more correct experiments show that even a small length of the gray matter left in the cord, when all around it is cut away, is sufficient for the conveyance of impressions up the cord of the frog, but how the conveyance is effected is as yet a question.

Some isolated facts concerning the anatomy and physiology of the spinal cord remain to be reported:

Dr. Harless§ has described some singular results obtained by the action of a constant weak galvanic current on the spinal nervous system of frogs at the time of their greatest irritability, i. e. before the waking from hybernation. One pole was applied to the skin, the other to the spinal nerves of a beheaded frog. When the violent general convulsions had ceased, periodically inter-

\* Oesterr. Medic. Wochenschr., 10 Jan. 1846; from Froriepe's Notizen, 1845, No. 783.

† Hypotheses have been suggested in the year just passed by Drs. Todd and Volkmann (l. c.), but they both seem to me insufficient for the facts.

‡ See last Report, p. 50, and Report for 1842-3, p. 20.

§ Müller's Archiv, 1845, Heft i.

mittent convulsions, with regular intervals of rest from three to six seconds long ensued, and these often went on for one or two minutes. After these had ceased, there were no more spontaneous twitchings; but if, while the body was at rest, the slightest shock were given to the dish or the table on which it lay, it produced severe tetanic convulsions of the whole frame, which lasted from seven to eight seconds. The same convulsions were produced in an amputated leg subjected to the same influences; they were therefore not dependent on a reflex influence, but were direct. Careful examination showed that the shock which produced the convulsions did not at all displace the poles of the battery, so as to break and then renew the circle, and thus give each time a fresh galvanic irritation. And it was found that a shock, though slight, if given even to the connecting wire alone of the battery, was sufficient to excite the same tetanic convulsions. It was thus and by later experiments evident, that a shock communicated to a voltaic apparatus is sufficient to produce an alteration in the intensity of current, increasing it enough to produce convulsions in the animal galvanometer and a greater deviation of the multiplier-needle.

*Cerebro-spinal Fluid.* M. Longet\* has found that the peculiar unsteady tottering movements, like those of drunkenness, which M. Magendie ascribed to the removal of the subarachnoid fluid of the spinal cord, are really due to the division of the muscles of the occipito-atlantal region, which is made to form a passage through which the fluid may be drawn off. Whenever M. Longet drew off the fluid without injuring those muscles, the animal preserved the power of motion unimpaired; but when he divided the posterior sub-occipital muscles, (including always the recti capitis postici minores, and the supra-spinous ligament in the animals in which it exists,) the peculiar defects of motion were produced, although the cerebro-spinal fluid was left untouched, and the sheath of the cord unopened. He ascribes the impairment of motion in these cases to the falling of the head when its attachments to the atlas are destroyed, and the consequent dragging and pressure of the upper part of the cord, and especially of the medulla oblongata and pons. For the effects of the division of the muscles and other tissues are completely prevented by artificially supporting the animal's head in a raised position; and in different animals, the degree in which the movements are impaired is directly proportionate to the amount of separation which takes place between the occiput and atlas, when their connexions (the occipito-atlantal ligament excepted) are divided. The speedy recovery of the animal, which Magendie ascribes to the rapid reproduction of the fluid, M. Longet considers to be due to the readiness with which the nervous masses (especially in animals) adapt themselves to new and unnatural pressure. He observed a striking analogy between the effects of the divisions of these muscles, and those observed by M. Flourens and himself in consequence of injuries of the cerebellum; and hence draws another evidence, that the former are due to the pressure and dragging of the medulla and pons, with which the crura of the cerebellum are connected.

*Relation of the Nerve-roots to the Spinal Cord.* Van Deent has published a plate from an accidentally-obtained preparation magnified 320 diameters, which appeared to show the mode in which the filaments of the nerve-roots are arranged in the substance of the cord. The central filaments of the root are drawn passing straight into the cord; the upper and lower ones passing in oblique lines upwards and downwards respectively. They all appear variously interlacing with the filaments of the cord, which, for the most part, run longi-

\* Bull. de l'Acad. de Médecine, 15 Sept. 1845, and Gazette Médicale, 6 Sept.; and in other contemporary journals. See also Comptes Rendus, 7 Juillet, 1845.

† Tijdschrift voor Natuurl. Geschied. en Physiol., 1844, st. 2.



tudinally; but he could in no instance find a continuity between the two. His account thus far agrees nearly with that of Stitting.

**ANATOMY OF THE BRAIN.** *Cerebro-spinal Membranes and Vessels.* Among the observations which Purkinje\* has republished from his Essay, first printed in 1838, are many concerning the nerves of the membranes and vessels of both the brain and the spinal cord. In the cerebral dura mater, the nerves are most abundant in the neighbourhood of the trunks of the three chief meningeal arteries. They come to these from the sympathetic system,† and the principal part lie in company with the arteries; but some also leave them and ramify separately in the substance of the membrane.

In the dura mater inclosing the spinal cord, Purkinje could not find a trace of nerves; but on the fibrous lining of the vertebral canal and the sinuses between it and the bodies of the vertebræ, (which fibrous lining may be regarded as an outer layer of the dura mater, divided at the foramen magnum,) he found abundant plexuses of sympathetic fibres.

The pia mater of the cerebellum displays many nerves, which branch separately from the arteries as those of the pia mater of the cord do; but they are less abundant than those are. The nerves ramifying on the pons and on the cerebrum appear, on the contrary, to belong exclusively to the arteries. In the choroid plexuses no trace of nerves could be found; but there is a dense plexus of filaments of the sympathetic system around the vena magna Galeni. It passes into the tentorium cerebelli, and appears to belong to it more than to the venous system.

In the pia mater of the spinal cord, a much more copious distribution of nerves exists than in any part of the cerebral membranes. The largest fasciculi, containing from thirty to fifty filaments, are near the anterior spinal artery, whence some pass into the process of pia mater in the anterior fissure, and form loops therein. Other large bundles which, for the most part, run longitudinally, are near the ligamentum dentatum, and about the posterior median line of the cord, though here they are less abundant than in front. In the neighbourhood of the origins of the spinal nerves, the bundles of sympathetic filaments are fewer and smaller. Their number is greater about the upper than about the lower part of the cord.

All the nerve-filaments of these plexuses in the pia mater appear to belong to the sympathetic system. They combine with those already mentioned on the pons and cerebellum; but have never appeared to be connected with the roots of the cerebro-spinal nerves. Some of the filaments come to the pia mater with its arteries; but they soon part company, and the nerves appear to increase, and form plexuses quite independent of the arteries.‡

*Dimensions of the Brain.* M. Baillarger§ has invented a new mode of measuring the surfaces of brains, by dissecting out all the white substance from their interior, and then unfolding the exterior, and taking a cast of it. From his measurements he estimates that the average superficial extent of the human brain is 669·3 square inches; and that it is far from true that, in general, the intellect of different animals is in direct proportion to their respective extents of cerebral surface. If their absolute extents of surface be taken, the rule is manifestly untrue in many instances; and it is not more

\* Müller's Archiv, 1845, Nos. iii, iv.

† He suggests also that those which dissectors have supposed to be branches of the fourth and fifth nerves, going to the dura mater, are really branches of the sympathetic, which pass through these nerves to the membrane.

‡ As already stated, this account of the nerves of the membranes of the cord is confirmed and extended by the original observations of Mr. Rainey. It is also confirmed by Volkmann, in his article, *Nervenphysiologie*, already often referred to.

§ Gazette Médicale, 19 Avril, 1845; Report from the Acad. de Médecine, 15 Avril.



true if the extent of surface in proportion to the volume of the brain be regarded; for, according to M. Baillarger's measurements, the human brain has less superficial extent in proportion to its volume than that of [many] inferior mammalia.

**PHYSIOLOGY OF PARTICULAR CLASSES OF NERVES. *The Motor Nerves.*** It has been already mentioned that E. H. Weber has successfully employed the continuous electric current developed by a rotating magnet for maintaining constant contraction of the voluntary muscles. He has also\* applied it for testing the functions of the pneumogastric nerves; and it has been employed by Volkmann,† in an extensive series of experiments on the functions and modes of action of the motor nerves generally. The chief results are as follows:

1. The central organs, but not the nerves, are capable of an excitement which induces fixed muscular contraction even after the withdrawal of the external stimulus. For, with the magneto-electric stimulus applied directly to them or their nerves, the voluntary muscles, the œsophagus, separated fasciculi from the heart, and the iris (!) remain contracted just as long as the stimulus acts upon them, but no longer; but if the same stimulus be applied to the brain or spinal cord, the contraction of the voluntary muscles and œsophagus is prolonged after its withdrawal.

2. The motor nerves of the anterior, but not those of the posterior extremities [of the frog], arise from the uppermost part of the spinal cord. For if the anterior part of the cord be electrified, the contraction of the anterior extremities is often prolonged after the withdrawal of the stimulus; but that of the posterior extremities ceases with the cessation of the stimulus. Now, if the motor nerves of both extremities arose in the brain, this difference would be unaccountable; and as, according to the preceding rule, it is stimulus of the central organs alone which induces prolonged contractions, it follows that the upper part of the cord is a centre to the brachial, but not to the sciatic, plexus [of the frog].

3. The motor nerves of the heart, stomach, and intestines arise neither in the brain nor in the spinal cord; for none of these organs is thrown into fixed contraction when the brain and cord are directly stimulated by the magneto-electric current, although it is a law that all muscles will thus contract when either the origins or the trunks of their nerves are stimulated.

4. The heart has a central organ in itself. If, when cut out and whole, it is sufficiently excited by the magneto-electric current, it remains in fixed contraction after the current is interrupted; a condition which never happens in muscles separated from their centres, or in separated portions of the heart itself; for in these the contraction ceases with the stimulus.

5. Central organs modify the motor stimuli passing through them, and are thereby regulators of movements, [the *contractions* hitherto mentioned are states of fixed cramp-like contraction; the *movements* here named are movements of alternate contraction and relaxation]. For direct stimuli through the motor nerves always produce fixed contraction of their several muscles; but if the same motor nerves be acted on by reflected stimuli, then (even though these stimuli be continuously applied to the incident nerve, as by the magneto-electric current) *movements* of the muscles ensue, and these with order and an appearance of purpose. The replacement of the cramp-like contractions by the orderly movements in such a case, can be accounted for only by the intervention of the central organ through which the stimulus acts upon the motor nerves.

6. The heart, stomach, and intestinal canal possess central organs, a por-

\* Archives de Physiologie, Janv. 1846.

† Müller's Archiv, 1845, No. 5, p. 407.

tion of whose centripetal fibres lie in the vagus, the spinal cord, and the chain of the sympathetic. For when any of these parts of the nervous system is included in the magneto-electric current, there is no contraction of the heart, stomach, or intestines; whence it follows that neither their nerve-fibres nor their centres are directly stimulated in the experiment.\* But orderly movements of alternate contraction and relaxation do, in this experiment, ensue in them; these, therefore, in accordance with the fifth rule, must be reflex movements, regulated by the nervous centres of the several organs, to which centres the stimuli were conveyed by fibres going to them from the vagus, sympathetic, or spinal cord.

**PHYSIOLOGY OF PARTICULAR NERVES.** *Third Nerve.* Among these experiments by Volkmann, are some† which show that when the third nerve is stimulated by the magneto-electric current, the voluntary muscles of the eye are thrown into contraction, which ceases on the instant of withdrawing the stimulus, while the contraction of the iris is sometimes prolonged beyond that time. In the voluntary muscles, the contraction indicates that they were stimulated directly through their nerves; in the iris it indicates a stimulus received by it through a centre; and Volkmann suggests that the difference is explicable only by supposing that there are in the trunk of the third nerve fibres which convey centripetal impressions to the ophthalmic ganglion, as to a centre from which the stimulus is conveyed to the nerves of the iris.

*Fifth Nerve.* Two interesting and well-observed cases of paralysis of the fifth nerve on the left side are recorded by Mr. Dixon.‡ In one (a woman fifty-three years old) there was (on the paralysed side) loss of motion in the muscles of mastication, and loss of common sensation in the conjunctiva, Schneiderian membrane, tongue, and skin of the face, except near and about the angle of the jaw. There was also complete loss of taste on the edge and forepart of the left half of the tongue, though on all other parts of the tongue the sense was perfect; and on the same side, there were loss of smell, partial loss of hearing, impaired vision, and cessation of the flow of tears while weeping at the other eye. Subsequently, the paralysis involved the third, and perhaps the optic nerve, of the same side. In the other case, the losses of nervous power were similar, but they were complicated by neuralgia of some of the branches of the fifth, and by inflammation (perhaps connected with the paralysis) of the conjunctiva, cornea, and other parts of the left eye. [In regard to the question whether the sense of taste be perceived through the fifth, or the glosso-pharyngeal nerve, these cases, like several others, are less decisive than at first they seem; for in those parts of the tongue in which the fifth nerve is distributed, it may only supply, as it does in the eye and the nose, a certain condition which is necessary in order that the special sense may be exercised through a special nerve. Even the facial nerve appears to have an office in the tongue essential to the sense of taste; yet it is not itself the nerve of taste].

M. Marchal (de Calvi)§ has related five cases, in which paralysis of the third nerve followed neuralgia of the fifth. One patient had diplopia without apparent deviation of the globe, which ceased on compression of the external frontal nerve. In two of the cases, the neuralgia followed a wound of a branch of the fifth nerve.

*Facial Nerve.* Under the title of the 'Anatomy of the Geniculate Ganglion,'

\* In experiments by E. H. Weber (Arch. Gén. de Physiologie, Janv. 1846), it is said that the heart ceased to beat on strongly electrifying the pneumogastric nerves, and that its action was weakened and retarded when the stimulus of the nerves was less.

† Medico-Chirurgical Transactions, vol. xxviii, p. 373.

§ Bull. de l'Académie Royale de Médecine, 15 Oct. 1845.

† L. c. p. 426.

Dr. Morganti\* has published a long and important monograph on the facial nerve. He insists on the distinction of its two roots, reckoning as one the small fasciculus, the *portio intermedia* of Wrisberg. He traces, as Malacarne did, the principal root into the substance of the medulla oblongata, in which it at once divides into two fasciculi, one ascending, the other transverse. The first of these fasciculi is composed of longitudinal fibres, which seem to descend from the lateral tract of the medulla oblongata into the nerve; the second, or transverse, is composed of more numerous fibres, and looks like the continuation of the principal root of the nerve; it passes through the substance of the lateral tract, and is directed to its posterior surface, towards the median line of the floor of the fourth ventricle, where it meets its fellow of the opposite side, and is covered by the expanded gray substance of the cord.

The smaller root, or *portio intermedia*, may be traced to the external part of the posterior [restiform?] tract of the medulla oblongata, in which it connects itself with the filaments of the vestibular branch of the auditory nerve, arising from the substance of the restiform body, where it combines to form the crus cerebelli.

By the aid of nitric acid and very careful dissection, Morganti has unravelled the communication between the trunk of the facial, this *portio intermedia*, and the vestibular division of the auditory nerve. The *portio intermedia* gives off a small branch, which unites with one from the vestibular, and in union with it enters the vestibular nerve. Then, next, it gives off two slender branches, which join the chief trunk of the facial, and cannot be traced further. Then an appearance of a plexus is produced by a branch being given off from this chief trunk, which branch crosses in front of the *portio intermedia*, and then winds behind it, looking just as if it were given to the *portio intermedia* and to the geniculate ganglion, but really passing them and rejoining the trunk. By separating this branch, it is shown that the geniculate ganglion is formed exclusively on the fibres of the *portio intermedia*, and has only a connexion of contiguity with the trunk of the facial. Further, it appears that the geniculate ganglion gives origin—1st, to the superficial petrosal nerves, (to the greater of which is added a small branch or two from the trunk of the facial); and, 2d, to two or three descending branches. These descending branches appear to join the knee-shaped bend of the trunk of the facial, but being unravelled they separate into—1st, numerous small branches forming a kind of plexus and then joining the main trunk of the facial; and, 2d, the chorda tympani, which the author holds, is thus formed from the *portio intermedia*, with the addition of one or two small branches from the trunk of the facial.

The author confirms this account by evidence from comparative anatomy; and he draws from the whole the conclusion that the geniculate ganglion (for the truly ganglionic nature of which he gives sufficient proofs) is to be classed with the ganglia on the posterior roots of the spinal nerves. In accordance with this view also, he holds that the facial is a mixed or double-rooted nerve, analogous to the fifth and spinal nerves; that the trunk (as it is here called) is its anterior root, and the *portio intermedia* its posterior root; that the superficial petrosal nerves are mixed branches, chiefly composed of fibres going from the facial to the spheno-palatine and otic ganglia; the chorda tympani, a mixed branch from the facial to the lingual branch of the fifth; and that, after the formation of the geniculate ganglion, the trunk and branches of the facial contain both sensitive and motor fibres continued from its roots.

The author opposes, in detail, all the objections to these views; and in answer to the belief that the trunk of the facial is insensible till it is joined by branches from the pneumogastric and fifth, and that the chorda tympani is

\* Annali Universali di Medicina; Giugno, 1845.

composed of motor fibres exclusively, he adduces experiments. In many instances he divided the trunk of the facial directly after its exit from the stylo-mastoid foramen, (at least two inches further back than Panizza did); and in every one, acute pain was produced both by the division and by the irritation of the central portion of the trunk. The experiment was performed on horses, asses, dogs, and a sheep; and he shows reason for believing that the pain was not due to anastomoses of the fifth with the facial previous to its exit from the skull; though he admits that some of it might be due to the anastomosis of the auricular branch of the pneumogastric. In many other experiments he irritated the chorda tympani, (which he exposed through the posterior wall of the tympanum,) and every time he did so the animal gave signs of pain.

M. C. Bernard\* has added four cases of paralysis of the facial nerve, attended by impairment of taste, to those five cases which he published in 1843.† Dr. Guarini's experiments, showing that this is the motor nerve of the lingualis muscle, were noticed in the report for 1842-3. M. Bernard‡ showed the same by experiments at nearly the same time as Dr. Guarini; and Dr. Verga§ corroborated them somewhat later: so that this fact may be considered certain. To prove the influence which the chorda tympani thus distributed in the lingualis muscle exercises upon the sense of taste, M. Bernard adduces these nine cases of disease of one of the facial nerves in men, and many experiments on animals. In all these, the taste of the corresponding side of the tongue was impaired; the flavour of quinine, citric acid, and other strong substances was very slowly and slightly perceived, while on the other side of the tongue the perception of them was instant and acute. He shows that this impairment is from the paralysis of the chorda tympani, by cases in which the cause of paralysis of the facial nerve being seated in front of the giving off of the chorda tympani, the taste was not impaired; and by experiments in which the facial being divided behind the part where the chorda tympani is given off, the sense was destroyed. By this last experiment, in which no injury could be done to any filaments of the Vidian that may join the facial, and by other experiments, which show|| that the chorda tympani is insensible, he makes it clear that the influence which the chorda tympani has on the sense of taste cannot be due to branches of the fifth nerve transmitted by the Vidian to the facial. Neither can the loss of taste be due to the dryness of the side of the tongue, for it is as moist as the other side. His own explanation is, that the papillæ of the tongue are rendered incapable of the vermiform movement by which they absorb the sapid particles, and bring them into contact with the gustatory nerve [a notion at least as improbable as any that his facts disprove].

In none of these four cases by M. Bernard was there any deviation of the uvula. They may, therefore, seem to confirm the evidence from Dr. Hein's¶ experiments that the facial does not send branches to the muscles of the palate or uvula.

But it is not certain that in these cases the seat of the disease of the nerve was behind the connexion of the petrosal nerves with it; and, if it were not, the muscles of the palate might be unaffected, though all the others supplied by the facial were paralyzed. Notwithstanding these cases, therefore, and although Valentin\*\* also has failed to excite movements of the palate in either the horse, dog, cat, or rabbit, by irritating the trunk of the facial nerve, yet, on the whole, I think the evidence is in favour of the opinion that, at least in man, the facial nerve sends filaments through the great superficial petrosal

\* Archives Gén. de la Méd., Decembre, 1844.

† Annales Medico-physiologiques, Mai, 1843.

‡ In the Inaugural Thesis of M. Pomies, Paris, 1842.

§ Gazzetta Medica di Milano, Giugno 24, 1843.

|| According to Morganti, this is wrong; but the experiment is not essential to the conclusion.

¶ See last Report.

\*\* Lehrbuch der Physiologie, ii, p. 673.

nerve to the spheno-palatine ganglion, and thence to the levator palati and levator uvulæ.\* It is impossible otherwise to explain the deviation of the uvula, which is often seen when the facial nerve is paralysed. The absence of contraction of the palate-muscles when the nerve is irritated may be connected, as M. Longet suggests, with the filaments having to pass through a ganglion; in the same manner as irritation of the third nerve often fails to produce contraction of the iris.

*Pneumogastric Nerves.* Signor Poletti† shows that the removal of portions of both these nerves in the horse, sheep, and rabbit does not diminish the impression of the necessity of breathing. There is the same struggling for breath when the respiration is in any way hindered as there is while the nerves are entire; and if the animals, after division of the nerves, are put in an exhausted receiver, their anxiety for air is as great as when the nerves are not divided. In sheep, the glottis remains open though both the recurrent nerves are paralysed by the division of the pneumogastric nerves, and they breathe easily till the air-passages are obstructed by frothy fluid, or till the nostrils are purposely held close; then there ensues extreme dyspnœa. There were the same anxiety and dyspnœa when, after division of the pneumogastric nerves, the spinal cord in a rabbit was divided between the second and third cervical vertebræ; the diaphragm still acted with great energy. Poletti therefore supposes that, since in this last experiment nearly all the cerebro-spinal incident nerves were separated from the medulla oblongata, the sympathetic nerve must be an important agent in transmitting the impressions of the necessity of breathing.

From some experiments related by M. Dupuy‡ it would appear that in a few minutes after the ligature or division of the pneumogastric nerves, the blood of the carotid artery is black like venous blood, and contains less fibrine than it did before. If the life of the animal be prolonged for about six days by tracheotomy, the respiration becomes difficult, the signs of malignant pustule manifest themselves, and by placing portions of the spleen of such a horse in a wound made in a healthy horse, the disease may be communicated.

From many experiments on dogs, Dr. Mendelsohn§ deduces that the congestion and other disorders of the lungs which follow division of the trunk of the pneumogastric are due, not to the paralysis of the nerves of the lungs themselves, but to that of the branches of the recurrent on which the openness of the glottis depends; for the same affection ensues whether the recurrent or the pneumogastric trunks be divided. In both cases the glottis is nearly closed in inspiration, and the symptoms are the consequence of the diminished respiration. In regard to the diminished frequency of respiration after division of the pneumogastric, Dr. Mendelsohn says that the very same is observed after division of the recurrent nerves; and, in either case, may be prevented by the introduction of a tube into the trachea.

The continued irritation of this nerve by the magneto-electric current produces fixed cramp-like contraction of the œsophagus, and movements of alternate contraction and relaxation of the stomach. It follows, according to Volkmann's rules already mentioned, that the stimulus is conveyed to the œsophagus directly, through its motor fibres forming part of the trunk of the pneumogastric, and to the stomach indirectly by a reflected influence. Now, since the removal of the brain and spinal cord does not interfere with this

\* In addition to the evidence for this view in the last Report, p. 52, see the cases concerning the diagnosis of fracture of the base of the skull in the Bull. de Therapeutique, Mars, 1845; and a case of tuberculous disease of the petrous bone, with paralysis of the facial nerve and deviation of the uvula, in the Oesterr. Medic. Wochenschr., 11 Oct., 1845.

† Bullettino delle Scienze Mediche, Luglio, 1844.

‡ Bull. de l'Acad. de Medecine, 31 Oct., 1844.

§ Roser and Wunderlich's Archiv, 1845, Heft ii; and Oesterr. Medic. Wochenschr. 19 Juli, 1845.



result, it follows that the stomach must have reflecting nervous centres in itself, and that there must be fibres in the pneumogastric nerves which are centripetal in their relation to these local centres.

*Accessory Nerve.* Volkmann\* has repeated his experiments on the motor nerves of the larynx; and, in confirmation of his former statement and that of Dr. John Reid, says, that in numerous experiments he has never once seen movements of the muscles of the larynx follow irritation of the accessory nerves, but has as constantly seen them follow irritation of the pneumogastric. He has also completely divided all the roots of the accessory nerves in a dog, and has seen the movements of the larynx continue unchanged.

But after all the evidence adduced in the last two Reports in favour of the accessory being at least the motor nerve of some of the muscles of the larynx, I cannot think these experiments conclusive against it.

*Hypoglossal Nerve.* Dr. Stokes† has never, in many examinations, been able to find what Mayer described as the small posterior ganglionic root of the hypoglossal nerve in the ox. He has, however, found that it always has a small root derived from the side of the spinal cord anterior to the roots of the accessory nerve. From this origin the root passes up into the skull, and, without having any ganglion, is united to the lower of the two fasciculi in which the hypoglossal nerve passes through the dura mater.‡

#### ANATOMY AND PHYSIOLOGY OF THE ORGANS OF SPECIAL SENSE.

**OF THE EYE.** *Vessels and Nerves of the Cornea.* Professor Gaddi,§ of Modena, describes, from a minute injection of the vessels of a child two years old, the vessels of the conjunctiva bulbi as forming a dense circle by numerous angular anastomoses around the border of the cornea. From the angles of union, he says, some very minute vessels are given off, which pass like rays, or like the vessels of the membrana pupillaris towards the centre of the cornea; and, besides these, there pass, from the same angles, many more, which insinuate themselves between the commissure of the cornea and sclerotica, and thus form communications with the vessels of the choroid.

Purkinje,|| in the series of examinations already often quoted, has discerned, by making the cornea transparent in acetic acid, a "tolerably rich network of nerves" in it. These fibres variously combine with each other; those entering on one side from the ciliary nerves mingle with those from the other, so that no nerve-fibre appears to be lost in the substance of the cornea, or to pass into the superjacent conjunctiva.

*Structure and Movements of the Iris.* Some experiments of Signor Guarini¶ would make it probable not only that the movements of the iris are mainly due to two sets of muscular fibres,—one circular, for the contraction of the pupil, the

\* Wagner's Handwörterbuch der Physiologie, art. Nervenphysiologie, p. 500.

† Cycl. of Anatomy, art. Ninth Pair of Nerves.

‡ Other recent works of interest on the physiology of the nervous system are—1. J. H. Lenzinger, Die menschliche Nervensystem physiol. bearbeitet; Zurich, 1845, 12mo—Idealistic. 2. K. F. Burdach, Umriss einer Physiologie des Nervensystems; Leipzig, 1844, 8vo—Outlines. 3. Remak, Neurologische Erläuterungen; in Müller's Archiv. No. 5, 1844, containing, with controversial matter, brief accounts of small ganglia in the nerves of the heart, lungs, and larynx; of the six layers of the cortical substance of the brain; and of the central fasciculus in the nerve-tubules of the abdominal cord of the river cray-fish:—all according with former descriptions by the author. 4. Romberg, De paralyti Respiratoria; Berlin, 1845, containing the outlines of the physiology of the pneumogastric and its branches. 5. Cowan, Two cases of Carcinoma of the Brain, in the Provincial Med. and Surg. Journal, April 16, 1845. In one case loss of control over the muscles appeared to be owing to implication of the cerebellum: the patient, in walking, deviated to the left side, and the disease affected the left side of the cerebellum and pons, and the left crus cerebri. 6. Bourguery, Recherches sur le Système Nerveux Splanchnique: in the Gazette Médicale, 3, 10 Mai, 1846.

§ Bullettino delle Scienze Mediche, Nov. e Dicembre, 1844; Report from the Società Med. Chir. di Bologna, 30 Ottobre, 1844.

|| Müller's Archiv, 1845, No. iv. p. 292.

¶ Annali Univ. di Medicina, 1844, and Gazette Médicale, 26 Avril, 1845.



other radiating, for its dilatation,—but, also, that these movements are under the influence of two sets of nervous fibres, of which one set, distributed to the circular muscle, are derived from the third nerve through the ophthalmic ganglion, and the other, distributed to the radiating fibres, are supplied by the branches of the cervical spinal nerves which pass through the superior cervical ganglion of the sympathetic. The experiments show, as Valentin's did, that after division of the third nerve the pupil dilates, and that after removal of the superior cervical ganglion it contracts. In animals killed by strychnine the pupil is dilated; but if, before giving the strychnine, one of the superior cervical ganglia be removed, and the pupil of the same side be thus made to contract, it dilates only a little after the administration of the strychnine, while, in the same case, the pupil of the opposite side is extremely dilated. Again, if one of the third nerves or the ophthalmic ganglion be irritated in a body still irritable, the pupil of the corresponding eye contracts slowly and does not dilate again. But if, after the pupil of a live animal has contracted on irritating the third nerve, the superior cervical ganglion be irritated, the pupil dilates again.

But while these movements of the iris after death and on the irritation of the nerves indicate its muscular structure, Guarini relates another experiment to show that the turgescence of its vessels has also some share in producing or maintaining the contraction of the pupil. Thus, he says that the contraction of the pupil, when after death the third nerve is irritated, is never so complete as it is during life; because the circulation having ceased, the congestion of the blood-vessels with which the muscular fibres are interlaced, and which they compress when they contract, cannot take place; but the dilatation of the pupil on irritating the superior cervical ganglion after death is as complete as during life.

*Membrana Pigmenti.* C. Bruch\* describes the numerous round or oval nuclei among the pigment-cells scraped from the choroid, as forming a separate layer between the choroid and the cells of the pigment. Among those taken from the pig's eye, he found many fixed in a delicate, pellucid, structureless membrane; they lie, commonly, in close-set rows, or, more rarely, scattered in the membrane; and some, which have probably been accidentally separated, are found unattached. Bruch has traced this membrane in many animals, and in man, in whom it extends over the whole choroid, the ciliary ligament, and the posterior surface of the iris. Its nature and purpose are uncertain; whether it be only such a basement or germinal membrane as lies beneath the epithelium of mucous membrane in general, or a layer of embryo pigment-cells with cytoblastema, cannot be determined; but, if any part does, this deserves the name of *membrana pigmenti*.

*Tapetum.* Ernst. Brücke† has added observations to those which he published last year, to prove that the office of the tapetum is to reflect light, on the staff-shaped bodies of that part of the retina which is most used in vision, and so to make vision possible to certain animals, at times when those without a tapetum are in darkness. He has produced an extensive comparative anatomy of the tapetum; and has shown that while all the other various colours which the tapetum of the dog and other animals presents in different lights depend on it alone, the red colour which it occasionally reflects is due to the large vessels beneath it coming into view. And he has discovered that there are in different animals two distinct kinds of tapetum,—the one *fibrous*, the other *cellular*. The fibrous tapetum, which has been long known to consist of smooth, transparent, undulating fibres, is found in ruminants, solidungula, the elephant, some marsupials, and the whale tribe. The cellular tapetum, hitherto unknown, is found in the carnivora and in seals. The cells, which may be best seen after pieces of tapetum have been macerated for a day or two in water with some hydrochloric acid or alcohol, are smooth, nucleated, elongated, unequally and imperfectly hexagonal, and very large, measuring from  $\cdot 0006$  to  $\cdot 0028$  of an inch in diameter.

\* Untersuch. zur Kennt. des körnigen Pigments der Wirbelthiere; Zurich, 1844, 4to, p. 6.

† Muller's Archiv, 1845, Heft iv, p. 387.

With transmitted light they appear yellowish, and their nuclei pellucid; but with reflected light, they are beautifully blue and their nuclei black. They are peculiarly subject to deposits of earthy matter.

*Retina.* Pacini\* has described the retina as consisting of five layers. The first or most internal is formed of the expansion of the white double-contoured (?) fibres of the optic nerve, lying close together in a transparent layer. The second is a single layer of nucleated and nucleolated transparent corpuscles, like simple ganglion-corpuscles, which he calls nerve-cells. The third, of a reddish-yellow colour, consists of gray nerve-fibres arranged in rays, which terminate in the cells of the second layer. The fourth is composed of several layers of corpuscles, identical with those described by Ehrenberg in the cortical substance of the brain and in other parts of the nervous system, and called by Pacini, from their nuclear characters, nuclear nerve-corpuscles. The fifth layer is formed by the little cylinders of the membrane of Jacob [the staff-shaped bodies of others]. Of these layers, the 1st, 4th, and 5th cease at the ciliary ligament; but the 2d and the 3d, as if forming together a true ganglionic system, appear to be continued over the ciliary processes and iris.

*Lens.* Sundry observations on the structure of the lens are published by Professor Harting.† Among them are the following. In both the new-born child and the adult he finds some of its flat fibres composed of rows of cells fixed end to end; and some having in or on their walls distinct round or oval nuclei, each with two or three very small irregular nucleoli. Both these kinds of fibres are found only at the equator of the lens, in which part they form the outermost layer. Neither the deeper layers at this part, nor the superficial layers near the poles, present a trace of such fibres. The lens of the new-born child has many more of the fibres with nuclei than that of the adult has.

In the lens of a cow, just beneath the surface, and in that of a titmouse (*Parus major*), near its nucleus, he found some of the fibres composed of from five to seven finer parallel fibrils; and in other fibres near the surface of the lens of the titmouse, there were transverse striæ, which (like Wagner, who has also observed such fibres) he compares to those of the primitive fibres of muscle.

*Vitreous Humour.* Ernst Brücke‡ has confirmed the account which he gave of the laminated structure of the vitreous humour by another mode of demonstration. If an eye, frozen hard, be slowly thawed, and the sclerotica and other membranes be separated gently as soon as they are soft enough, lamellæ of ice may be split off from the vitreous humour, exposing the layers of membrane of which it consists, the outer layers being parallel to the retina, the inner to the posterior surface of the lens; and all apparently structureless.

A more exact account of the structure of the vitreous humour is rendered by Hannover.§ He has examined it principally and with most advantage in the eyes of horses, hardened in chromic acid, so that sections in various directions could be made through them. By these means he has cleared up a doubt which Brücke's essay left, by finding that the several layers which compose the vitreous humour all form completely closed sacs, the outer sacs inclosing the inner, and all imitating the general form of the front of the eye behind the lens. Thus, in its laminated arrangement, the vitreous humour somewhat resembles a bulb, and the shape of its layers or sets is such, that a line drawn from the middle of the optic nerve to the middle of the posterior wall of the lens would pass through the apices and the middles of the convex bases of them all.

On this same plan of concentric sacs of delicate membrane are formed the vitreous humours of the cat, dog, ox, sheep, and horse. But the human vitreous

\* Atti della sesta Riunione degli Scienzi. Italiani, in the Ann. Univ. di Medicina, Luglio, 1845.

† Tijdschrift voor Natuurk. Geschied. en Physiol., 1845, d. xii, st. 1. His account of the growth of the lens is mentioned under the head of Nutrition.

‡ Muller's Archiv, 1845, Hefte ii, iii. For his former account, see the last Report, p. 57.

§ Ibid 1845, Hefte v. There is in the same journal a paper by him on the arrangement of the fibres of the lens; but his description could not be understood without diagrams.

humour is differently constructed; it consists of sectors, all with their arcs directed outwards, and their angles converging to the axis of the eye. Its structure may be best compared with that of an orange, sections of which in various directions will display the various appearances which are seen in different sections of the human vitreous humour. In the axis of the eye, to which the angles of all the sectors of the vitreous humour converge, is the hyaloid canal, the passage which is open in the child for the arteria centralis retinae; but the angles do not reach this canal; the substance next to it is uniform and textureless, as if all the sectors had become very thin, and had coalesced. The number of rays which may be seen diverging from this central part, and which represent the number of sectors, is about 180; the width of the arc of each sector is about one sixth of a line. It is not certain whether each sector has its own walls, or whether one wall serves for two. Whichever it be, these walls may be described as lamellæ of pellucid structureless membrane, extending from the tunica hyaloidea in rays towards the axis of the eye, and forming a kind of membranous skeleton for the more fluid part of the vitreous humour.\*

*Relations of non-luminous Rays to the Eye.*—To determine why those rays which, after dispersion by the prism, are beyond the spectrum (the so-called chemical and calorific rays of light), do not produce on the eye the sensation of light, and to determine whether the retina is insensible to them, or they are hindered from reaching it, Ernst Brûcker† has instituted some simple and very interesting experiments. The colour obtained from guaiacum wood is changed to a dark bluish-green on exposure to diffused light, the change being effected by the chemical rays,—those beyond and just within the violet. But if part of a surface tinged with the original colour be so screened that the light can pass to it only through an ox's or other animal's lens, the colour of this part is hardly changed. A part similarly screened, but having light admitted to it through a cornea, is only a little more changed; by light admitted through vitreous humour, yet a little more; and if to this screened part the light be made to pass through both a cornea and a lens, then, while all the rest of the surface undergoes the usual complete change of colour, this remains often unchanged, and is at most changed in only the slightest degree; so that it appears that the transparent parts of the eye absorb the chemical rays of light before they can reach the retina, and that therefore they are invisible.

Another set of experiments was directed to determine the relation of the calorific rays (those beyond and just within the red) to the same parts of the eye. These also appeared to be completely arrested; rays of light passed freely through the cornea and lens, and fell upon a thermo-electric apparatus; but its needle remained immoveable, as if the whole of the heating rays were absorbed.

**EAR.** *Movements of the Ossicula.* Eduard Weber‡ has found that the articulation of the head of the malleus with the incus is such, that when the malleus is moved with the membrana tympani, it cannot move upon the incus; but the two bones always move together, as if they were one bone. Their axis of movement is the line drawn from the slender process of the malleus to the lesser process of the incus, so that the membrana tympani impresses the movement on the malleus and incus, and these turn, as on a pivot, on the two processes adhering to the wall of the tympanum. Thus, when the membrana tympani is carried inwards, the stapes is pushed within the fenestra ovalis by the long process of the incus; and when the membrana tympani is carried outwards, the stapes is moved out from the fenestra ovalis.

\* Hannover's attention was first directed to this structure of the human vitreous humour by a morbid examination of the appearance of its structure in two congenital cases of colobama, which are described in the same number of Muller's Archiv., p. 482.

† Muller's Archiv, Hefte ii, iii, 1845.

‡ Archives de Physiologie, Janvier, 1846; Report from the Naples Congress, 1845.

Hence appears the utility of the fenestra rotunda for the movements of the stapes. For the stapes could not thus move if the cavity of the internal ear was bounded by unyielding walls, since the fluid within it is hardly compressible. But as the fluid of the vestibule communicates with that of the cochlea, especially with that of the scala vestibuli, which again freely communicates with that of the scala tympani, so when the membrane of the fenestra ovalis is pushed inwards towards the vestibule, the membrane of the fenestra rotunda will be pushed outwards; as it is always seen to be after death in such a case. And thus the movements of the membrana tympani produce indirectly the flux and reflux of the fluid of the labyrinth, from the fenestra ovalis to the fenestra rotunda, by percussion and by the yielding of the lamina spiralis of the cochlea.

**TONGUE. Structure of its Papillæ.** A new and very interesting account of the papillæ of the tongue is given by Dr. Todd and Mr. Bowman.\* They have found that all those hitherto described as simple papillæ, appearing different by being variously grouped, are really compound organs; i. e. they are papillæ clothed with secondary, simple, and much more minute papillæ, which are concealed under the epithelial investment, and are scarcely or not at all visible till it is removed. They have also discovered minute simple papillæ, hitherto unknown, which are unequally dispersed among the compound ones, and occupy much of the space at the base of the tongue behind the circumvallate papillæ. The epithelium covering these makes the surface appear quite smooth; but when it is removed, they are seen prominent, each having an envelope of basement membrane, and receiving a capillary loop like those which pass into the simple papillæ of the skin. Papillæ like these compose the papillæ circumvallatæ. The surface of the fungiform papillæ is clothed with secondary simple papillæ, each of which receives a capillary loop from the more complex arrangement of vessels within the primary papillæ. The filiform and conical papillæ also are compound, being variously beset with fine acuminate secondary papillæ, more stiff and elastic than the others. The epithelium, also, of these papillæ (while that of the fungiform papillæ is very thin) is so thick and dense, that they appear white; it is also sent off from their surface in many fine hair-like processes, more or less stiff, or is arranged in variously imbricated scales, within which there are in some cases real minute hairs† pointed at the end, and having extremely fine central canals. Their structure sufficiently indicates that these papillæ are subservient to mastication rather than to taste.

#### STRUCTURE AND FUNCTIONS OF THE ORGANS OF GENERATION.

**Nerves of the Male Genital Organs.** According to Purkinje,‡ fine-fibred nerves existed in the tunica albuginea in the neighbourhood of the epididymis. Many such, also, accompany the arteries of the penis. A rich network of large cerebro-spinal nerve-fibres exists in the fibrous tissue of the penis; and fine-fibred nerves with little ganglia may be found, but with difficulty, in its spongy tissue.

**Processus Vaginalis in the Female.** Professor H. Meyer§ has described, and confirmed therein the accounts of Camper and some others of the older anatomists, the processus vaginalis of the peritoneum in the female fœtus. Its position and arrangement are exactly analogous to those which it has in the male.

**Maturation of Ova; Menstruation.** Two cases of apparent discharge of ova at the menstrual period are recorded by M. Serres.|| The ovum could not be seen in either, but in both, Graafian vesicles were found recently burst and very vascular.

\* *Physiological Anatomy and Physiology*, vol. i., p. 435. The beautiful figures illustrating these structures describe them better than words can.

† The existence of these minute hairs guides at once to the explanation of the seemingly strange cases in which large tufts or coats of hair have been found on the tongue. See some such cases recently related by M. Landouzy in the *Bull. de l'Acad. Roy. de Médecine*, 15-30 Dec. 1845.

‡ *Müller's Archiv*, 1845, Heft iv., p. 293.

§ *Ibid.* Heft iv., p. 363.

|| *Comptes Rendus*, 18 Novembre, 1844.

Dr. Schweig\* having ascertained in 60 women the length of interval between 500 occurrences of menstruation, finds that the mean length of the interval is 27.39 days; and that therefore the periodicity of menstruation corresponds, not with the phases of the moon, (for the lunar month marked by these is, on an average, 29.53 days); but with the nodes of the moon, the period from one of these to another of the same kind being 27.56† days.

Dr. Guy's‡ tables, made from observations of 1500 women, show that 15 years is the age at which the greatest number of women (English, I suppose) first menstruate; that the 14th year comes next in order, then the 16th, and that the 13th and 17th, 12th and 18th, 11th and 19th, present numbers which are successively less, but respectively nearly equal. Of the 1500, 791 first menstruated at 14, 15, or 16. Of 400 women, 159 ceased to menstruate between 45 and 50; 140 between 40 and 45; 51 between 35 and 40; 41 above 50; and 9 under 35. As a general rule, also, it appeared that the earlier menstruation begins the longer it lasts.

Mr. Robertson,§ continuing his researches into the question of the age at which women in different climates begin to menstruate, has obtained information from Moravian missionaries on the coast of Labrador, which proves that among the Esquimaux of that region the average period of first menstruation is the same as in this country. The fecundity of the Esquimaux women is also shown to be not less, but considerably greater, than that of the average of English married women; neither, since the introduction of Christianity among them, have early marriages been frequent.

He has also obtained tables|| showing the average ages of puberty and first conception among Hindoo women. Among 71 at and near Bangalore, the average age at which menstruation first took place was 13 years and 2 months; the average age at the birth of the first child was 16 years and 5 months. Similar tables have been constructed for native women of Bengal, living in and near Calcutta, by Bábu Modusooden Gupta and Dwarkanath-das Bosu, the Demonstrator and Curator of the Calcutta Medical College.¶ In 152 women the average age at first menstruation was 12.48 years; two of these first menstruated when 8 years old; 2 when 9 years old; 2 when 17; 1 when 16. In 93 of these women the average age at which the first child was born was 14 years and 8 months; in 1 case only at 10; in 6 at 11 years; in 1 at 30; in 1 at 19; in 2 at 18. Yet the general result of the inquiries, though they show so early a period of puberty, is not wholly unfavorable to the opinion of Mr. Robertson, that the early puberty is the consequence of early sexual excitement; for girls are always given in marriage before their first menstruation, and it is customary to send them at as early an age as nine years to the houses of their husbands; and it is stated that if distance hinders this, menstruation is generally delayed till the 13th year. Some analogous instances of early fecundity brought on in animals by premature sexual intercourse are mentioned by Dr. Berthold.\*\* Among them is one of a kid only fourteen days old which was impregnated by an adult goat; at the end of the usual period of gestation it bore a kid, mature, though

\* Gazette Médicale, 2 Août, 1845; from Roser and Wunderlich's Archiv für Phys. Heilkunde, Heft ii, 1845.

† So it is said in the original; but this period of 27.56 days is more nearly equal to the length of the anomalistic month of 27.5546 days, marked by the successive revolutions of the moon from perigee to perigee. The average interval between two menstrual periods, 27.39 days, is more nearly equal to the average length of the sidereal month, or 27.3216 days, in which the moon makes the complete circuit of the heavens. The true length of the nodical month is not 27.56 days, but 27.213 days. See Baily, Astronomical Tables.

‡ Medical Times, Aug. 9, 1845.

§ Edinburgh Med. and Surg. Journ., Jan. 1845.

|| Ibid. October, 1845.

¶ India Journal of Med. and Phys. Science, February, 1845.

\*\* Ueber das Gesetz der Schwangerschaftsdauer; Göttingen, 1844, p. 32. The accounts of the physiology of menstruation, by Raciborski and others, are examined by Dr. Alexander, in Oppenheim's Zeitschrift f. d. ges. Medicin, Dec. 1844.



very weak, to which it gave milk, and which grew up healthy and strong. The young mother gave more than milk enough for its kid, grew up to full size and strength, and twice afterwards had kids.

*Processes following the Discharge of Ova; formation of Corpora Lutea.* Dr. Dubini\* has carefully examined the ovaries in the bodies of many women, and has found—1. That in those who have never menstruated the ovaries have never shown any trace of cicatrices; but in those who have menstruated, cicatrices were never absent. 2. That in a few cases the ovaries of women who had been recently [but how recently?] delivered presented no corpus luteum. 3. That the number of the so-called old corpora lutea, i. e. of vesicles with thick opaque white walls, and deeply wrinkled, which are found in the ovaries of different women, bears no relation to the number of children they have borne.

M. Raciborski† states that in regard to the formation of these bodies, which he supposes to consist in the “concentric hypertrophy” of the internal or granular membrane of the Graafian vesicle, there are characteristic differences between the females of the human species and of other mammalia. In the cow, ewe, doe, &c., the corpora lutea begin to be formed directly after the expulsion of ova, and they are just the same, whether the ova are impregnated or not; they are always genuine true corpora lutea. But in women, if the ovum expelled at any menstrual period is not impregnated, the *hypertrophy* of the internal layer of the vesicle is soon arrested, and it remains a thin yellow membrane in contact with the more or less altered clot of blood. If it is impregnated, the consequent process is different, but in degree only, not in kind; in this case the hypertrophy goes on, till very shortly the cavity of the vesicle is almost completely filled by the accumulated substance. And in this admission of the differences between the corpora lutea, according as the ova are impregnated or not, Berthold‡ agrees; holding, I think, that the purpose of the development of the true corpus luteum is that a process may be going on in the ovary which may exclude the ordinary maturation and discharge of ova during gestation.

Some elucidation of what is thus roughly, though correctly, described as hypertrophy of the inner membrane of the Graafian vesicle, is afforded by the inaugural dissertation of Dr. Zwicky,§ who has minutely examined the formation of corpora lutea in cows and sows. The general results of his investigations may be thus briefly stated:—at the period of heat, when the ova are about to be discharged, the Graafian vesicle becomes more vascular and enlarges; serum is more abundantly secreted into it, and its membranes thicken. The cells also, part of which form the granular membrane of the Graafian vesicle, while part float in its fluid, are changed before the exit of the ovum. Flocculi appear in the fluid, and vascular folds and villi on the inner surface of the vesicle, which all consist of the altered cells. Some of the cells are changed by elongation into small narrow fibrous cells, which at last become fibres of imperfect fibro-cellular tissue; and others are changed into larger round or ovate cells, which also, unless they burst, are ultimately changed into similar fibres. And again, some other cells become four, or five, or even ten times as large as they were, and very finely granular; their nuclei also become larger and clearer, and their nucleoli more evident. Their enlargement appears to depend on the accumulation of the little fat-granules which the Graafian vesicles always contain. As they increase they pass from the round or ovate, into the oblong or acuminate form. The fat-granules and other fatty particles are those in which the yellow colour of the corpus luteum, when it exists, is contained.

When the ovum is discharged through the ruptured Graafian vesicle, blood is

\* *Annali Univ. di Medicina*, Febr., 1845, p. 277.

† Report from the Acad. des Sciences; *Gazette Médicale*, 23 Nov. 1844.

‡ Ueber das Gesetz des Schwangerschaftsdauer; Göttingen, 1844, p. 17.

§ De Corporum Luteorum origine atque transformatione; Turici, 1844, 8vo.



effused into the cavity, in the sow, but not, according to Zwicky, in the cow. The blood about half fills the enlarged cavity. After it has coagulated, it becomes gradually more solid, being compressed by the wall of the vesicle, which is both growing thicker and contracting. Thus compressed, and encroached on by the increasing and enlarging cells of the interior wall of the vesicle, the clot at last disappears, having, it would seem, contributed little to the formation of the corpus luteum.

The wall of the vesicle, after the exit of the ovum, is from half a line to two thirds of a line in thickness, and it continues to increase inwards by a continuance of the same process of multiplication and enlargement of its cells as preceded the discharge of the ovum, till, at length, the cavity of the follicle is completely filled. While it is thus increasing also, one portion of its fibrous cells are combined in fasciculi of connective or fibro-cellular tissue, which, traversing the whole mass, hold it together, and support the blood-vessels which run through it; and another portion are placed promiscuously between and among the large round cells. As soon as by this growth of the inner membrane of the Graafian vesicle its cavity is filled, its period of involution or degeneration begins; it grows smaller, more solid, and dries. In this change, some of its large round or ovate cells become narrow, and assume the appearance of fibres; and the longest, which are not thus changed, burst, and probably are absorbed with their contents.

The corpus luteum in this process of involution consists of only fibrous cells in various stages of development, and a large abundance of fatty matter. The fibrous cells become narrower, and their nuclei being drawn out, the whole gradually puts on the appearance of uniform slender cylindrical fibres, like those of immature fibro-cellular tissue. By these the walls of the Graafian vesicle are thickened, or else, as in cows, the fibres after a time coalesce with the stroma of the ovary, and can no longer be separated from it. And the end of the corpus luteum is, not that it is absorbed, but that it is lost sight of when this transformation of its component cells into fibrous tissue, like that of the stroma of the ovary, is complete.

*Gestation.* Dr. Berthold\* estimates that parturition after healthy gestation takes place when the ovary, having omitted during pregnancy the maturation of nine menstrual ova, is preparing for the maturation and discharge of what, in the unimpregnated state, would have been the tenth. The proper period of gestation† will thus be equal to the interval comprised between the beginnings of ten menstruations, *minus* from ten to fourteen days. The above-named interval will not be always the same, while in different women, and in the same, under different circumstances, the intervals between the successive menstruations vary; but it may be estimated as equal to the number of days which intervened between the beginnings of the ten menstruations before the pregnancy. On an average this period will be between 290 and 300 days; the ordinary period of gestation will therefore be between 280 and 290 days; but it may be as short as 273, or as long as 291 days, in those whose habitual periods are shorter or longer than usual. In those whose menstrual periods are very short, parturition may take place at the eleventh instead of the tenth period.

Dr. Berthold's evidence for his mode of calculation is, 1st,—in a few cases in which it has proved true; 2d, in an apparent analogy in the case of cows and

\* Ueber das Gesetz der Schwangerschaftsdauer; Göttingen, 1844.

† This expression is not correct, because gestation is not begun till impregnation has taken place. The calculations are here made as if gestation began on the first day of a menstruation; but impregnation may take place some days (probably not less than twelve) after that day, or perhaps a short time before it. If, however, this mode of calculation is so correct, that the knowledge of the day of impregnation is unimportant, we must certainly admit Dr. Berthold's theory, that parturition is determined, not by the age or state of the child, or by the state of the uterus, but by that of the ovary, when it is reassuming its periodical action after its inaction of nine of its ordinary periods; a theory supported by the fact that, in pregnant animals, removal of the ovaries always produces abortion, but great injury of the tubes has no such effect.

sheep, in whom parturition occurs a little before the day on which the tenth heat-period would have fallen, if they had not been impregnated; 3d, in more distant analogies of some other periodic processes, such as the shedding of teeth and horns; 4th, the possibility of impregnation taking place in both women and animals within a few days after delivery; the ovum thus impregnated being probably that for the maturing of which the ovary was in preparation when parturition ensued; 5th, the apparently similar condition of the ovary shortly after delivery and shortly before the time of heat or menstruation.

*Lactation.* M. Dumas\* has observed a constant difference between the milks of the truly carnivorous and the herbivorous animals, in that in the former the sugar of milk is either absent or exists in quantity too small to be abstracted. But as soon as carnivorous animals (as bitches) are fed with bread or other amylaceous food, sugar is formed in the milk; and it disappears when their food is again made exclusively of animal principles. The casein of the milk of bitches is similar to that from the herbivora.

According to Dr. John Davy,† the coagulability by heat of the colostrum of the cow is due, not to its containing albumen, but to a peculiar modification of casein in it. Its nutritive matter is more concentrated, it is more easily coagulated by rennet, and it is less readily changed by the action of atmospheric air than the later milk is. The human colostrum (according to the examination of a few specimens) is not peculiarly rich in nutritive matter, neither does it coagulate on being heated.

\* Report from the Acad. des Sciences, Gazette Médicale, 4 Oct. 1845; and in the Ann. des Sciences Naturelles, Sept. 1845.

† Medical Gazette, April 18, 1845; and Medico-Chirurgical Transactions, vol. xxviii, 1845.

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2. A Practical Manual, containing a Description of the general Chemical and Microscopical Characters of the Blood, or Secretions of the Human Body. Part II. By J. W. Griffith, M.D., F.L.S. London, 1846. 8vo, pp. 164. 5s.
3. Remarks on the Dysentery and Hepatitis of India. By E. A. Parkes, M.B. London, 1846. 8vo, pp. 271.
4. Remarks upon Medical Organization and Reform, Foreign and English. By E. Lee. London, 1846. 8vo, pp. 121. 4s. 6d.
5. The Mineral Waters of Kreutznach. By J. E. P. Prieger, M.D. Lond. 1846. 8vo, pp. 92. 3s.
6. Notes and Recollections of a Professional Life. By the late W. Fergusson, M.D. Edited by his son, James Fergusson. London, 1846. 8vo, pp. 248.
7. Flora Capensis. Contributions to the Botany and Topography of Gibraltar. By E. F. Kelaart, F.L.S., F.G.S. Lond. 1846. 8vo, pp. 219.
8. Moral Philosophy; or, the Duties of Man considered in his Individual, Domestic, and Social Qualities. By George Combe. Third Edition. Edinburgh, 1846. Royal 8vo, pp. 116. Double columns, 2s.
9. Outlines of the Course of Qualitative Analysis followed in the Giessen Laboratory. By H. Will. London, 1846. 8vo, pp. 103.
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14. Elements of the Theory and Practice of Medicine, designed for the use of Students and Junior Practitioners. By George Gregory, M.D. Sixth Edition. London, 1846. 8vo, pp. 799. 16s.
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THE  
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PART FIRST.

Analptical and Critical Reviews.

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ART. I.

*The Nature and Treatment of Cancer.* By W. H. WALSH, M. D., Professor of Pathological Anatomy in University College, Physician to University College Hospital, and to the Hospital for Consumption and Diseases of the Chest.—*London*, 1845. 8vo, pp. 590.

WE proceed with our analysis of the second division of this valuable work, in which the disease is described as it affects various organs, tissues, and parts.

CANCER OF THE DIGESTIVE ORGANS AND APPENDAGES.

1. *Lips.* Labial cancer, without being very common, is of tolerable frequency. There is some variety in its mode of origin and progress. It may commence by infiltration of scirrhus in a limited portion of the lip, underneath the skin, at its junction with the mucous membrane. After a time one of these tissues gives way, leaving a fissure which discharges a thin acrid fluid that dries into a scab. When this falls, or is torn off, it is reproduced, until, as the ulceration advances, no more regular scabs are produced, but fungous granulations spring up from the scirrhus basis. In some cases, the first symptom is said to be a fissure, which resists all treatment. It has been known to commence as a cutaneous warty excrescence, or, as described by Dr. Warren, as a pustule; and it is most certain that venereal ulcerations of this part sometimes become cancerous, though the frequency of such an event has been much exaggerated. But, however commencing, as the disease advances, the skin, mucous membranes, and labial glands form a prominent mass, which, combined with the fungous productions from the sore, produce an abundant salver that constantly escapes over the lip, and irritates or excoriates the neighbouring skin. The ulceration may spread over the entire cheek, and even part of the external ear. It may affect the maxilla, and has even been known to reach the sternum. The salivary and lymphatic glands become enlarged

and painful, and, especially the latter, may partake of the diseased action. General contamination of the system is comparatively rare. Scirrhus is the usual form of cancer in this part; encephaloid has rarely, if ever, been observed. It is excessively rare in females; and is almost invariably seated in the lower lip. The only affection with which it is likely to be confounded is *venereal ulceration with indurated base*; in many cases the history is the only diagnostic guide.

When the ulceration is superficial, it may sometimes be successfully treated by caustics, the acid nitrate of mercury and the chloride of zinc being the best. It is generally supposed that excision is more successful here than in other parts; but there has been much exaggeration on this point.

2. *Gums*. The term *epulis* is applied to all solid growths from the gingival membrane of either jaw. Cancerous epulis is generally of the scirrhus species. It is more common in the lower than the upper jaw, and usually appears at the reflexion of the membrane on the alveolus, towards the last incisor or bicuspid teeth. It commences by infiltration; but a tuberculated nodule, sometimes with very deep anfractuositities, soon results. It generally varies in size between that of a pea and a nut, but has been said to attain the bulk of a walnut. Ulceration eventually takes place, the sore fungates and sometimes bleeds, and the discharge is peculiarly fetid. The disease destroys the soft parts, and may, but rarely, affect the bone. It is most common in advanced age and in males. There is usually lancinating pain. It is distinguished from the flabby, fungous granulations which sometimes form in the gums, near carious teeth, by its density, its tuberculated shape, and pale colour; and from erectile epulis, by the pulsating characters of the latter. From simple epulis it can scarcely be known, excepting from the result of operation.

3. *Tongue*. Lingual cancer is tolerably frequent. Out of 8289 fatal cases of cancer, reported in the Paris registers, in 36 the main disease was seated in the tongue. Scirrhus is the usual form; but the fungous excrescences sometimes exhibit the encephaloid character. It may commence as a small, somewhat knotty, and irregular tumour, which is generally seated in the anterior part of the organ, midway between the raphe and the edge, and rarely extends beyond the middle line; or the first appearance is a small excrescence, which in some instances is said to become pedunculated. In a few rare cases simple ulcers become cancerous; and still more rarely the morbid matter is deposited in erectile tumours. In either case the surface ulcerates, and the glands become affected in the usual way. Distant organs are not commonly implicated. Acute pain is not a constant symptom; but there is generally an aching sensation, with occasional sharp pangs, darting towards the ear. One of the most distressing concomitants is the necessity for incessant sputation. There are, of course, pain and difficulty in speaking, mastication, and deglutition. These increase with the progress of the disease, until the performance of these functions becomes impossible. The cachexia is frequently intensely marked, and the sufferer is commonly cut off by its progress. According to Mr. Travers, strong and healthy males, æt. 40 and upwards, are the most usual subjects; but the disease is not unfrequently observed in females, and much younger persons.

Lingual cancer may be confounded with *simple induration* of one side of the base of the tongue. The situation and absence of constitutional symptoms will distinguish between the two. Mr. Travers describes a *globular tumour* seated deeply in the organ, which is recognized by its uniform and unyielding surface, and by its disappearance under the use of alkalies and tonics. *Simple ulceration*, produced by mechanical irritation, is usually known by its rapid cure after removal of the cause. *Syphilitic ulcers* are less easily distinguished; they are generally larger, and have less marked and less circumscribed surrounding hardness. The discharge is also less abundant, and they are deficient in the firm everted edge and sprouting fungi. In many cases the history is our only guide. The *fissured* or *dyspeptic ulcer*, which often originates in psoriasis, may bear a close resemblance to cancer, but it has not the hard basis, is often in the middle line, and the rest of the tongue is chapped and irregular. Dr. Warren describes an *enlargement of the mucous glands*, occurring on the side of the tongue, with a red fungous appearance; but differing from cancer in being sensitive, not painful, and unattended by real ulceration or thickening of the organ. *Hypertrophy of the mucous membrane* sometimes gives rise to irregular fissured elevations on the surface. *Hypertrophy of the tongue generally*, a most rare malady, can scarcely be confounded with cancer. *Erectile tumours* are known by their pulsation; but it must be remembered that they may become the seat of cancerous deposits.

In regard to treatment little can be said. Ablation may be practised to avert impending death; but as a curative measure there is no evidence in its favour.

We pass over the description of cancer of the hard and soft palate, the tonsils, and salivary glands, but must say a few words on *pharyngeal carcinoma*. This is rare. Four out of the 8298 cases in the Paris registers were of this kind. The infiltrated form of scirrhus is the most usual species. The disease first presents itself as a hard, imperfectly circumscribed mass, situated in the submucous tissue, and not painful on pressure. Eventually it ulcerates, and passes through the ordinary stages. The symptoms are dysphagia, slight at first, but gradually increasing until deglutition becomes impossible; painful regurgitation of the discharges, and, if the larynx become involved, dyspnea. Hemorrhage is sometimes the first symptom. The patient commonly dies of inanition; and the treatment can be merely palliative. The diagnosis is not always easy. *Simple induration*, with thickening of the submucous tissue, will occasion the same dysphagia, and can only be distinguished by the result of treatment and by the progress of the disease. *Sacculation of the walls of the pharynx*, which also produces some of the symptoms, may be known by the facility with which instruments can generally be passed. *Venereal ulcerations* will be recognised by the history; *polypi* by their form, which is never assumed by cancer in this place; and *enlargement of the cervical vertebrae* by manual examination.

*Œsophagus*. The registers above noticed give 13 deaths from cancer of this organ. It is most frequently seated in the upper end of the canal behind the larynx, most rarely at its middle part, but it may occupy the whole extent of the tube. Infiltrated scirrhus deposited in a stratiform

manner is the usual condition, and it generally commences in the sub-mucous tissue. Distinct scirrhus tumours are comparatively very uncommon; and tuberiform encephaloid still more so. Distant organs are rarely implicated. The *nature* of the symptoms is the same as when the disease is situated in the pharynx, but as the stricture is lower down, the first stages of deglutition are performed naturally. Subsequently, if the upper end is affected, the greater proportion of each mouthful of food at once regurgitates; if a lower part, three or four mouthfuls pass easily, and the accumulated matter is then gently expelled by the action of the muscular fibres. These occurrences are at first only occasional, but eventually the dysphagia becomes permanent. It is necessary, however, to bear in mind that the disease may be intermittent throughout. The œsophagus sometimes forms a large pouch above the stricture, in which the ingesta accumulate; and here, according to Mr. Purton, they undergo a species of chymification. The progress of the disease is accompanied with pain, and obstinate hiccup is not uncommon. Spasmodic dysphagia is a frequent complication, and when some of the food passes into the stomach, it is often rejected by vomiting. The cachexia is commonly marked, and the patient dies of inanition. There is also danger of severe hemorrhage. Perforation may also take place, with effusion into the mediastinum, larynx, trachea, bronchi, pleura, or pulmonary tissue. The diseases stimulating carcinoma of this tube are *spasmodic stricture* and *spasm of the diaphragm*, to be recognized by the history, and the result of treatment; *paralysis* of the muscular coat, known by the easy passage of instruments, and the fact that large masses are more easily swallowed than small ones; *dilatation* and *sacculation* of the tube, also to be diagnosed by the probang; *hypertrophy* and *induration* caused by inflammation, this is generally the result of the ingestion of poisons, or the lodgment of some mechanical irritant, and the history is therefore our guide. Enlarged glands, abscesses, aneurisms, &c., may also cause great obstruction, but a careful examination will prevent error. The treatment can be merely palliative; but as some organic strictures have been cured by persevering dilatation, it will be well to give this plan a trial, and probably Dr. James Arnott's system of fluid pressure would be the best method. Leeching the throat often gives great relief, and benefit is sometimes derived from the application of the lunar caustic, to alter the condition of the sore, when ulceration has taken place. The œsophagus tube may be necessary, and if the disease is situated at the upper part, life might be prolonged by making an opening below the obstruction.

*Stomach.* The mortality from cancer of the stomach is very considerable. In this respect it yields to no organ but the uterus. Of the 8289 deaths in Paris, 2303 are referred to the stomach. In 67 cases, MM. Herrick and Popp found this organ diseased in 19. Cancer of the stomach may exist alone, but is usually associated with similar affections of other organs, more especially the liver. It is almost invariably primary. All the varieties of the three species of carcinoma occur in this organ; it is the special site of colloid cancer; and the milt-like variety of encephaloid is more common here than elsewhere. Infiltration is essentially the mode of deposition in this place, and, indeed, throughout the alimentary canal; and the seat of the deposit is the submucous cellular tissue, though



the mucous membrane, particularly when hypertrophied, may become a nidus of formation. The pylorus is the part most commonly affected; next the cardiac orifice; then, the greater, and, lastly, the lesser curvature. Dr. Walshe has not met with an instance in which the disease was limited to the fundus—a fact of importance as bearing upon the supposed origin of the disease from the ingestion of irritants. Three-fourths, or even more, of the organ may be disorganized; and this is particularly the case when colloid is the species.

The mucous membrane long resists the disease, and its chief tendency is to become irregularly hypertrophous, giving rise to the apparent formations of vegetations, &c. The cellular structure undergoes very marked thickening, and this occurring between the muscular fibres produces the striated appearance so commonly observed. The muscular coat of the sound parts of the organ is often enormously hypertrophied. The peritoneal coat is seldom affected, excepting in cases of colloid.

The size of the organ varies extremely. When the pylorus is affected it is often greatly enlarged; when the cardiac orifice, it is contracted; when the body of the organ is alone diseased, the general bulk commonly remains unchanged. It is a curious and unexplained fact, that, where the pyloric orifice undergoes dilatation, the walls of the stomach become hypertrophous.

The progress of the disease presents nothing peculiar. Ulceration is slow to commence; but when once begun, its ravages are extensive. In most cases, adhesions, especially to the liver and pancreas, take place before the peritoneum gives way.

The disease is more common in males than in females, and between the ages of 35 and 60. It is often hereditary, and has apparently, in many cases, been induced by mental distress.

Cancer of the stomach, in the early stages especially, may be confounded with other affections; the most practically important of which are *gastrodynia* and *chronic gastritis*. We subjoin the following sketch of the chief points of distinction:

<i>Gastrodynia.</i>	<i>Chronic Gastritis.</i>	<i>Gastric Cancer (early period.)</i>
Tongue variable; but often pale, and pitted at the edges.	Tongue dry, red, contracted, smooth, shining, or saburral.	Tongue pale or natural.
Eruclation frequent of air, without disagreeable smell.	Eruclation not a prominent symptom.	Eruclation of air more or less fetid, sometimes horribly so, a prominent symptom.
Appetite depraved, irregular, capricious.		Appetite diminished, or even totally suppressed.
Sensations, sometimes of heat, sometimes of cold in stomach; thirst not common.	Sensation of heat in stomach; thirst.	These symptoms not observed.
Solids more easily digested than liquids.		Liquids more easily digested than solids.
Digestion completed, though with much labour and suffering.	Digestion imperfectly completed.	Digestion not properly effected.
Pain variable, occurs in irregular paroxysms; is often relieved by ingestion of food or pressure.	Epigastric pain not very severe, and scarcely ever felt when the stomach is empty; increased by pressure.	Epigastric pain may be agonizing; the lancinating character sometimes marked; often increased by pressure.
Epigastric pulsation not uncommon.	Is not observed.	Is not observed.
Never runs a completely latent course.	Never completely latent.	May for a variable time be completely latent.

**Gastrodynia.**

Chronic vomiting is most frequent in females, and is almost confined to persons affected with hysteria.

Vomiting of coffee-ground-looking matter does not occur, unless from accidental and rare hæmatemesis.

Bowels generally constipated, but not obstinately so.

Febrile action accidental and rare.

In females the chlorotic tint is often present.

Often accompanied with various nervous or hysterical symptoms.

Hypochondria occasionally present.

Is more frequent than the other two.

Is more common in women than men.

May exist in very young persons (e. g. æt. 15).

Is often hereditary.

Is rarely referrible to any distinct local exciting cause.

Is relieved or cured by stimulant, tonic, and anodyne treatment.

**Chronic Gastritis.**

Vomiting of sudden and severe character sometimes the very first symptom; occurs irregularly before or after eating.

Coffee-ground looking matter sometimes vomited; but this is rare and exceptional.

Irritation, colic, and diarrhea frequent, from extension of inflammation to intestine.

Evening fever not uncommon.

Violet discoloration of the lips, conjunctivæ, face, &c., often present.

Not so attended.

Hypochondria not caused by chronic gastritis.

Is rarer even than cancer.

Is probably equally frequent in both sexes.

Occurs at all ages.

Is not hereditary.

Is often referrible to some distinct local exciting cause.

Is relieved or cured by antiphlogistic treatment.

**Gastric Cancer (early period.)**

Vomiting of sudden and severe character is never the first symptom; it occurs generally early in the morning, subsequently at variable periods after eating, or at periodical intervals.

The matters vomited are at first glairy, then half-digested food, then coffee-ground or soot like.

Bowels habitually and obstinately constipated; occasional severe diarrhea.

Fever absent.

Straw-coloured tinge of skin may be obvious.

Not so attended.

Hypochondria not an effect of gastric cancer. (?)

Is much rarer than gastrodynia.

Occurs more frequently in men than in women.

Is excessively rare before æt. 30.

Occasionally runs in families.

Is rarely, if ever, referrible to local agencies.

Is not cured, but is relieved by special treatment.

As the disease advances the diagnosis becomes less obscure, but cases do now and then occur which present all the ordinary combinations of symptoms, and yet are not cancerous; so that the detection of *tumour* is the only absolutely certain sign. This is most easily discovered when seated in the pylorus or great curvature: it is much more difficult to detect when occupying the lesser curvature or the cardiac orifice. It must be remembered that the situation of the tumour changes continually; and it is of the last importance to bear in mind that the whole course of the disease may be of an intermittent character.

In regard to treatment but little can be said. Conium is the only supposed specific which is applicable. Dr. Walshe has derived benefit from a combination of trisnitrate of bismuth and extracts of hop, stramonium, and conium, in pill; and he believes that the treatment generally should be such as is applied to nervous, rather than inflammatory affections. Leeches and blisters may be used to relieve occasional local irritation. Opium is contraindicated by the constipation; but the Indian hemp may be tried. A drop or two of oil of cajeput on sugar is the safest carminative. Cold applications or a blister sometimes also relieve the flatulence, and a dose of morphia has been found useful in the same way. But most is to be done by regulating the diet, observing what agrees with the patient, making the *quantity* taken at each meal *small*, securing perfect regularity in the hours, and complete mastication of each morsel. Sickness may be relieved by effervescing draughts, prussic acid, blisters, rough ice allowed to melt in the mouth, the application of ice in bladders to the epigastrium, or

an occasional dose of creosote. The bowels are best kept open by enemas : drastic purges are quite inadmissible.

M. Recamier narrates a case of pyloric tumour, "possibly cancerous," which was reduced in bulk by pressure, applied by means of a folded napkin secured by a bandage round the body. It may be well to try some such plan as this.

*Intestines.* The intestinal canal is among the more frequent seats of cancer. The Paris register gives 378 out of the 8289 deaths; in 221 of these the disease occupied the rectum, showing the much greater liability of the great than of the small intestines to affections of this nature.

Cancer of the small intestine is almost invariably primary : it is most commonly found in the duodenum and upper part of the jejunum, and is so rare in the ileum that its existence there has been altogether questioned. The symptoms are those of chronic intestinal disease complicated with evidences of increasing obstruction, passing eventually into a state of strangulation, but the actual diagnosis must depend upon the discovery of a tumour. The treatment is similar to that of cancer of the stomach.

Cancer of the rectum and anus are so well known, that we need not delay upon them. Dr. Walshe speaks in any thing but favourable terms of the operation for extirpation, and would limit its performance to those cases in which, from the completeness of the obstruction, speedy death appears inevitable, and when the use of bougies has failed to give even temporary relief. But even under these circumstances the surgeon has his option of making an artificial anus, either in the groin, in which case the peritoneum must be opened, or (according to Callisen's method, modified by Amussat) in the lumbar region, without injuring that membrane. For our own parts, we are inclined to give our decided preference to the last-mentioned operation.

Primary cancer is sometimes, but very rarely, found in the *mesenteric glands*. Dr. Carswell has shown that the disease may be seated on the free surface of the *peritoneum* ; and the omenta, especially the *omentum majus*, is not infrequently affected, secondarily, with colloid.

The *post-peritoneal cellular tissue* is by no means an uncommon locality of cancer, and the tumours, which are usually composed of scirrhus and encephaloid in variable proportions, often rapidly acquire an enormous size without giving rise to any marked symptoms.

Cancer of the *spleen* is of so little practical importance that it may be passed by unnoticed.

The *pancreas* is among the rare sites of cancerous formation. It has been denied that the disease is ever of primary occurrence in this organ, but such an opinion is contradicted by well-ascertained facts, and, indeed, it would appear that its secondary affection is the least common. Both scirrhus and encephaloid are found here ; the former is usually infiltrated, the latter is developed in the form of tumour. Generally a part only of the gland is affected, the head being perhaps the most frequent seat. The symptoms are obscure, and of course vary according to the influence which the diseased mass exerts over adjacent organs. There is usually pain ; often vomiting ; in many instances, but less invariably than has

been thought, an abundant watery discharge from the mouth, which discharge is supposed, but not proved, to proceed directly from the pancreas; and, when the tumour has ulcerated, discharge of blood from the stomach and by stool. It should be observed, however, that the absence of such bloody discharge is no decided evidence that softening has not taken place.

*Liver.* The liver stands pre-eminent among all the organs of the body for the frequency of its cancerous affections—even the primary form is extremely common. And of all its organic diseases cancer is the most frequent; but, when occurring primarily, it is slow to contaminate distant parts. Encephaloid is found here in the pure cerebriform state, and in the solanoid and hematoid varieties; scirrhus chiefly in the chondroid and napiform varieties. Dr. Walshe has never seen colloid in this organ. The most common combination is scirrho-encephaloid.

When primary, cancers in the liver are usually limited in number; when secondary, they are often exceedingly numerous, and this is peculiarly the case when the stomach is the part first affected. The lobules appear to be the seat of deposition. Dr. Walshe has invariably found, on examining a section of the liver, adjoining cancerous nodules, with a common lens, that the surface is sparsely studded with minute milky, though somewhat transparent, spots, looking as if the discoloration, which softens off at the edges, were produced by the point of a brush. The lobules seem *infiltrated* with the milky-looking material, which is most abundant towards their centre. Their outline and size exhibit no obvious change.

Our author has never seen the general bulk of the liver reduced notably below the natural standard, but in some instances absorption of its substance proceeds so uniformly with the morbid deposition, that no enlargement occurs. In other cases the dimensions of the organ are greatly increased. The existence of the one or other of these states makes a vast difference as regards the facility of diagnosis.

The disease is more frequent in males than females, and is most common between the ages of 57 and 70. Many of the symptoms are the same as in cancer of the stomach: those which specially point to the liver are tumour in the hypochondria, jaundice, and ascites. When the tumour can be felt it is nodular on the surface: this distinguishes it from fatty enlargement. Jaundice, without ascites, occurs in somewhat less than half the cases. Ascites, without jaundice, is much less frequent. The two combined occur in a fourth or fifth of all cases; in about a third neither is present.

The treatment should be generally conducted as that of gastric cancer. Dr. Walshe thinks he has seen the progress of the disease stayed by liberal inunction of the iodide of lead ointment over the hepatic region, and the internal administration of liquor potassæ in infusion of taraxacum.

#### CANCER OF THE RESPIRATORY ORGANS AND APPENDAGES.

Passing by the cancerous affection of the *nares*, *frontal* and *sphenoid sinuses*, *epiglottis*, *larynx*, *trachea*, *bronchi*, and *bronchial glands*, all of which are noticed, more or less fully, by our author, we arrive at the *lungs*; and

here we must delay for a little space, remarking *in limine*, that the reader will find in this section the best and most comprehensive account of the subject that we have anywhere met with. In 1091 post-mortem examinations of persons dying of all diseases, MM. Herrick and Popp found the lungs cancerous 6 times. In 67 cases of cancer these organs were affected 6 times. Of the 8289 cases included in the Paris registers, the fatal event is ascribed to the lungs in 8. The disease is most common as a secondary affection; when primary it shows little tendency to contaminate the system. Encephaloid is the species most frequently met with. The right lung is the most common seat of primary cancer; in the secondary affection both are usually implicated.

Secondary cancer generally exists in the nodular or tubercous form, more rarely as stratiform and cupulated sub-pleural patches; yet more rarely as a large irregular mass; and, least commonly of all, infiltrated through the pulmonary tissue. Primary cancer is found as an irregular mass in the majority of cases. It frequently occurs in the infiltrated form; very rarely in the nodular shape. It appears as the sub-pleural stratiform patch, or the sub-pleural nodule; is often both in the infiltrated and irregularly tubercous form in the same lung; and may exhibit itself as a series of fringed processes hanging from the free border of the organ.

All parts of the lung are liable to the disease, but the different forms affect a preference for particular localities. Thus, the disseminated nodular mass occurs scattered through the lung generally, but has a special tendency to accumulate near the pleura. The irregular tubercous mass is commonly found in the middle lobe of the right lung, whence infiltration extends in the majority of cases, though it may proceed from the apex towards the base, or *vice versâ*. It is probable that the intervascular interstices of the lobules are the main seat of formation. The secondary nodule doubtless originates in stagnation occurring in the ultimate radicles of the pulmonary vein. The progress of the disease is the same here as in other parts. The nodular form rarely produces any effect on the pulmonary tissue, except detrusion, with or without condensation, or atrophous rarefaction. A large tumour, of course, gives rise to condensation. Pneumonic consolidation is rare. Gangrene and pulmonary apoplexy have been observed. The ultimate branches of the pulmonary artery and vein are sometimes obliterated through extreme compression: in a case recorded by Heyfelder these vessels were converted into ligamentous cords from the heart onwards. It remains to be ascertained, whether, in such instances, a new circulating system is established round the affected parts, as Van der Kolk and Guillot have shown to arise in tuberculization. The trunk and larger branches of the pulmonary vein not unfrequently contain masses of the morbid deposit, and the same may occur in the superior cava. The bronchi undergo compression, or sometimes obliteration; or they may be partially destroyed by the extension of ulceration; or they are perforated by a portion of the morbid formation, which then vegetates in their interior. When a chief bronchus passes through the growth, and is there compressed, dilatation may take place above the obstruction. Little notice has been taken of the condition of the mucous membrane. The pleura is invariably in an unhealthy state. It is a fact of great practical significance, that in no case have phthisical tubercles, in any form or

stage, been observed, and that ulceration of the small intestines has not been noticed.

The growth of cancer of the lung is not limited to any particular period of life, but the male sex seems to be more prone to the disease than the female. In 10 cases the mean duration of the disease (calculated from the first appearance of symptoms) was 13·2 months; the greatest 27 months; and the least 3·5 months.

In most cases the disease, when of secondary type, runs an entirely latent course, giving rise to no symptoms, physical or functional; but this is far from being the state of matters in the primary affection. Here pain is almost invariably present; there is usually dyspnoea, and always cough, which in some few instances is dry, but generally accompanied by expectoration. The matter thus brought up varies in quality; it may be simply mucous or catarrhal, or purulent or bloody. In the latter case the blood seems mixed thoroughly with serosity, mucus or muco-pus, and the sputa, thus composed, are opaque and commonly free from viscosity, though they may be somewhat glairy, and in colour may be pink, or resemble red or black currant jelly. Actual hemoptysis is of frequent occurrence. It existed in 10 out of 19 cases analysed by Dr. Walshe, and of the remaining 9, in 4 there was bloody expectoration. This is a very high proportion, 72 per cent., while M. Louis gives 66 per cent. as the ratio in the consumptive patients observed by him. The decumbency is generally on the back, with slight inclination towards the affected side; in some cases there is orthopnoea towards the close of life. We have also as important symptoms the effects of *centripetal* and *centrifugal pressure*; the former exhibited in dysphagia, from pressure on the œsophagus; distension of the superficial veins, general tumidness of the face and neck, prominence and staring expression of the eyeballs, lividity of the surface and œdema, from compression of the deep venous trunks; and stridulous respiration, weakness of voice, and labour in speaking, from pressure on the trachea. The latter is shown by expansion of the walls of the chest, but this only occurs when the disease is of the tuberos form.

*Physical signs.* These are well marked and most satisfactory, as will be seen from the subjoined tabular view:

(A) Simple cancerous infiltration, or infiltration associated with limited tuberos formation, the latter especially seated towards the root of the lung.

(B) Cancer of the lung mainly tuberos, but (it may be) associated with infiltration to a limited extent.

### *Inspection.*

Retraction or depression of the affected side, with more or less marked deepening of the intercostal spaces; diminished motions of expansion and elevation; diminished costal motions.

Diseased side expanded generally, but not with perfect uniformity, nor bulged inferiorly; intercostal spaces widened, flat, or even slightly fuller than natural; motions, both general and costal, completely abolished; fluctuation imperceptible in intercostal spaces.

### *Application of the Hand.*

Vocal and tussive fremitus diminished in intensity; impulse of heart distinctly perceptible over the anterior surface of implicated side in some rare cases.

Surface unnaturally smooth and even; vocal and tussive fremitus completely deficient; neither simple nor peripheric fluctuation; double pulsation sometimes transmitted from the heart.



*Mensuration.*

Semi-circular measurement natural or diminished; deficient increase of width during inspiration.

Semi-circular measurement of the side natural or increased; width of side unaltered by inspiration; antero-posterior diameter increased; vertical measurement increased in cases of very extensive formation; distance between the nipple and the median line greater than on the opposite side.

*Situation of surrounding Parts.*

Mediastinum may be detruded towards the opposite side; the heart may be similarly displaced laterally or downwards; the diaphragm and infra-jacent viscera are not depressed.

Heart and mediastinum detruded laterally; corresponding division of the diaphragm with the infra-jacent viscera more or less depressed.

*Percussion.*

Sound intensely dull and of short duration; resistance of walls strongly marked; special character of sound tubular in some cases about the lower border of the infra-clavicular region; the dullness of sound sometimes extends beyond the middle line.

Sound completely and extensively dull and of short duration; resistance of walls extreme; limits of the dull sound not altered by changing the position of the patient.

[Before softening of the cancerous matter.]

*Auscultation.*

Strongly marked, diffused blowing respiration, or, with the progress of the disease (in consequence of accidental obstruction or obliteration of a chief bronchus), respiration weak or almost suppressed,—retaining, as long as it continues audible, the bronchial or blowing special character, bronchophony; bronchial cough; heart's sounds audible with undue intensity over the affected part.

Diffused or tubular blowing respiration, intensely developed in some cases; rhonchi either absent, or those of co-existing bronchitis; bronchophony; bronchial cough; heart's sounds audible with undue clearness; double blowing murmur in very rare cases accompanying the cardiac pulsation above-mentioned.

[After softening and elimination of cancerous matter.]

*Percussion.* Sound may become somewhat clearer; acquiring at the same time more distinctly the tubular, almost amphoric, special character; the resistance of the walls may proportionally diminish.

*Auscultation.* Cavernous respiration; mucous, cavernulous, or cavernous rhonchus.

The unaffected lung is the seat of exaggerated respiration, and the percussion sound of the corresponding side may be fuller and clearer than natural.

We have been thus full in noticing the physical signs, because it is clearly by a correct appreciation of these that the physician will avoid errors in diagnosis: the original work contains many precise rules for his guidance, but we have not space to enter upon them.

The disease is necessarily fatal, and but little can be affected by treatment. Dr. Walshe has found the intense dyspnoea more relieved by large blisters, frequently repeated, than by any other measure.

We must refer to the original for an account of the little that is known regarding cancer of the *organs of circulation*.

## CANCER OF THE KIDNEY.

It is only of late years that the subject has attracted much attention, but our author has succeeded in collecting forty examples as the basis of his description. When cancer affects the kidney the disease is rarely limited to that organ, and in the great majority of cases its implication is secondary. Primary cancer is, however, less uncommon than is generally fancied. The associated organs vary extremely, but

M. Rayer has shown that the liver and right kidney, and the adjacent parts of the stomach or descending colon, and the left kidney, are often affected together. It is less common than might be expected to see the bladder and kidney simultaneously diseased. Encephaloid is by far the most common species; scirrhus is decidedly rare, and colloid has not been detected in this locality. When the disease is primary it is disposed to pass through its stages rather rapidly; its advance is slow when secondary. The destruction of the renal substance and softening of the cancer forms cavities, filled more or less with variously altered blood and detritus. These cavities may remain shut sacs, or communicate with and pour their contents into the calices or ureter, or, in rare cases, into the extra-renal cellular tissue, the peritoneal cavity, or the interior of some portion of the intestine.

Encephaloid occurs in the kidney in the infiltrated and tuberos forms; the former more especially when the disease is primary, the latter when secondary. Both originate in the cortical substance. In some cases the disease affects the various parts of the organ pretty equally; in others the morbid matter accumulates, especially at the convex surface of the kidney, or in its upper half, while the lower part remains perfectly untainted. As the disease advances the organ loses its natural shape: Dr. Walshe has seen it large enough to fill three-fourths of the abdomen. The lymphatic glands and vessels are frequently implicated, and, by pressing on the duct, may give rise to great danger.

In 35 cases, the disease affected both organs 16 times; the right alone, 13 times; and the left, 6. It is most common in males, and in persons of advanced years, but may occur at any period of life.

In a few extremely rare instances the disease has remained latent until the patient's death; in others, pain of variable character has been the sole symptom throughout. Generally, pain is the only evidence of the malady until it has advanced so far that the tumour can be felt externally. The *direct* diagnosis depends upon the existence of this and another symptom, hematuria. The *tumour*, seated in the lumbar region, varies in size; it may scarcely protrude beyond the false ribs, or may reach the crest of the ilium; it may extend laterally beyond the umbilicus, and may obviously push forward the abdominal walls. In some cases it is distinctly visible and palpable in the lumbar region; in others there is no posterior intumescence. It is irregularly circumscribed, unevenly tuberos on the surface, unequally prominent, and unequally resistant in its different parts, variously elastic, but never really fluctuating. Whether the seat of spontaneous pain or not, it is almost invariably tender under pressure. Its dimensions are best ascertained posteriorly by the dull sound under percussion, which extends laterally to the flank; anteriorly, the amphoric note of the intestine is often producible over a limited portion of the dull sounding surface, from folds of intestine lying in front of the tumour—this is of importance in the diagnosis. The growth has usually been rapid.

In the early stages, the urine generally continues unaltered; but when the disease is advanced changes in that fluid are constant, and these consist essentially of the presence of blood or certain of its elements, of pus or encephaloid matter. *Hematuria* denotes either the breaking up of the tumour, or the coexistence of cancer in the calices or pelvis. It does not

necessarily remain a continuous symptom, and the quantity of blood lost varies greatly. *Albuminuria* has been observed, but it does not persist for more than a few days after the cessation of hemorrhage. The discharge of *pus* is seldom very great; but the microscope would probably often detect the presence of encephaloid matter.

Retraction of the testicle is of rare occurrence. The digestive organs almost invariably suffer, and there is complete anorexia. The cachexia is not commonly highly developed. There is emaciation, and usually œdema of the lower extremities. General dropsy is rare; and it is a singular fact that the brain does not suffer, and the intellectual faculties often remain clear to the last.

Little can be done by treatment. The diet should be nutritious, unless there be hemorrhage. Astringents and iron are useful in warding off attacks of hematuria and the cachectic symptoms, especially general dropsy. Opium should be given to relieve the pain. Small local bleedings may be occasionally useful in the same way, and to relieve hemorrhage; but they should only be employed when milder means have failed. Counter-irritation can only prove injurious.

The diagnosis is sometimes exceedingly difficult, and mistakes have occasionally been made by even the most accurate observers. Our readers will find in Dr. Walshe's pages full directions for their guidance; by the accurate following of which the chances of error will be greatly diminished.

#### CANCER OF THE BLADDER.

Cancerous diseases of this viscus are much more frequent than is commonly supposed: 72 of the 8289 deaths, so often alluded to, were caused by it; and Dr. Walshe believes that if the term *fungi*, habitually applied to growths in that organ, were more correctly defined, the estimate would be much higher. Tuberiform encephaloid is the usual species, scirrhus in any form being comparatively rare. In most cases there is only one tumour; and the common seat is the triangular space between the orifices of the ureters and the urethra, and the parts immediately adjoining this space. It also occasionally springs from the fundus and anterior or posterior surfaces, but has very rarely been observed on the sides of the organ. The progress of the disease is the same as in other hollow viscera, but, as a general rule, ulceration is slow to occur. The bladder is generally contracted, but sometimes enlarged; its walls are usually hypertrophous, and the mucous membrane presents the columnar aspect. Evidences of cystitis are sometimes seen, and calculous disease frequently coexists. The prostate may or may not be similarly diseased, and the condition of the kidneys and upper part of the urinary passages is various; but it should be observed that the coexistence of primary cancer in the kidney with primary cancer in the bladder is, considering the intimate functional connexion of the parts, singularly rare. The primary disease is almost exclusively confined to the male sex, and does not appear to occur before the fortieth year.

Among the earliest symptoms is frequent and difficult micturition. Uneasiness, sometimes amounting to severe pain, and increasing after the urine is passed, is experienced at the neck of the bladder. In some cases there is complete retention, in others a full stream may be suddenly stopped. These variations are caused by the position and mobility or

fixedness of the tumour. The urine is usually turbid, and often contains ropy matter, composed of pus modified by the evolution of ammonia from decomposed urea. Hemorrhage is frequent, often the first symptom, and occasionally the cause of death. In some cases tumour can be felt in the hypogastrium.

The diagnosis is most obscure, often impossible; but it is probable the microscope may afford assistance, by revealing the presence of cancer-cells in the urinary deposits.

The treatment should be merely palliative. To attempt the removal of the growths by operation would be to hasten the death of the patient.

#### CANCER OF THE GENITAL ORGANS.

The extreme inequality which prevails in the proneness of the two sexes to cancerous disease of these parts, and of the breast, is well shown in the following table, constructed from the Paris registers:

Of the Male,		Of the Female.	
Testicle	21 cases.	Uterus	2996 cases.
Penis	10	Ovary	64
Scrotum	7	Vagina	14
Prostate	5	Vulva	2
	43		3076
Breast	5	Breast	1147
	48		4223

*Male Organs.* Scirrhus is the species which most commonly invades the *penis*. A basis of this kind, however, not unfrequently gives origin to encephaloid vegetations. The disease may commence in almost all parts of the organ, but the glans and prepuce are by far its most common primary seat. It may originate either from a warty excrescence, or a pimple; or it may infiltrate the glans, or appear as a complication of venereal ulcerations. Phymosis, either congenital or acquired, is an exceedingly common accompaniment: and it appears probable that the irritation occasioned by this condition of the parts may act as an exciting cause of the disease in persons predisposed to cancer. Circumcision is, therefore, an advisable prophylactic measure, where the constitutional taint is known to exist. The duration of the disease is exceedingly various. It passes through the ordinary stages, and contaminates the lymphatic glands, the inguinal according to M. Buret, becoming first affected if the superficial parts are involved, the pelvic, if the deeper seated. Amputation is the only possible remedy, and it affords but a very miserable chance of cure. M. Lisfranc has proposed to dissect off the prepuce when this is the diseased part, leaving the rest of the organ intact; but the measure is one of doubtful utility.

The *vesiculæ seminales* have been found cancerous in some cases, but nothing is known of the symptomatology or diagnosis of such a condition.

The disease is rare in the *prostate*. When it exists it is usually of the encephaloid species. All the lobes of the gland may be affected, but the middle is specially liable. There is nothing peculiar in the symptoms, and the treatment must be merely palliative.

It is in the *testicle* that we find the most numerous examples of carcinoma, and here, too, encephaloid is the common species, true scirrhus

being very rare. The growth, when of large bulk, often has the characters of a composite tumour: and yellow discoloration, usually ascribed to the association of tuberculous matter, but most probably produced by softened fibrin, is very frequently observed. Both testicles are prone to the disease, but there is no example on record of their being, either simultaneously or consecutively, affected in the same individual. In the great majority of cases the body of the testis is the part first involved; and it seems probable that the disease commences by infiltration of the intervacular interstices of the gland. The progress is the same as in other parts, but ulceration of the investing scrotum is slow to occur. The tunica vaginalis becomes early diseased; the arteries of the cord are greatly enlarged, when the tumour has acquired considerable bulk; and the subcutaneous veins exhibit the usual dilatation. Contamination of the lymphatic system invariably occurs, the lumbar glands being most commonly affected.

The lungs, liver, kidneys, brain, meninges, bones, especially of the spine, the mesenteric glands, vena cava, and heart, are among the observed seats of secondary cancer, when the testis is its primary nidus; and perhaps the implication of the brain and spine is slightly more frequent under these than any other circumstances.

The disease may occur at all ages; and it is here that external violence has been most observed as an exciting cause. Horse exercise should, therefore, be avoided by persons of cancerous descent.

Increase of bulk and hardness of some particular point are the symptoms first noticed. These advance progressively, until the organ may have acquired a large size, and have changed its natural ovoid for a spheroidal shape. The diagnosis is sometimes extremely difficult. *Simple hypertrophy* is known by its commonly affecting both testes; *simple chronic induration*, which in many of its symptoms closely resembles cancer, by its yielding to mercurial treatment. *Syphilitic induration* is accompanied by dull pain, which is generally worse at night, but never lancinating: the organ, though sometimes increased, is not unfrequently diminished in bulk, and, as it were, wrinkled, its investments retain their natural character; the density of the gland increases, and its suppleness and elasticity are lost. The history and effects of treatment will also help to correct error. *Tuberculous disease* scarcely occurs except in phthisical subjects. In the ulcerated state mistakes can hardly be made; in the chronic unulcerated condition, the diagnosis will be aided by the smaller bulk of the part, its more distinctly nodular character, and its extremely slow progress. *Fibrous tumour* is excessively rare; it might be distinguished by its great weight, and the absence of pain and constitutional symptoms. *Cystoid and cysto-sarcomatous growths* present great difficulties; but they grow more slowly, are not productive of pain, and do not affect the cord, lymphatic glands, or constitution.

Compression, and the internal use of the iodide of arsenic, combined with the local inunction of the iodide of lead ointment, is a plan of treatment which deserves full trial. The results of extirpation are melancholy in the extreme.

*Female Organs.*—We need not dwell upon cancer of the external genitals. The disease occurs in the *ovaries* either as a primary affection, as an extension or a sequence of the same morbid state in the uterus, or

consecutively to the growth of cancer in distant and unconnected parts; but this is rare.

Scirrhus and encephaloid, both tuberiform and infiltrated, occur in this organ. The latter is the most common. Dr. Walsh has never met with colloid himself, nor found any satisfactory account of its being observed by others. The cancerous matter sometimes exists alone, but more commonly it is superadded to other morbid formations, and especially to multilocular cysts. Under such circumstances it is deposited in various forms on the walls of the cysts; but it is necessary to caution young observers against the frequent mistake of regarding as true scirrhus production, simple cysts the walls of which have been converted into fibrous tissue. The disease is usually confined to one ovary, though both are not unfrequently implicated. The morbid mass contracts adhesions with surrounding parts in many cases, and these adhesions have been found extensive where there had been no symptoms of peritoneal irritation—a fact of obvious practical importance. Of the special causes nothing is known, and the symptoms are merely those of ovarian enlargement in general. That it is no easy matter at all times to diagnosticate with accuracy the precise organ with which an abdominal tumour is connected, our readers are well aware; but we have not space to enter upon the question here. Let it then be granted that the growth, in any given case, is ovarian, how are we to determine its *nature*? If there be marked cancerous disease in any other organ, or if the cachexia be established, we have good ground for believing that the disease in question is of a similar kind. But should these helps be absent, we must be guided by the local characters of the tumour.

“*Fibrous tumours of the ovary* may, when large, be distinguished from scirrhus by their size; from encephaloid by their hardness, inferior elasticity, smoother non-lobulated surface, and uniform consistence in every part. The condition of the patient's health may facilitate the distinction. *Unilocular cysts* are of globular outline and non-lobulated; they usually contain a serous fluid, in which case the fluctuation of this will readily prove that the mass of the tumour does not consist of cancerous matter; and even where the contents are of a gelatinous character, the phenomenon is sufficiently well marked to prevent mistake. They are frequently movable, and are of slow growth. The constitution suffers little in this disease. *Multilocular cysts*, on the contrary, either from the coexistence of their contents (firm, jelly-like matter, blood, fluid or coagulated, atheroma, acephalocysts, pilous and fatty substance), or from the non-communication of their cells, rarely give rise to more than very obscure fluctuation, which may almost be confounded with the elastic doughiness of encephaloid. Careful examination will, however, enable the observer to avoid this error.

“It is not enough, however, that we be able to affirm that the entire mass of the tumour is not composed of cancer—an extremely rare occurrence; it is necessary to ascertain whether, and to what extent, encysted growths contain carcinomatous formations in their walls. In certain cases this may be done by the detection, both by manual examination and percussion, of irregular indurated masses contrasting with the softer divisions of the remainder of the tumour; when the cysts are exceedingly tense from thorough repletion with fluid, the sign of lobulation may not be ascertainable: in this state of things evacuation of the fluid by paracentesis will remove this difficulty in the way of the diagnosis. Nevertheless the discovery of these lobulated masses is not a certain sign of cancerous formation; bunches of sub-cysts, with gelatinous or albuminous contents (I mean as respects their outward appearance), aggregated in the parietes of larger cysts,



give rise to a precisely similar sensation. When hydatid fremitus exists in a marked manner, the presence of *acephalocysts* may be safely announced. Rapidity of course of the disease furnishes one of the best evidences of encephaloid complication."

In regard to treatment, Dr. Walshe strongly urges the full trial of iodine, both externally and internally, in the way before noticed. His own experience enables him to affirm that the progress of the disease may be thus arrested, and the size of the tumour in some cases actually diminished. In respect of puncture, it should be remembered that when there is encephaloid matter in the cysts, the evacuation of their fluid contents often occasions a more rapid deposition; the operation should, therefore, be undertaken only under the pressure of necessity.

We should now proceed to the consideration of the very important subject of *uterine cancer*, which is most ably treated by our author; but we have already wellnigh exhausted our assigned limits, and we deem it more advisable to employ what little space yet remains in bringing before our readers some points in the history of the disease which are less generally known; and we, therefore, purpose to pass entirely by this section and the next succeeding, which treats of *cancer of the breast*, with the simple remark, that in them will be found lucid descriptions of the morbid conditions, clear rules for diagnosis, and judicious advice in regard to treatment.

#### CANCER OF THE ORGANS OF INNERVATION AND APPENDAGES.

*Encephalon.* Every part of the encephalon has been the observed seat of cancer; but it is by far the most common in the cerebral hemispheres. Each of the three species is found here, but encephaloid much the most frequently. It exists in both the tuberiform and the infiltrated forms, the size and extent varying much. Generally, the brain contains but one cancerous growth. In some instances the tumours appear actually continuous with the surrounding brain; in others they are surrounded by a pseudo-cyst; while, most commonly, without such an envelope, they seem deficient in intimate connexion with the tissues around. The disease may be either primary or secondary. The state of the non-cancerous portions of the brain varies: it may be quite healthy; or exhibit punctiform injection, and flattening of the convolutions; or the neighbouring parts may be in a state of colourless softening. It is a singular fact that subarachnoid and ventricular effusion is usually very inconsiderable, and often altogether wanting.

The symptoms are unhappily obscure. Pain usually exists; but its characters, severity, and persistence vary within wide limits, and it is sometimes absent throughout. It is occasionally limited to a single spot, but is more frequently without defined position. The sensibility of the limbs and trunk may be diminished, increased, or perverted. Paralysis may or may not be present; it is sometimes hemiplegic, sometimes paraplegic, or it may be universal. It usually comes on gradually, but it may be developed at once. Partial or general convulsions are not uncommon. The intellect may or may not be affected. There is usually some derangement of vision, and deafness has been noted in some cases; but the senses of smell and taste rarely suffer. The nutritive functions remain

long unimpaired, but eventually they fail, and the patient becomes emaciated. The skin rarely acquires the yellow tint.

Colourless softening is the affection most likely to be confounded with cancer. The marks of distinction, as stated by M. Durand-Fardel, are the less frequency of cephalalgia, its being rarely limited to one side of the head, and almost always frontal; the absence of complete amaurosis, and the extreme rarity of convulsions in non-paralysed parts.

But even when the evidence of a tumour in the brain is decisive, it may not be cancerous; for various other productions are found in the same situation. Experience has not yet given any sure marks of diagnosis; but it should be remembered, with reference to one form, the tubercular, that M. Louis has proved that, with the rarest exceptions, the presence of tubercle in any organ, involves, in subjects aged upwards of fifteen, its existence in the lungs. Careful examination of these organs will, therefore, render assistance.

The treatment can only be palliative; it should be directed to the removal of irritation in the brain.

When the *cerebellum* is the seat of disease, the pain is in some cases referred to the occiput and nucha, but this is by no means invariable. The intellect is rarely affected. In a very few cases only has there been defective co-ordination of muscular action, which ought to have been frequent had the doctrines of certain physiologists been founded on truth. Hemiplegia is excessively rare; paraplegia still more so. Convulsions are more common. Complete or incomplete amaurosis has been often observed. Deafness has never occurred, unless the *portio mollis* has been actually implicated; and the phrenological views of the functions of this organ receive no support from recorded cases of this disease.

#### CEREBRAL MENINGES AND CRANIUM.

Under this head Dr. Walshe includes those cases of intracranial cancerous disease, which are practically distinguished by the morbid production perforating the cranium, and forming a fungous growth on its external surface; as likewise those where it originates in the meninges, without making its way outwards through the skull; and those excessively rare cases, wherein the disease is alleged to have formed in the subpericranial cellular membrane. The whole chapter is one of extreme interest and value; but a mere glance must suffice us here.

The disease, which is usually encephaloid, but in some few instances true scirrhus, may originate in any one of the following situations: 1, the external or the internal lamina of the *dura mater*; 2, *pia mater*; 3, cerebral substance; 4, cranial bones, generally the diploe, but occasionally the compact tissue; 5, subpericranial cellular tissue. In most cases there is only one tumour, but sometimes they are very numerous. They have been found at all ages, and with nearly equal frequency in both sexes. The symptoms vary according as the tumour continues still within the cranium, or has made its way externally. Under the first-mentioned circumstances they are the ordinary evidences of compression, sometimes developed suddenly, but most commonly by slow degrees. When the bony covering has been perforated, which is effected either by carcinomatous opening of the osseous tissue, or, as is more usual, by atrophous absorption, the cancerous formation protrudes through the adventitious

foramen in the form of a fungous tumour. This tumour may possess one or more of several characters, which, if combined in any number, are pathognomonic. It has the lobular surface and soft elasticity of encephaloid; it is immovable, yet non-adherent to the skin, which retains its natural appearance. The irregular edges of the perforation may sometimes be traced round it. It is the seat of two pulsations—one synchronous with the arterial pulse, the other corresponding with the rise and fall of the brain during respiration. It may be reduced either wholly or in part, and thus give rise to symptoms of compression; when left to itself, it recovers its previous appearance and size. It is not usually tender to the touch, but is often the seat of darting pain, which may be relieved by *gentle* pressure. Death usually occurs before ulceration has taken place.

But almost all these symptoms may be absent, and this constitutes the great difficulty in diagnosis. The disease may also be confounded with other maladies, as *encephalocele*; which, however, unless the result of accident, is almost peculiar to new-born infants—is, with the rarest exceptions, seated on the sutures or fontanelles, where cancer is not observed, is of equable consistence throughout, and passes through an opening with smooth and even edges. *Erectile tumours*, when they have attained any size, usually affect the condition of the skin, and their pulsations are more influenced by the general state of the circulation, and can be more readily controlled by pressure on the carotid. *Aneurism* will be known by the blowing murmur and the absence of opening in the skull. *Encysted tumours* and *abscess* could hardly be mistaken, unless the observer were very careless. *Cephalhæmatoma* exists in new-born infants alone; and only two cases of perforating fungus at this age are on record.

As respects treatment, Dr. Walshe recommends the trial of gentle compression. The lamentable results of surgical interference are strikingly shown in a table at p. 520.

Here we must pause. Our notice has extended to a much greater length than we had anticipated, and yet we feel that we have done our author but scant justice; and now we leave altogether untouched the very interesting and valuable information which he has collected regarding the cancerous affections of the spinal column and its contents, the nerves, skin, bones, and subcutaneous cellular tissue. To these, as well as all other portions of the volume, we again earnestly direct the attention of our readers; being confident that no where will they find a safer guide, or a more intelligent teacher than Dr. Walshe.

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## ART. II.

*Die Gallige Dyscrasie (Icterus) mit acuter gelber Atrophie der Leber.*

Von Dr. P. J. HORACZEK, practischen Arzte in Wien, &c.—Wien, 1844.

*On Bilious Dyscrasy (Icterus) with Acute Yellow Atrophy of the Liver.*

By Dr. P. J. HORACZEK.—Vienna, 1844. 8vo, pp. 278.

THIS is an enlarged edition of a work that has been received with considerable favour; and in its present altered and amended form, is intended by its author, to serve as an introduction to the subject of biliary disease in general.

Dr. Horaczek points out the confusion and error that have been introduced into this important department of pathological inquiry by preceding writers, who have either confounded diseases essentially different under the same appellation, or have applied different names to diseases closely allied, if not actually identical.

He objects to the names icterus and jaundice as expressive of a symptom merely, and proposes to substitute for them the terms "bilious dyscrasy," "cholosis," or "cholomæa," as significant of the peculiar dyscrasy of the blood, and of its saturation with more or fewer of the elements of the bile, that constitute the essence of this group of diseases.

The disease he subdivides as follows: 1, primary, or idiopathic, or protopathic cholosis; 2, secondary, or deuteropathic, or symptomatic cholosis; 3, polycholia.

*Primary, or idiopathic bilious dyscrasy*, or cholosis, which is the subject of the work before us, immediately consists, the author tells us, of a diseased composition of the blood, and a contemporaneous disturbance or total suspension of the secreting functions of the liver; both being the result of a disaccordance or derangement of the due vital relations between the nervous and vascular systems. Various anatomical and physiological changes are constant attendants on these primary diseased conditions, of which the most characteristic is a diminution in volume and alteration in texture of the liver, which, under the name of *acute yellow atrophy*, he has made part of his nosological definition.

To primary bilious dyscrasy belong the greater number of rapidly fatal cases distinguished as jaundice, as well as many known under the names hepatitis, bilious fever, cephalocholosis, &c.

*Secondary bilious dyscrasy* originates in various structural changes of the liver itself or of the neighbouring organs, in consequence of which, bile is again taken up into the circulation after its formation, and the functions of the liver are mechanically impeded.

*Polycholia* consists in an inordinate formation of the constituent materials of the bile, and their accumulation in the blood, concurrently with undiminished or even increased functional activity of the liver.

The earliest and most important change characteristic of primary bilious dyscrasy is that which takes place in the blood. In consequence of a deficiency of fibrine, icteric blood coagulates imperfectly. The coagulum breaks down easily, and is of a dark yellowish brown or dirty red colour, little altered by contact with the atmospheric air. As far as can be ascertained by microscopic investigations, the blood-corpuscles are diminished in number, and swollen. They also vary from each other in shape, and are less clearly defined in figure than natural. The serum is opaque and viscid, and it, as well as the buffy coat, when the latter does occur, is deeply yellow from a large admixture of the colouring principles of the bile. The other biliary constituents are not found in the blood in cases of purely idiopathic cholosis; though the peculiar atomic combinations from which the liver prepares the bile, as well as the biliary acids, sometimes are. The peculiar atrophy of the liver involves shrinking to a half or even a fourth of its usual size, with wrinkling of its peritoneal coat. Its parenchyma is soft, flaccid, bloodless, and inelastic, of a greenish or dirty orange colour; and deprived of its peculiar granular structure, so that its different con-

stituents can no longer be distinguished from each other. The gall-bladder is generally found contracted, and contains a colourless, or gray, or greenish slimy bile; its ducts are contracted and empty.

The spleen is in almost every case enlarged to twice or thrice its usual size, and easily lacerable or even reduced to a dark red pulp.

In severe and fatal cases the brain is found to have undergone serous infiltration, and great softening, especially round the central white parts. Yellow serum is found poured out as well over the convolutions of the brain and base of the skull as in the ventricles.

Anemia and relaxation of tissue or softening are found to be conditions common to the muscular, nervous, vascular, and glandular structures.

The mucous membranes in general, and the gastric mucous membrane in particular, are frequently softened. The mucous coat of the stomach, especially at the greater curvature, is found reduced to a dirty gray or greenish pulp. This change sometimes extends to the œsophagus, but more frequently to the duodenum; it is most frequently met with in the case of children.

Besides the foregoing, which Dr. Horaczek looks upon as constant and characteristic results of primary bilious dyscrasy, there are others of a casual or occasional character, of which the most important are congestions of the lungs, and passive inflammations of those organs or of the pleura or peritoneum; the products of which indicate the cacoplastic and diseased character of the blood.

The formation of coagula and limited inflammations in the portal or neighbouring veins, with consecutive purulent deposits in the liver, lungs, or other organs, are rare but very serious attendants on primary cholosis.

After having enumerated the symptoms which usually mark the onset of the disease, such as gastric and enteric disturbance, languor, lassitude, incapability of, or disinclination to, mental or bodily exertion, irregular chills and heats, loss of rest, depression of spirits, irritability of temper, &c.; the author enters at some length upon the consideration of the attendant febrile disturbance and characteristic yellowness.

The fever which seldom goes beyond mere irritative reaction, has generally an irregularly remittent character; the exacerbations assuming a regular quotidian or tertian type, only in situations favorable to the development of intermittent fever, or in the case of persons predisposed to neuroses of the ganglionic nerves. In many instances it disappears altogether after a few days. In the case of strong young persons, or those characterized by great vascular or nervous irritability, who have been subjected to the action of strong exciting causes, the disease often sets in violently with spasms or convulsions of an epileptic, tetanic, or hydrophobic character, or even with apoplectic symptoms; the attendant fever then wears an inflammatory aspect, with severe pain in the head, region of the liver, or abdomen, general turgescence, obstinate vomiting, red tongue, injected eyes, diminished secretions, heat, thirst, increased sensibility to light and sound, and full, hard, frequent, contracted pulse. After some days this high febrile action ceases, leaving behind a weak, soft, slow, and unequal pulse, or it may continue in a moderate form during the greater part of the illness, and terminate only with the general decline of the symptoms, or under imperfect critical efforts, manifested by general

perspiration, deposits in the urine, bilious stools, or hemorrhage. The characteristic and never-failing discoloration which attends cholosis, extends over the surface sometimes in the course of a few hours, and also stains every organ and tissue in the body, except the substance of the brain and nerves, and the enamel of the teeth. It is obviously the result of vicarious efforts to get rid of the biliary elements accumulated in the blood, and varies from a clear or dark yellow to a green bronze, and even reddish hue, according to race, occupation, and the presence of other cachectic conditions. Thus, in cases of chlorotic complication it is of a grayish; in scorbutus, of a greenish or dark yellow tint. Other modifications of which it is susceptible, which have led to the adoption of distinctive appellations, such as *icterus variegatus*, *ict. partialis*, *ict. dimidiatus*, &c., generally belong to the secondary forms of cholosis, and depend on the coexistence of other dyscrasies or on organic disease.

The presence of the biliary colouring materials in the urine is shown by its characteristic saffron yellow colour when seen in thin layers, and also by its power of staining other objects, as well as by the effects of nitric and hydrochloric acids. If the freshly passed urine be carefully poured on a sufficient quantity of nitric acid, and the two be gradually mixed together, the red or yellow colour of the urine will pass into green, blue, and violet. If the colouring material in the urine be small in quantity, the change to green is succeeded by a restoration of the yellow colour, without the intervention of any intermediate tint. The yellow or brown colour of the urine, is also changed to green by hydrochloric acid, but less certainly and obviously; the smallest colouring admixture of the urine may also be detected by using a good achromatic microscope.

The other constituents of the bile, such as biliary resin, bilin, cholesterin, &c., are also present in icteric urine. The cholesterin is in some cases deposited, and may be detected under the microscope as regular prismatic crystals, or more frequently as an amorphous mass. Icteric urine, when voided, throws up a permanent froth, has a saponaceous feel, gives out a peculiar sickly sweetish smell, and deposits on the vessel a fatty opalescent scum.

When the disease is at its height the urine is alkaline, and disposed to putrefy; but as the symptoms decline it becomes acid soon after emission. The urea is also generally diminished, as well as the fixed salts and earthy phosphates. The specific gravity varies a good deal; it is often normal, but still more frequently falls to 1010-1008. These changes Dr. Horaczek looks upon as resulting from the diminished energy of the vascular system, and consequent imperfection of the respiratory functions, the effect of which is, that a smaller quantity of oxygen is brought into contact with the blood, and the decarbonization of its normal and abnormal constituents is diminished; and hence, as in the case of the amphibia, the formation of urea is lessened, and that of uric acid is increased.

The taste and reaction under nitric acid show that the biliary constituents are present in some of the other secretions, amongst which may be mentioned the perspiration which tinges linen—the serum, whether of the blood or that poured out by blisters, or from œdematous parts—the various mucous discharges, and those from abscesses and ulcers.

The paucity of observations does not allow us to decide whether the tears,



saliva, semen, and milk, undergo similar changes. But the bitter taste of the milk observed by some, and the refusal of the breast by the infant, show that it must have sustained some qualitative change.

The characteristic pain of the liver, which sets in at the commencement of the disease, and accompanies it throughout, is of a nervous character, and is generally referred to a circumscribed spot over the left lobe. In slight cases it amounts only to a feeling of tension and oppression, with occasional fugitive stitches; aggravated by pressure so as to compel the patient to make a forced respiration. In bad cases it becomes very severe. It goes hand in hand with the loss of function and decrease in size of the organ.

The increase in volume of the spleen, which keeps pace with the atrophy of the liver, is evidenced by feelings of tenderness and oppression in the left hypochondrium, which sometimes increase to severe pain obstructing respiration.

There is also a remarkable pain in the eighth or ninth dorsal vertebra; as well as pains in the limbs and joints of a rheumatic character, which alternate with a feeling of stiffness, and a consciousness of stupor, or total loss of sensation in the parts. As these painful affections are witnessed in cases where there has been violent vomiting; Dr. Horaczek thinks they may be attributed to the mechanical agitation of the body. Embarrassed respiration and constriction of the chest, which are increased at night, or on making any sudden movement, and sometimes induce fits of palpitation, he attributes to the diseased condition of the blood. Disease of the heart or lungs inducing similar symptoms may be detected by percussion and auscultation. The advanced stages and severer forms of the disease are marked by cephalic and nervous symptoms, which set in sometimes quite suddenly and unexpectedly. To restlessness, sleeplessness, high delirium, repeated vomiting, and tonic or clonic spasms, succeed apathy, diminished sensibility to external impressions, low muttering delirium, with rapid, feeble, unequal pulse, gradually increasing, and at length profound coma, paralytic affections, total unconsciousness, involuntary evacuations, cold viscid sweats, and death. This modification of the disease has been named nervous-jaundice, or cephalocholosis. Sometimes the disease runs on to regular typhus, or it ends in dysentery, or the powers sink under coffee-ground vomiting and purging, indicative of passive hyperemia, and softening of the mucous lining of the intestine canal.

Of combinations and complications, besides those just noticed, the most serious is that with inflammation of the abdominal veins, which the author thinks may be attributed to the rapid shrinking of the liver inducing retarded circulation and partial coagulation of the blood. The coagulum so formed undergoes a purulent decomposition, and induces inflammation of the inclosing vessels, attended by the usual symptoms. One very important remark, especially in a practical point of view, is, that genuine healthy inflammation is incompatible with cholosis; though inflammatory congestion with softening of the mucous and serous membranes, and passive inflammations of the lungs and other parenchymatous organs are frequent enough.

All these processes are, however, slow and irregular, without any tendency to a critical termination, and partake of the nature of mere conges-

tion and infarction, with a disposition towards decomposition rather than to proper inflammatory plastic processes. Whilst alluding to certain periods of life as predisposing causes of cholosis, he takes occasion to notice icterus infantum, respecting which he seems to doubt what may be its true relation to biliary disease in general. He thinks it probable that many cases belong to primary cholosis, both from the symptoms during life and appearances detected after death. Some may be referred to secondary varieties of the disease, but there are not a few whose true circumstances and position remain to be determined by numerous and accurate observations. With respect to progress and duration, he defines the disease to be ("atypische") one *sui generis*, irregular in progress, undetermined as to limits, and of uncertain duration, which develops itself gradually or suddenly.

In some cases the symptoms disappear suddenly before they have become more than precursory, or they are so mild as scarcely to claim medical interference, or interrupt the patient's usual avocations.

The average duration of the disease is from four to six weeks. When prolonged beyond eight or ten weeks, it passes into the secondary forms. It may, however, destroy life in eight or ten days. It is not the subject of proper critical efforts, for although a certain degree of alleviation sometimes follows deposits in the urine, general perspiration, discharges of blood, eruptions on the skin, and the restoration of suppressed secretions or excretions, yet Dr. H. does not consider them properly critical, because they do not appear at any regular period, are only accidental, and the disease often terminates favorably without them. Bilious stools frequently bring about a very decided improvement, but they have not always such a desirable result, nor is the time of their appearance determined; and, therefore, even they cannot be strictly called critical. The prognosis is generally favorable. Bilious dyscrasy sometimes, though not often, ends in other diseases; generally in bilious cachexia, with a disposition towards the reappearance of the original malady, or towards the development of its secondary forms. In such cases we have digestive derangement, chronic vomiting, irregular action or torpor of the liver, disposition towards the formation of gall-stones, shrinking and hardening of the liver, &c. It may also end in putrid fever; and if the brain has been much engaged, it may leave behind epilepsy, maniacal affections, or paralysis of the extremities.

When death does take place, Dr. Horaczek views it as the result of poisoning and paralysis of the cerebral nervous system, evidenced by the cephalic symptoms before enumerated, which follow the derangement or disturbance of the mutual relations between the nervous and vascular systems, and the subsequent softening of the organs chiefly implicated. Chemical analysis of the blood, and of the various secretions and excretions, is the most suitable, and, with the improvement of chemistry, will be the most certain method of diagnosing cholosis.

We may thus determine whether cholosis, anemia, cachexia following the abuse of spirituous liquors, that which attends cancer, pyæmia, or purulence of the blood, phlebitis, &c., all of which are accompanied by more or less yellow discoloration of the surface, are also complicated with cholosis or not.

It is often difficult to distinguish primary cholosis from some of its secondary forms, dependent on disease of the liver, pancreas, pylorus, duodenum, pleura, &c., or on obstruction of the biliary ducts, feculent accumulations in the colon, or pregnancy. Disease of the hepatic parenchyma, attended with increase of volume, may, however, be detected by manual examination. There is, besides, in such cases emaciation, with long-continued dyspepsia and dropsical appearances; the yellowness, moreover, is earthy, the skin is dry, harsh, and faded, and the symptoms indicating the narcotising effects of the accumulated biliary constituents on the nervous system are not well pronounced.

It is difficult to diagnosticate jaundice from gall-stones, from primary cholosis, especially if it attack young persons under the influence of mental emotions, and with acute symptoms, such as vomiting, pain in the liver, &c. Gall-stones are, however, generally a disease of persons advanced in life; the pain they cause is colicky, attended by a sensation of burning heat, sometimes remitting, or coming on at intervals like the pains of labour, and it often ceases suddenly, or is relieved by remedial agents. There are also cramps of the abdominal muscles, and of the whole body; the skin is covered with cold sweat, the pulse is contracted, the head is not engaged, and inflammation of the liver or neighbouring parts often succeeds.

Polycholia cannot readily be confounded with primary cholosis, as it is attended by increased size and increased functional activity of the liver, together with an overflow of bile into the intestinal canal; as well as by an increased formation of biliary matter in the blood, and jaundice, consequent on its insufficient separation through the liver. To polycholia belong bilious fevers, and bilious pneumonia and pleurisy.

It remains to notice some of the chief therapeutical indications according to our author, as well as the means by which he proposes to work them out.

The causes of the disease, if still in activity, must if possible be removed, or their operation be neutralized.

The restoration of the disturbed relations between the functions of the vascular and nervous systems he lays down as another indication. In reference to which he remarks truly enough, that as the precise nature of this disturbance, not less than of the normal relations of these functions to each other, is and ever will be a mystery, we can only resort to such rational or well-devised empirical measures, as tend to restrain their activity when in excess, and to excite it when unduly depressed.

We are as yet acquainted with no agents calculated to neutralize the injurious effects of the biliary elements on the blood, except those found to be useful in other cachectic and dissolved conditions of that fluid; of which the best are the mineral and vegetable acids, and preparations containing chlorine. The most important indication is to eliminate the biliary admixture from the blood, by exciting the activity of the liver; for this purpose purgatives must be used, of which he prefers rhubarb and aloes in full doses. Calomel must not be given except in occasional and purgative doses, lest the plasticity of the blood be still further reduced; and for the same reason the author objects to saline aperients, which merely in-

crease the intestinal secretions, except in those cases where it is desirable to make the intestinal canal the seat of a vicarious discharge of the biliary elements, in consequence of a total and persistent suspension of the functions of the liver.

If we are unable to restore the functional activity of the liver, the best substitute for it will be excitement of the kidneys by means of diuretics.

If symptoms of biliary oppression of the brain display themselves, the restoration of the hepatic functions must be attempted by strong emetics, drastic purgatives, dry cupping, stimulating applications to the abdomen, and stimulating enemata.

If the cerebral oppression be caused by hyperemia, or an inflammatory state of the brain, which is sometimes, though rarely, the case, we may resort to local bloodletting, cold applications to the head, sinapisms to the feet, or stimulating pediluvia, repeated doses of tartar emetic in decoction of tamarinds, and cooling acid drinks.

If the cerebral affection be purely nervous, besides external stimulants and revulsives applied to the head itself, as well as other parts of the body, the author resorts to the internal use of stimulants and tonics, enumerating in addition to those generally used in these countries, phosphorus and the flowers of the arnica, which last he prefers to all others, on account of its specific effects on the brain.

Pain in the liver, which is almost always purely nervous, he treats with external emollients, derivatives, and sedatives.

He subjoins a tolerably extended list of popular and superstitious remedies, to which additions, equally whimsical and disgusting, might be made from sources nearer home.

The book concludes with a detailed narrative of upwards of 90 cases. More than one third of the number terminated fatally, and the morbid appearances which they presented on post-mortem examination are also given, so that we are furnished with copious illustrations by which to test the doctrines and precepts inculcated by the author.

We here conclude our account of Dr. Horacek's work, from which we have extracted largely. Although the author's pathological views are not always very definite, and are sometimes very open to criticism, we still think the book is well worthy of a careful perusal, as it supplies an important addition to our knowledge of a very obscure class of diseases, which it presents under aspects chemical, pathological, and even semeiological, in some respects both new and important; and induces us to look with considerable interest for the fulfilment of Dr. Horacek's intimated design, of giving a complete view of the subject, by the publication of his researches on the other forms of bilious dyscrasy.

## ART. III.

*Transactions of the Provincial Medical and Surgical Association.* New Series. Vol. II.—London, 1846. 8vo, pp. 285.

THE contents of the present volume are the following:—1, The Retrospective Address in Medicine, by Edward Charlton, M.D.; 2, The Retrospective Address in Surgery, by Thomas P. Teale, Esq.; 3, On Grinders' Asthma, by C. F. Favell, M.D.; 4, On the Inverted Displacement of the Urinary Bladder, by J. G. Crosse, Esq.; 5, Report of the Reading Dispensary for the years 1841–4, by Charles Cowan, M.D.; 6, A Statistical Report of the Surgical In-patients of the Royal Berkshire Hospital from 1839 to 1845, by George May, Esq.

I. Our readers have had so much from ourselves in the form of Reports of the progress of medical science in its different branches, that they will excuse us for not giving any account of the two excellent addresses with which the volume opens. We must also content ourselves with referring to the original report of the Berkshire Hospital and Dispensary, which do great credit to the industry and talents of their authors. It is surprising, when we consider the great number of hospitals scattered over all our counties, that so few of their brethren have emulated Dr. Cowan and Mr. May in enriching the pages of these transactions with similar documents.

II. Mr. Crosse's paper gives the history of a case of rare occurrence, and is important, as exhibiting the value of a careful examination and consequent just diagnosis, and the dreadful risk attending a mistake in this particular. We extract the more material points of the narrative:

"In the year 1829, a highly respected colleague of mine, since deceased, received under his care a healthy-looking female child, aged between two and three years, on account of a tumour, about the size and shape of a walnut, projecting visibly at the external *labia pudenda*. It was of a florid red colour, and somewhat granulated upon its surface, so as to resemble a large strawberry; and the surgeon entertained a notion that it was a vascular tumour, which might be removed by ligature, on which account he requested me to inspect it.

"After a slight examination, I expressed my doubts as to its being a vascular tumour, and dissuaded him from the hasty application of a ligature. I could not, however, immediately explain its nature, having no conception how such a tumour could be formed by the displacement of parts only, without any superadded morbid growth. Towards the posterior part of the tumour, and on its sacral aspect, there was an aperture, which was conjectured to be the entrance into the displaced urethra. A very small female catheter easily entered this aperture, and passed along a channel a little to the left side of the median line: urine distilled in drops through the catheter, but there was not a gush, although the instrument had entered so far that we concluded it must have reached the cavity of the bladder. Besides what thus oozed through the catheter, slightly tinged with blood, there was an oozing of urine from another source, which was not explained until a second and more strict examination, instituted a few days afterwards, on my casually coming to the patient's bedside, just as the surgeon was prepared to apply a ligature round the neck of the tumour.

"I now found, concealed in a fold of the tumour, and near to the posterior junction of the labia, two orifices not far asunder, from which the urine oozed, and which were evidently the vesical terminations of the ureters. On pressing the tumour firmly, as if to reduce it like a hernia, I found it yield and pass gradually behind the symphysis pubis, and within the labia; and under a continuance of the taxis it all retired, leaving the external parts in their proper shape and position. A passage remained, through which the tumour on retiring had taken its course, which was actually the dilated urethra, into which I *could* and *did* introduce my little finger, until it fairly entered the cavity of the replaced bladder; for it now became clearly demonstrated that the vascular red tumour, externally presenting itself as first described, was the urinary bladder in its entire thickness, including its mucous, muscular, and peritoneal coats, prolapsed through the dilated urethra, and at the same time inverted or turned inside out. The proper lining membrane of the bladder became, in the progress of this displacement, the external covering of the tumour. As fast as the urine was secreted by the kidneys, it oozed from the terminating orifices of the ureters, which were concealed within a fold of the exposed surface of the tumour, and approximated to each other. The neck, or deepest and narrowest part of the tumour, just concealed within the labia, was covered by the inverted lining of the urethra, the inversion being complete.

"In this instance, had a ligature been efficiently applied to the neck of the tumour, as was contemplated, the bladder would have been removed, including all its coverings, the ureters cut through just above their terminating orifices, and the peritoneal cavity largely opened, with a necessarily fatal result!

"As the friends of the child could not be applied to, the history was imperfect. It was stated that the tumour had existed for a considerable time, and been always attended by stillicidium urinæ; also that it had been once replaced, but descended again, shortly before it came under my observation. During the short period that the child remained under my notice, after the replacement of the bladder, there was no relapse; and since this account was sent to press, I have been fortunate enough to ascertain, and to be enabled to add, that the patient is still living, after an interval of sixteen years, and is a healthy young woman, save only the affliction of the incontinence of urine, with which she has been constantly troubled, but without any relapse of the vesical displacement.

III. We had occasion, some two years past,\* to draw the attention of our readers to the disease familiarly known as Grinders' Rot or Asthma, in connexion with the work of Dr. Calvert Holland. The tolerably full notice we bestowed upon that work will save us from the necessity, in the present place, of entering into any preliminary details concerning the various modes of occupation and pursuit of the artisans who fall victims to this singular affection—singular, perhaps, rather in its peculiar mode of manifestation, than in the fact that some disease or other of the pulmonary organs should be the result of the mode of working pursued in Sheffield and its vicinity. Besides, much fuller (and, as far as may be judged from internal evidence, more correct) information on this branch of the subject may be derived from Dr. Holland's volume than from the essay now before us.

"The object which I have peculiarly in view, in the present communication," says Dr. Favell, "is to determine the pathology of grinders' asthma, or rather, perhaps, I should say, to exhibit the lesions of the respiratory organs, which morbid anatomy most frequently reveals in the persons of those who have fallen victims to this disease."

Here two things most different—the pathology and the morbid ana-

\* Brit. and For. Med. Rev., October, 1844.



tomy of a disease—are spoken of as though they were all but one and the same. We notice this as a mere verbal inaccuracy. The author knows as well as we do, the difference between them.

Dr. Favell says that the most remarkable physical signs observed in these cases are “more or less imperfectness of expansion in some portion of the chest, an abnormal amount of dullness on percussion, the substitution of the tubular for the vascular murmur, and the occasional existence of amphoric resonance, cavernous respiration, and pectoriloquy.” Now Dr. Holland, on his side, puts forth the following statements:—“The chest generally sounds well on percussion,” “far better than would be anticipated from the pulmonary affection;” and, further on, the sound on percussion is described as “being frequently much louder than in health.” The respiration is, according to the same writer, natural, puerile, or bronchial. Doctors will differ; so that there is nothing more than an amiable deference here to the common opinion, which hath made itself known in the well-worn saw. But if the cause of the difference be sought for, we are, we confess, puzzled. Can any of the “grit,” or the “dust,” or the “flying particles,” so common in the locality, have obtruded itself into the ears of either observer, and transformed the sounds actually evolved into others harmonizing more or less distinctly with the theories held by each? But whatever be the explanation, the fact is a most singular one.

“Sensual gratification is the great object the grinders have in view,”—a character which is in nowise peculiar (as Dr. Favell's inference would seem to be) to the class of whose pulmonary diseases, he has composed a history. Nor do we, as a general fact, descry the utility or philosophy of seeking to enrol debauchery, venereal or spirituous, among the “causes” of the grinders' rot, when it is clear as the light of heaven, that if the poor wretches led the chaste and temperate lives of very anchorites, and continued to inhale this pernicious grit, they would perish victims of the “rot” its inhalation engenders. We have no doubt that spirituous drinks and consequent inebriety do greatly aggravate the local malady of these miserable men; but, at the same time, we say that it might be asserted with almost as fair a show of probability, and for any thing shown to the contrary by the historians of the “rot,” that habits of inebriety act rather in the direction of slackening than accelerating the speed with which the pulmonary disease runs its fatal course. This may appear a paradox to some pathologists (we mean those of the *a priori* school of reasoning),—but we profess to *observe*, and not to frame opinions for one sect or the other.

Dr. Favell relates some cases of the disease with the particular view of illustrating its morbid anatomy. From these cases it appears that the main morbid changes discovered are—“1, tubercles; 2, small bodies resembling currants disseminated extensively on the surface and throughout the substance of the lungs; 3, large masses found in different portions of the pulmonary tissue; 4, emphysema; 5, dilatation of the bronchial tubes; 6, inflammation of the lining membrane of the bronchi, trachea, and larynx; 7, adhesion of the pleuræ; 8, enlargement of the bronchial glands; 9, enlargement of the heart; 10, a granular condition of the kidneys.”

The disease of the kidneys and the adhesions between the pleuræ the

writer regards as intercurrent affections, and not necessarily connected with the disease of the lungs. He very correctly shows the error of Dr. Holland's notions concerning the state of the bronchial glands and dilatation of the bronchi as constituting peculiar phenomena in this disease.

The primary or essential changes are inflammation of the larynx, trachea, and bronchi, small bodies resembling currants, and tubercles. The laryngeal and tracheal inflammation, with occasional ulceration, is said by Dr. Holland to be in a large number of cases the primary affection—he holds that it remains sometimes for a considerable period. This Dr. Favell "doubts." But Dr. Holland says, that on examining "the larynx during the continuance of these symptoms, the mucous membrane is often much more florid and vascular than is natural, and occasionally small ulcerated points are observed." And now behold Dr. Favell has Dr. Holland on the hip, and, having him there, is somewhat merciless towards his colleague and townsman: "Now, this statement I shall not attempt to contravene, inasmuch as I do not pretend to be able to inspect the lining membrane of the larynx during the lifetime of the patient." Dr. Favell is evidently sharper than Dr. Holland; his sidelong cuts at the rival historian are remarkable enough. Indeed, if we may judge from Dr. Favell's exhibition of temper, it would seem probable that the existence of harmony scarcely prevails amid the doctors of Sheffield; and we cannot but think it would have been more dignified and decorous in the author to have shown a less critical spirit in dealing with the opinions of his townsman, more especially in a paper publicly read in an assembly of their *local brethren*.

The "currant-like bodies" Dr. Favell considers are nothing more than the dilated extremities of veins, containing some of the solid constituents of the blood. He says that he has traced such appearances with the scalpel. Were we disposed to be critical, we might fairly question Dr. Favell's being any more able to trace the *extremities* of veins with the scalpel, than Dr. Holland to see the interior of the larynx in the living man; but we understand both, and we merely advert to the circumstance from the natural suggestion of stones and glass houses.

Dark masses as big as a nut or as an orange—sometimes grayish, sometimes black—sometimes dense, sometimes easily cut, are found in some cases. What are these? "In some instances I believe they are occasioned by the effusion of blood into the parenchymatous substance of the lungs, constituting what is properly called pulmonary apoplexy; but in the majority of cases they are doubtless the consequence of pneumonia, either in the acute or chronic form, and to which the grinders are peculiarly liable." Now this "sometimes one, and sometimes the other" mode of getting rid of a difficulty, reminds us of the well-known answer of the Cantab. at the "Little-Go," who to the question of his good-natured examiner, as to whether the sun moved round the earth, or the earth round the sun, boldly replied (by way of making sure) "sometimes one and sometimes the other." Dr. Favell must surely have felt that as two states could never be more dissimilar in nature and pathological signification than hemorrhage and inflammation,—yet he makes them have almost convertible morbid changes.

On the whole this essay gives us a rather more precise account of the

errors and inadvertences of Dr. Holland than of the real nature of the disease, concerning which it professes to enlighten us. It, however, exhibits talent; and we are glad to receive it as an earnest of something better on the same subject from the same pen. The author's general conclusion is that "the disease essentially depends on congestion, or inflammation of the parenchymatous structure of the lungs; in some cases giving rise to the formation of tubercle, and in others occasioning pulmonary degeneration without tubercular deposit."

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#### ART. IV.

1. *Pathologische Anatomie des menschlichen Körpers*. Von JULIUS VOGEL. Erste Abtheilung. (Allgemeiner Theil.)—Leipzig, 1845.

*The Pathological Anatomy of the Human Body*. By JULIUS VOGEL.—Leipsic, 1845. pp. 533.

2. JULIUS VOGEL *Icones Histologiæ Pathologiæ. Tabulæ Histologiam Pathologicam illustrantes. Viginti sex Tabulæ, continentes CCXCI Figuras, quarum CCLXX ad naturam delineatæ sunt*.

*Erläuterungstafeln zur pathologischen Histologie mit vorzüglicher Rücksicht auf sein Handbuch der pathologischen Anatomie*, herausgegeben von Dr. JULIUS VOGEL, ausserordentl. Professor der Medizin in Göttingen. Sechs und zwanzig Tafeln mit 291 Figuren, wovon 270 nach der Natur gezeichnet sind.—Leipzig, 1843.

*Illustrations of Pathological Histology, illustrative of his Manual of Pathological Anatomy*. By Dr. JULIUS VOGEL, Extraordinary Professor of Medicine at Gottingen. Twenty-six plates, with 291 Figures, of which 270 are drawn from Nature.—Leipsic, 1843. pp. 120.

DR. JULIUS VOGEL has long been known as an accurate chemist, a good microscopic observer, and a sound pathologist,—qualifications which eminently fit him for the duty he has now undertaken of publishing a treatise on morbid anatomy. In addition to the works whose titles head this article, he is favorably known as the author of essays "on the Sputa in various Diseases," and on "the Physiology and Pathology of Pus," of the chemical portion of Wagner's 'Elements of Physiology,' of an "Introduction to the use of the Microscope in the Chemical Analysis of Animal Matters" (reviewed in Vol. XVII, p. 424), and of the articles "Inflammation," "Morbid Tissues," and "Hypertrophy," in Wagner's 'Cyclopædia of Physiology,' now in the course of publication.

In the introduction to the present work we find some excellent remarks on the proper means of acquiring a knowledge of pathological anatomy, and as we believe that these means are neglected, and indeed, despised, by some of our English pathologists, we avail ourselves of this opportunity of pointing out to them what are deemed by their German brethren the qualifications requisite for the successful cultivation of this department of science.

"The study of those morbid changes to which the various parts of the body are

liable, depends on a thorough previous knowledge of their normal relations; hence pathological anatomy requires a perfect knowledge of the structure of the healthy body, and of the uses of its different parts, in order to be able to estimate the influence which any morbid alteration of an organ impresses on its function. We must not content ourselves with studying the coarser changes, such as are visible to the naked eye, but we must examine those finer modifications of the tissues which can be recognized only by the microscope; hence the necessity for an accurate knowledge of general anatomy or histology. In the investigation of delicate points connected with pathological histology the microscope is altogether indispensable, and the application of chemical reagents must be observed under it. Chemical analysis is indeed of the greatest importance in pathological anatomy, being the only means by which we can on several points obtain the desired information." (p. xxxii.)

The whole work is divided into two parts,—the general and special. Of these we have at present only the former, the latter being promised in the course of the present year. The *Icones*, which were published about three years ago, refer both to the special and general department, there being twelve plates illustrative of general, and fourteen of special pathological anatomy.

**GASES.** The first chapter is devoted to the consideration of the *abnormal development of gaseous matters*. Accumulations of gas within the body may arise from very different causes.

1. *They may be produced by the external pressure of the atmospheric air.* The mechanism of this form of origin is most strikingly seen in those cases of general emphysema which arise from an injury to the lungs dependent on a penetrating wound of the thorax. Our author gives the following graphic sketch of the progress of the affection.

"The air admitted into the cellular tissue of the thorax gradually works its way over the body, and the emphysema thus becomes more or less general. The orbits become closed up; the eyes and mouth remain shut, in consequence of the swollen condition of the eyelids and lips; the nose is hidden between the tumid cheeks; the skin of the neck is so monstrously distended that all distinction between the head and neck disappears; the scrotum swells to such a size as to conceal the penis; the limbs enlarge and assume a cylindrical form; the palms of the hands and the soles of the feet (in consequence of their firm connexion with the subjacent tissues) being the only parts not affected. In unfavorable cases the patient dies from impeded respiration and apoplexy, in consequence of the compression exercised on the air-tubes and jugular veins by the swelling." (p. 2.)

2. *Gases may be developed in the body in consequence of decomposition, fermentation, and putrefaction.* In accordance with the laws of organic chemistry, most animal and vegetable bodies undergo decomposition at the temperature of the human body, and in the presence of moisture, even when air is excluded. Gases are thus developed, which vary in accordance with the putrefying substances that give origin to them. Non-nitrogenous substances yield carbonic acid, carburetted hydrogen, and hydrogen; nitrogenous matters yield ammonia in addition to carbonic acid, and if sulphur and phosphorus are present, sulphuretted and phosphoretted hydrogen, and hydrosulphate of ammonia are also developed. Gas is thus developed in the human body from two distinct sources,—from food in the intestinal canal, in the act of decomposition, and from the decomposition of the

tissues of the body itself. The gases produced in the intestinal canal occasionally permeate through its walls into the peritoneal cavity. The slate-gray colour of the surface of the spleen and liver frequently observed in *post-mortem* examinations is due to the action of sulphuretted hydrogen or hydrosulphate of ammonia which must have escaped in this manner.

This chapter concludes with a few observations on the disputed question—*whether different parts of the body can actually secrete gas*. On this point the author offers no decided opinion.

**DROPSIES.** The second chapter is devoted to the consideration of dropsies, which are divided by our author as follows:—1. *Serous dropsy*, or that in which the fluid is identical in its qualitative composition with the serum of the blood. 2. *Fibrinous dropsy*, or that which contains dissolved fibrin, and in its chemical composition resembles the liquor sanguinis. 3. *False dropsy*, in which the fluid differs essentially in its chemical composition from either of the preceding forms.

*Serous dropsy.* Dropsy in which the effused fluid corresponds with the serum of the blood is by far the most common of any of the above forms. Most cases of ascites, hydrothorax, hydrocele, anasarca, and œdema belong to this division, as likewise do the fluids of pemphigus, blisters, &c. Vogel gives a good sketch of the chemistry of the fluids effused in the form of dropsy; as however this subject is fully discussed in the chapter “on the fluid products of disease,” in Dr. Day’s edition of Simon; we must refer our Sydenhamic—that is, we hope, *all* our—readers to that work for information on the point. Then comes a section on the *causes and modes of origin of serous dropsy*, in which it is traced to a “want of balance between the porosity of the venous walls and the specific gravity of the blood contained therein; that is to say, it occurs when the venous walls become more porous, or the blood lighter, or more aqueous than in the normal condition.” In either case there is an increased transudation of serum through the walls of these vessels. This is the manner in which local dropsy invariably occurs where individual veins are compressed or in any way obstructed. It is thus that pressure of the impregnated uterus causes œdema of the feet and pressure on the *vena portæ* or *vena cava ascendens* produces ascites and œdema of the lower part of the body.

Although there is undoubted evidence that the blood under the influence of strong pressure gives origin to the effusion of dropsical fluids, yet we must not regard the process as a merely mechanical one. Why does not the fibrin which is dissolved in the liquor sanguinis enter into the dropsical fluid? And why, as a general rule, is it that these effusions contain as large an amount of salts, and more water, but less albumen than the serum of the blood? The difficulty of answering these questions shows us that the process is more complicated than at first sight it seems to be. A more accurate acquaintance with the relations of endosmosis than we at present possess is requisite to explain these points in a satisfactory manner.

*Fibrinous dropsy* is regarded by our author as being of more frequent occurrence than the serous form, although it has seldom been described, and has never hitherto received a special name. In its chemical composition the effused fluid is identical with the liquor sanguinis; it is serum

with dissolved fibrin. Vogel communicates seven analyses of fibrinous effusions, viz. one by Quevenne of the fluid of empyema, three by himself and two by Scherer of similar fluids, and one by Schwann of the fluid of ascites. Scherer's two analyses are recorded in p. 494 of the second volume of the English edition of Simon, which likewise contains analyses of similar fluids by Simon and Heller. In Schwann's case we cannot help thinking that there must have been some error in the chemical manipulation, the fibrin amounting to no less than 83 in 1000 parts of fluid, or rather more than twenty-four times as much fibrin as occurs in the liquor sanguinis. This fibrinous effusion is attributed by our author to the permeation of liquor sanguinis through the walls of the capillaries. In favour of this view he urges—1st. That the walls of the capillaries are much thinner and more delicate than those of the veins, and that the product of endosmosis will naturally be more concentrated and richer in solid constituents than in the latter case. 2d. That as serous dropsy is associated with dilatation of the veins and attenuation of their walls, so we learn from microscopic examination of the capillary system that a dilatation of those vessels and an attenuated condition of their walls precedes and is associated with the occurrence of the fibrinous fluid either in the parenchyma of an organ or in a cavity.

The fibrinous fluid is capable of organization, which is always effected at the expense of the fibrin contained in it. As far as development is concerned, it is indifferent whether the fibrin has coagulated within the body, or whether it has remained fluid, as in either case it acts equally well as a cytoblastema. There may be produced from it the most varied forms of tissue, either normal—as cellular tissue, simple muscular fibre, cartilage, bone, vessel, or nerve; or pathological—as pus, granular cells, cancer, tubercle, &c. Through this capacity for organization, fibrinous dropsy becomes the common source of a great variety of morbid growths.

*False dropsies* are made to include the cases of dropsy of the kidneys, gall-bladder, lachrymal sac, &c., which are common in the writings of the older pathologists. They are dependent on a closure of the duct of the secreting organ, and a consequent accumulation of the secretion.

**PATHOLOGICAL RELATIONS OF THE BLOOD.** Deviations in its physical characters, such as are directly obvious to the senses, are first considered; and, secondly, those of a chemical nature, which frequently require complicated processes for their detection.

Amongst the *physical changes*, the first in order is *change in colour*. After noticing the colour of the blood in cases of cyanosis and scurvy, our author remarks that he has "sometimes noticed venous blood of a bright red colour, with a shade of blue, much the tint that is developed on treating uric acid with nitric acid and ammonia. Thus, in the dissection of an arthritic subject, the blood of the renal veins presented this tint." The variations in colour are, however, not sufficiently constant to permit of our always finding the same tint in the same disease, as may be clearly seen by the subjoined table from the recent monograph of Popp\* on the Blood in various Diseases:

\* Untersuchungen über die Beschaffenheit des menschlichen Blutes in verschiedenen Krankheiten, von Dr. Karl Popp, Leipzig, 1845.



In 133 cases the colour of the blood was observed to deviate from the normal tint 41 times.

		Ordinary colour.	Very bright red.	Very dark brownish red.	Bluish red.
In 8 cases of simple plethora . . . . .		5	0	3	0
2 pregnancy . . . . .		2	0	0	0
4 irritation of the brain and spinal cord . . . . .		2	1	1	0
3 epilepsy . . . . .		0	2	1	0
2 puerperal convulsions . . . . .		0	2	0	0
2 hemiplegia . . . . .		2	0	0	0
1 poisoning by lead . . . . .		0	0	1	0
3 hypertrophy of the heart . . . . .		3	0	0	0
3 simple dilatation of the heart . . . . .		1	0	2	0
2 inflammation of the brain and spinal cord . . . . .		2	0	0	0
1 inflammation of the brain . . . . .		1	0	0	0
31 pneumonia . . . . .	25	5	5	0	1
6 bronchitis . . . . .	5	0	0	1	0
1 metritis . . . . .	1	0	0	0	0
1 inflammation of the eye . . . . .	1	0	0	0	0
1 erysipelas . . . . .	1	0	0	0	0
12 febrile and acute rheumatism . . . . .	8	3	3	1	0
3 simple rheumatic fever . . . . .	3	0	0	0	0
10 typhus . . . . .	2	3	3	0	5
2 scarlatina . . . . .	1	0	0	1	0
24 phthisis . . . . .	19	3	3	2	0
1 chlorosis, with dilatation of the heart . . . . .	0	1	1	0	0
1 scirrhus . . . . .	0	1	1	0	0
6 Bright's disease . . . . .	5	0	0	1	0

The serum, which in the normal state is clear, is sometimes opaque and of a milk-white appearance. This may depend on various causes. Firstly, on a large number of microscopic fat-globules; secondly, on the presence of a considerable quantity of minute granules of coagulated fibrin; and, thirdly (very possibly), on the formation of a free acid in the blood, whereby a portion of the albuminate of soda becomes decomposed, and albumen, in a finely granular state, is separated. The turbidity dependent on the presence of fat is sometimes observed in perfectly healthy persons, shortly after partaking of an abundant meal, when the blood is receiving a large quantity of chyle. Turbidity, arising from granules of coagulated fibrin, has been observed by Scherer in a pregnant woman with *bronchitis tuberculosa*, in a leuco-phlegmatic person suffering from attacks of vertigo, and in a spirit-drinker with cerebral congestion; by Simon in a man with Bright's disease; and by Zimmerman in several morbid affections.

Deviations in the *consistence, mode of coagulating, odour, and taste* of the blood are then considered, and the section concludes with a few remarks on *changes in the corpuscles*. The author has carefully collected all that is known on these points, but has contributed nothing original.

We now arrive at one of the most important subjects in the whole work, and one to which we have already devoted considerable attention in this Journal (Vol. XVII, pp. 136 and 427, and Vol. XX, p. 76); namely, *deviations in the chemical composition of the blood*. Since the publication of Andral's two memoirs in 1840 and 1841, and the works on animal chemistry reviewed in Vol. XVII, the contributions to the chemistry of healthy and diseased blood have been almost overwhelming. Simon's *Chemistry*, which was published (at Berlin) in 1842, contained, in addition to thirty-two of his own analyses of diseased blood, a full account of all that was then known and of all that had been previously done on the subject.

The English editor has added to these the whole pith of the elaborate memoir of MM. Becquerel and Rodier, besides numerous recent analyses by Heller, Scherer, Rinskoﬀ, Herberger, and others, and, in fact, has rendered the chapter on the blood a nearly perfect monograph on the subject up to the commencement of the present year. Although by no means so complete in its various bearings, Vogel's chapter on the pathological chemistry of the blood is still worthy of an attentive perusal by such of our readers as are familiar with the German language, although it contains little in the way of novelty requiring especial notice. His arrangement is excellent; and the following scheme will show that scarcely any important point in the chemistry of the blood is overlooked. The various morbid changes are divided into the two following groups:

I. Those in which any of the normal constituents of the blood are increased or diminished.

- a* An increase or diminution in the amount of fibrin.
- b* An increase or diminution in the amount of blood-corpuscles.
- c* An increase or diminution in the amount of water.
- d* An increase or diminution in the amount of albumen in the serum.
- e* An increase or diminution in the amount of salts.
- f* An increase in the amount of urea.

II. Those in which abnormal constituents are present in the blood.

- a* Free lactic acid.
- b* Carbonate of ammonia.
- c* A substance precipitable by acetic acid, resembling pyin.
- d* Sugar.
- e* Bile-pigment.

In addition to these, he mentions pus-corpuscles and entozoa as foreign organisms to be detected by the microscope rather than by chemical analysis.

In connexion with the determination of the blood-corpuscles by Figuier's method of analysis, which consists in retaining them on a filter after treating defibrinated blood with double its volume of a solution of sulphate of soda of spec. grav. 1.130, (see Day's Edition of Simon, vol. i, p. 190,) Vogel observes that he has always found the filtered solution of a somewhat red tint—a sign that the colouring matter of the blood is not perfectly insoluble in a solution of sulphate of soda. On the whole, however, he seems to approve of the process.

With regard to the abnormal constituents, free lactic acid has been found by Scherer in blood taken from the dead body in cases of puerperal fever and phlebitis, and on several occasions by Vogel, in the blood after death from miliary fever and rheumatism. We regret that no decisive proof of the true nature of the acid is given, as evidence for or against the occurrence of lactic acid in the animal body is just now of considerable importance. Enderlin, and indeed the whole Giessen school, deny that lactic acid can be formed in the human body; and we have the evidence of Pettinkofer that a zinc-salt may be obtained from the urine, resembling in all physical characters lactate of zinc, but differing in the circumstance of the acid containing a considerable amount of nitrogen. While on the

other hand, Berzelius, Pelouze, Goble, and Mulder as strenuously contend that, free or in combination, it occurs in almost every fluid of the body.

Carbonate of ammonia was found by Scherer in blood taken from the arm of a patient with typhus; and the substance resembling pyin was found by the same chemist in the blood of a woman who had died from metro-peritonitis. In his observations on the occurrence of bile-pigment in the blood, our author forgets to mention that the essential matter of bile (biliary resin, bilin, or choleic acid) has been detected by Orfila, Collard de Martigny, Clarion, Pettinkoffer, &c.

The remainder of the chapter is devoted to the consideration of *changes in the quantity of the blood, extravasation of blood, and the solution of the hæmatin and the consequent infiltration of the tissues*. These are all fully discussed and admirably illustrated by the Atlas.

We have now arrived at Chapter IV, which occupies no less than two hundred and eighty-three pages, and is devoted to the subject of morbid products and formations.

**MORBID PRODUCTS.** The following extract "on the development of morbid organic growths" will serve as a fair illustration of our author's plain, straightforward mode of handling his subject:

"It is dependent on laws essentially different from those of mere chemistry. This difference even exhibits itself in the formative material—the cytoblastema; which is most commonly fluid, but may, however, be solid. If solid, it must of necessity be amorphous; in fact, the only solid blastema for morbid products which has yet been observed is coagulated fibrin in an amorphous condition and infiltrated with water, such, for instance, as occurs in inflammatory exudation. But even this blastema was originally fluid, and only became solid through the coagulation of the fibrin. No other coagulated protein-compound, neither albumen, casein, nor globulin, has ever yet been observed to act as a cytoblastema. In the formation of morbid products from a mother-liquid, a solid condition of the plasma has never been observed to occur.

"As in the solid cytoblastema coagulated fibrin seems to act the leading part, so in the fluid cytoblastema dissolved fibrin appears of equally essential importance. This point is, however, of such importance as to require an accurate investigation. It is possible, in many cases, to isolate the fluid blastema of morbid products, and when it occurs in sufficient quantity to institute a chemical examination of it; as, for instance, in cases of exudation into serous cavities, or of a blister under the cuticle. In these cases we find it composed of water, fluid albumen and fibrin, fat, extractive matters, and various salts. That aqueous solutions of salts and extractive matters cannot of themselves alone act the part of a blastema to organized formations is beyond all dispute. They may, indeed, discharge the office of a mother-liquid; in many cases they may even enter into definite forms, as calcareous salts into bone, and chloride of sodium (according to Lehmann) into cartilage; but organized products can only be developed in them when a fibrinous fluid is also present, as, for instance, when exudation takes place into the uropoietic viscera or intestinal canal.

"The same is the case with regard to the fats, some of which admit of deposition in a crystalline form (as cholesterin in gall-stones), and may likewise enter as constituents into organized formations, but none of which, either alone or associated with salts and extractive matters, can act as a cytoblastema; at least, up to the present time no certain case of the kind has ever been observed. There remain, then, as the essential constituents of the blastema only the above protein-compounds, which, however, never occur alone in the body, but always in com-

bination with the above-named substances. But not all even of these are capable of higher development. Fluids which, in addition to the other constituents, contain no other protein-compound than dissolved albumen, never appear to assume the functions of a cytoblastema. Common as albuminous dropsical effusions are, they never give rise to organized forms, unless they also contain fibrin. I have made a very large number of observations on this subject, and have never met with a single exception to the above law. Moreover, a fluid containing casein as its sole protein-compound, seems to be incapable of acting as a cytoblastema. In the milk, for instance, as long as it contains mere casein, we never observe pathological formations, for the so-called *corps granuleux* are dependent on the normal development of milk; but if fibrin becomes mixed with it, morbid formations, as, for instance, pus-corpuscles, are produced. On the other hand, in all fluids which we regard as blastemata for morbid products, fibrin has been invariably detected. Hence we must regard it as the necessary and, apparently, the only essential constituent of the blastema. This law, respecting the necessity of the presence of fibrin in these cases, which I have seen to hold good in several hundred instances, without a single exception, is not at all in accordance with the course of normal development. Thus the egg—the prototype of all formative fluids—contains no fibrin, its place being apparently supplied by albumen. There may probably be some connexion between this fact and the observation of Mulder, that the albumen of the egg contains one atom less of sulphur than the albumen of the blood, and that, consequently, in its ultimate composition it is identical with fibrin. In the nutrition of the perfect organism, the general nutrient fluid—the modified blood-plasma permeating the walls of the vessels—acts as the general cytoblastema for all new formations. Whether the fibrin is the only essential formative material, or whether the albumen likewise takes part in development, is a question which cannot be answered with the same degree of certainty as in the case of morbid products, since the normal fluid of nutrition can never be obtained free from extraneous constituents in sufficient quantity to admit of exact analysis." (pp. 79-81.)

This is succeeded by a sketch of the cellular theory which has been so frequently brought before the notice of our readers that, although Vogel has introduced a considerable amount of original matter, we shall pass on to the consideration of special morbid products. We must not, however, omit to mention, that the cell-theory is excellently illustrated in Plate 1 of the Atlas, which, in addition to a diagram of a cell on a large scale, contains figures of the cells of carcinoma, tubercle, pigment, &c.—in all, twenty figures.

*Fluid Products. Pus.* The first of the morbid products specially considered is *pus*, and we have no hesitation in stating, that in the thirty pages devoted to this subject every fact of importance to the pathologist will be found recorded. The physical characters of normal pus (*pus bonum et laudabile*) are briefly noticed, and we then arrive at the most perfect description of the pus-corpuscle that has ever yet fallen under our notice. After a description of its size, form, and general appearance, we come to the history of its structure, which we shall give at length:

"The corpuscles of normal pus are organized forms, for the most part of a cellular nature, with a nucleus, cell-wall, and contents.

"The cellular structure with a decided nucleus is only apparent in unchanged corpuscles when the cell-wall is very transparent and delicate. In the majority of cases the nucleus is covered by a granulated opaque cell-wall, and does not become visible till the latter is dissolved or rendered transparent by acetic acid.

"In other cases in which the development of the pus-corpuscle is imperfect, we see only the nucleus and no cell-wall.

"The nucleus does not lie in the centre of the pus-corpuscle, but is situated eccentrically, and is usually attached to the inner surface of the cell-wall. We may convince ourselves that this is the case by allowing pus-corpuscles to float and rotate under the microscope. It is only the larger nuclei that form an exception to this rule, for they are occasionally so large as to occupy the whole space of the pus-cell. The nucleus of the pus-corpuscle presents many peculiarities, and is so different from other nuclei as to require a somewhat careful consideration.

"In other cells, the nucleus is a simple body; but in the pus-corpuscle this is not always, nor indeed generally, the case. It is usually composed of from two to five granules, forming a composite nucleus. Sometimes, on treating fresh corpuscles with acetic acid or with a solution of common salt, we can observe a single nucleus indented like a trefoil leaf, or cloven into two, three, or four smaller nuclei. But it is not every nucleus that undergoes this change; in some cases it appears to resist the action of these reagents. The large corpuscles, with a diameter varying from the 100th to the 80th of a line, exhibit several (two, three, or four) such nuclei, each of which is composed of smaller bodies, insoluble in acetic acid.

"The corpuscles forming the nucleus, when, by the addition of acetic acid, they are clearly brought before us, present various forms; sometimes (generally in good pus) they are elliptic, and present an excavated cup-like form, resembling fresh blood-corpuscles; sometimes, however, they present a spherical or oval appearance. In some cases they are distinct from each other, even lying in different parts of the cell-cavity; but they are more frequently in apposition, and connected together.

"This composite character of the nucleus (as revealed to us by the action of acetic acid) is highly characteristic of normal pus. The only other cases in which it occurs are in young gland-cells, and in the most recent layers of pavement-epithelium; but here they are not so general as in pus. Hence it follows that the size of the nucleus is liable to great variations—from being entirely absent, it may fill the whole cell. Its usual limits are from the 800th to the 400th of a line. Nucleoli are but rarely found in the nuclei of pus-corpuscles.

"The *cell-wall* varies in thickness, and surrounds the nucleus more or less closely. In very delicate and young corpuscles it is extremely thin, smooth, membranous, and transparent; in older and certain peculiar sorts of pus, it is thick, tough, opaque, and studded with minute granules. In many cases the corpuscles have no distinct cell-wall, consisting merely of a nucleus and an irregular deposition around it, without any clearly defined outline, as may be shown, not merely by microscopic investigation, but by its relations towards endosmosis. This is especially the case with young, imperfectly-formed corpuscles.

"Moreover, there are many differences in the *contents of the cell*. In pus-corpuscles with a single nucleus, a well-marked membranous cell-wall, and consequently a cavity between them, we often find no solid body in the cell-cavity except the nucleus: the contents must therefore be fluid, and are doubtless identical with the serum of pus. In other cases, in addition to the nucleus, granular contents, with independent chemical relations, are observed. Sometimes the contents seem so thoroughly fused into the cell-wall, that the two form only a single substance—a solid, but soft mass, in which the nucleus is imbedded." (pp. 107-10.)

The endosmotic and chemical relations of the pus-corpuscle are then considered, and the individuality of the following parts of that organism establish:

1. The *substance of the capsule*, which is soluble in solutions of the caustic alkalies and their carbonates, of borax, and for the most part in saline solutions, as those of muriate of ammonia, nitrate of potash, &c.; in part also in acetic acid. It forms the wall and a portion of the contents of the cell, and is doubtless a protein-compound, very similar to and pro-

bably identical with that modification of albumen which is precipitable by water, and is again dissolved by the addition of neutral salts or acetic acid.

2. The *substance of the nucleus*, which is insoluble in acetic acid, swells in saline solutions, and dissolves in solutions of borax, of the caustic alkalies, and their carbonates. This likewise is a protein-compound, but its exact nature is doubtful.

3. The substance of which the *minute molecules* consist, which remain undissolved on treating pus-corpuscles with solutions of the caustic alkalies or of borax. Lehmann and Messerschmidt regard this as a protein-compound analogous to keratin (or horny matter,) and in many cases this seems to be true. Sometimes, however, these molecules consist of fat; they are then soluble in ether, and if the pus-corpuscles are boiled in that menstruum previously to the addition of the alkali, these molecules do not make their appearance.

Many kinds of normal pus contain nothing in suspension besides the regular corpuscles; others, on the contrary, contain minute roundish molecules, often in a very considerable quantity. They are always very minute, for the most part less than the 1000th of a line in diameter, and swim (either alone or in heaps) in the serum and between the pus-corpuscles, which are frequently studded with them. These molecules are sometimes protein-compounds,—probably abortive pus-corpuscles; in other cases they consist of fat.

Finally, there are sometimes infusoria present, minute *monades* and *vibriones*, especially in pus from foul ulcers. By feeding them with carmine, Vogel was sometimes able to bring into view the minute specks representing their stomachs.

Moreover, we sometimes find accidental ingredients, as for instance epithelium-cells, fragments of epidermis, crystals of cholesterin or of ammoniaco-magnesian phosphate, or flocculi of amorphous or semi-organized fibrinous exudations. These can be easily detected by the microscope.

In his observations on the serum of pus, the author seems to attach little importance to the substance described by Guterbock under the name of *pyin*, and regarded by that chemist as characteristic of pus. Vogel observes that it is much more commonly found in bad than in good pus; and further, that it occurs in other morbid products, as for instance in carcinoma. After several chemical analyses of pus, we arrive at the important subject of *its formation*, to the consideration of which nearly six pages are devoted. The two following points may be regarded as fully established: 1. Pus-corpuscles cannot be formed from albumen alone. 2. When the full development of the pus-corpuscles is completed, the fibrin of the plasma is consumed, and the remaining serum resembles the serum of the blood, or the fluid of serous dropsy.

*Diagnosis of normal pus.* The following remarks on this subject are well worthy of attention.

“The recognition of this morbid fluid is apparently so easy that any one after once seeing it and observing the above physical properties would trust himself to distinguish whether or not a fluid were really pus; and yet there are numerous sources of deception. Fluids containing fragments of epithelium in suspension may



readily be mistaken for pus, and on examining the body after death we sometimes believe that we have discovered suppuration, when in fact no such morbid process has been going on. The examination of the fluid under the microscope is the best and, indeed, the only certain means of guarding against such deceptions. If that instrument reveals the presence of normal pus-corpuscles, and on the addition of acetic acid the characteristic nuclei appear, then we may be assured that we have been examining true pus. As I have repeatedly witnessed such deceptions, I will, by way of warning, mention two cases. A woman died from pleuritis with considerable purulent exudation into the pleural cavity. On examination I likewise found in the pelvis of the kidney and the ureter on each side a whitish yellow, thick, creamy fluid, which had all the physical characters of pus, and was mistaken for that fluid by the physicians who were present. As during life there were no symptoms of disease of the kidneys, and as dissection did not reveal any morbid change in those organs, the case was regarded as a demonstration of the resorption of pus, and of its subsequent removal by the kidneys. I examined this assumed pus microscopically, and found in it no trace of pus-corpuscles, but merely broken epithelium from the pelvis of the kidney and ureter.

"In another case, in which the patient died from peritonitis with exudation, the stomach and upper part of the intestinal canal were perfectly free from remnants of food and chyle, but contained a large quantity of a thick yellow fluid, which was mistaken for pus. In this case also the microscope showed that no pus-corpuscles were present, and that the fluid merely contained the broken cylindrical epithelium of the intestinal canal." (pp. 121-2.)

We fully agree with Vogel, that the microscope is of far more service in the detection of pus than all the so-termed pus-tests; further, it enables us not merely to distinguish pus from mucus, broken epithelium, blood-corpuscles, &c., but likewise to determine approximately the amount of those different substances, which chemical analysis has never succeeded in doing. The characters of Gluge's *compound inflammatory globule* are then noticed, and as we believe that very incorrect ideas are frequently maintained on this subject, and that too great importance is often attached to its occurrence, we shall devote a few lines to Vogel's description of it. Vogel objects to the term *inflammatory globule*, because its connexion with the process of inflammation is not more intimate than that of various other organized formations occurring in exudations; and further, because it is found under conditions in which there is very little probability of its resulting from inflammation—as for instance, in cysts in the thyroid gland. He prefers the term *granular cells*. When these granular cells occur in a perfect condition, they vary in diameter from the 200th to the 80th of a line; some are perfectly round, others oblong, irregular, and even angular. They appear to be agglomerations of minute granules, varying in diameter from the 800th to the 1000th of a line. The following is their mode of formation, as observed by Vogel in inflamed lungs. Cells with a nucleus and nucleolus, differing from pus-corpuscles in their larger size and in having a simple nucleus, are formed in the exudation. These become gradually filled with minute granules, which at first, when only few in number, readily admit of the nucleus being seen; subsequently, however, they conceal it, and the originally smooth cell-membrane becomes rugged, and the angular cell appears as a spherical agglomeration of molecules. Subsequently, the cell-wall appears to vanish, the inclosed granules separate and form irregular heaps, while each granular cell undergoes in miniature the identical process which a mass of coagulated fibrin undergoes in its

conversion into pus-corpuscles. This is regarded by Vogel as their usual mode of development; he believes, however, that occasionally the process may be reversed,—that isolated elementary granules are first produced, which collect in groups, and are finally invested with a cell-membrane. Numerous varieties of pus-corpuscles and granular cells are delineated in Plate III. of the “*Icones*.”

*Solid products.* The solid morbid products next claim our attention. Before proceeding to the consideration of the more strictly morbid textures we have a section extending over upwards of thirty pages, on the regeneration of normal tissues. There are successively considered the epigenesis of areolar tissue (*Bindegewebe*), of blood and vessels, of epithelium and epidermis, of fat and adipose tissue, of muscle, of elastic tissue, of granular pigment, of nervous tissue, and, finally, of cartilaginous and osseous tissues. The section is intended as an introduction to the important subject of *tumours*, which occupies the succeeding 128 pages.

In a histological point of view, tumours may be arranged in two great divisions. In the first, we place those whose histologic elements are identical with those of the normal body, and, further, which when once formed discharge the same duties as the other parts of the body, undergoing metamorphosis of tissue, and receiving their nourishment like a normal portion of the frame—*homologous, non-malignant tumours*.

In the second division we place those whose elements differ histologically from those of the normal body, and which from their nature give way, soften, and destroy the adjacent parts—*heterologous, malignant tumours*.

*Non-malignant tumours* are subdivided by Vogel into—

1. Tumours consisting principally of vessels—vascular tumours.

As a good account of these growths may be found in one of the recent volumes of the Sydenham Society (Swaine's edition of Hasse's “*Pathological Anatomy*,” p. 102,) we proceed without comment to

2. Tumours consisting principally of fatty tissue—adipose tumours. A true fatty tumour may, according to Vogel, become converted

α. Into general infiltration of fat (*polysarcia, obesitas*) by local hypertrophy of the adipose tissue.

β. Into fibrous tumour by the addition of areolar tissue.

γ. Into encysted tumour by the formation of a cyst around it.

In relation to diagnosis, it must be remembered that many forms of malignant tumour (encephaloid) in their physical characters closely resemble fatty tumours, and can only be distinguished from them by microscopic examination.

3. Tumours consisting principally of fibrous tissue—fibrous tumours.

The following experiment throws some light on the origin of these tumours. Vogel injected several ounces of a solution of hydrosulphate of ammonia into the abdominal cavity of a large dog, and immediately closed up the orifice. At the expiration of twenty-four hours he was killed, and on examination there was found an amorphous exudation of coagulated fibrin on several parts of the intestinal canal under the peritoneum, and blood was extravasated between the muscular and serous coats. On the anterior surface of the stomach there was a coagulum of blood, as large as a hazel-nut, surrounded by a thick layer of fibrin, and firmly attached to the outer wall of the stomach. “I am firmly convinced,” adds Vogel, “that

this coagulum would in time have been converted into a fibrous tumour, if the animal had not been killed. I have had frequent opportunities of making similar observations. This appears to illustrate the probable origin of fibrous tumours in man—at least of such as occur in the stomach, the intestinal canal, and more especially in the uterus, where there are more frequent opportunities for the formation of coagula.”

4. Tumours in which cartilage predominates—cartilaginous tumours.

5. Tumours in which osseous tissue predominates—osseous tumours.

6. Tumours consisting in whole or in part of dark pigment—melanotic tumours. The pigment entering into these tumours is by no means uniform in its characters. In many cases it consists of dark granules inclosed in round or oval cells, sometimes it is modified hæmatin, and it is occasionally composed of granules of sulphuret of iron. The first constitutes true, the two last, false or spurious, melanosis. The pigment is never the sole constituent of these tumours, the other elements being fibrous tissue, a few vessels, and not unfrequently malignant growths, as tubercle, encephaloid, or scirrhus. In true melanosis the colour is brown (of a bistre tint), blackish, or if only a little pigment is present, gray; in the false variety resulting from altered hæmatin, it is blue, violet, or brownish-black; and in that depending on sulphuret of iron it is of a slate-gray, or greenish-black. Melanotic tumours are more frequently observed in the female than the male sex.

7. Tumours containing gelatinous matter—gelatinous tumours; which are described by Vogel as follows:

“In many tumours there occurs a viscid jelly-like substance, partly infiltrated amongst the firm elementary tissues, and partly contained in appropriate spaces or cavities, sometimes in such abundance, and so greatly exceeding the other elements, that the tumours may with great propriety be termed gelatinous. The elements coexisting with gelatine in these tumours are various, generally fibres, vessels, and occasionally cartilage, and most commonly of all, cancer-cells forming gelatinous cancer—*colloid*.”

“This substance is always transparent and colourless, sometimes fluid, resembling thickened mucus; at other times resembling half-fluid jelly. Under the microscope it appears completely amorphous, and so perfectly transparent that it is not easy to see it. In six cases in which I have examined it, its physical and chemical characters appear constant and identical with those of mucus. . . . Nothing can as yet be stated with certainty respecting the origin of this substance; it arises, however, in all probability, like normal mucus, from modified protein-compounds of the blood.” (pp. 204-5.)

8. Tumours inclosed in a true cyst—encysted tumours.

The *malignant tumours* are divided into two leading groups. The former embraces those in which little (or no) organization is apparent, namely:

1. *Typhous deposits*.

2. *Scrofulous deposits*.

3. *Tubercle*.

The second includes the more highly organized secondary formations; namely, *carcinoma*, subdivided into

1. Cellular cancer—encephaloid.

2. Fibrous cancer—scirrhus.

3. Melanotic cancer.

4. Gelatinous cancer—colloid.

We pass over these subjects without comment; the character of the typhous deposit having been noticed in our review of Rokitansky, in Number XXIX, and cancer fully discussed in our recent review of Professor Walshe's 'Treatise.'

The remainder of this lengthy chapter is devoted to the consideration of unorganized morbid products.

The following scheme includes the various substances capable of occurring as deposits in the human body:

1. Protein-compounds.
2. Fats: *a*, cholesterin; *b*, margarin and margaric acid; *c*, olein; *d*, fatty granules of uncertain composition.
3. Uric acid and urates.
4. Salts of lime: *a*, oxalate of lime; *b*, basic phosphate of lime; *c*, carbonate of lime.
5. Ammoniac-magnesian phosphate.
6. Sulphuret of iron.
7. Bile-pigment.
8. Silica.
9. Various substances of easy solubility, deposited in consequence of evaporation, &c., as chloride of sodium, phosphate of soda, &c.

From one or more of these constituents are formed all the concretions occurring in the human body. These are arranged by Vogel into

1. Such as are produced from the secreted fluids.
2. Such as are formed in the parenchyma of organs.

The subject is completely treated in all its bearings, but contains little with any claim to novelty. The analyses of the concretions are for the most part taken from Simon.

We now arrive at a short chapter of twenty pages on the morbid changes in the physical properties of the tissues and organs. Changes of colour, volume, form, and consistence, are discussed, and a considerable amount of original matter introduced.

After a very brief and somewhat satisfactory chapter on the combinations of morbid changes, we come to the consideration of *independent organisms in the human body—parasites*. These are divided into *Epiphytes* and *Parasitic animals*.

The *Epiphytes* are subdivided into (1) vegetations in the human fluids, namely, the *torula cerevisiæ* in vomited fluids and fæcal evacuations, and Goodsir's *sarcina ventriculi*; (2) vegetations on the external skin and its appendages, namely, in *Tinea favosa* (Gruby, Fuchs, Bennett), in the sheath of the hair in *mentagra* (Gruby), within the root of the hair in *Herpes tonsurans* (Gruby, Hebra), and in *Plica polonica* (Gunsburg); and (3) vegetations on the mucous membranes, as, for instance, in the aphthæ of children, in the cicatrices of the mucous membrane after typhus, &c.

*Parasitic animals* are subdivided into infusoria, insects, arachnida, and worms.

The following experiment, in relation to infusoria in the blood, is interesting, although leading to a negative result. Two ounces of water, containing millions of infusoria of the same species (either *monades* or the young of the *cyclidium glaucoma*), were injected into the vein of an adult cat. At the expiration of twenty-three hours not a trace of the infusoria

could be observed, and when the animal was killed, two days subsequently, the search was equally unsuccessful. If Siebold's\* article on parasites is studied conjointly with the present chapter of Vogel, the reader will acquire an immense amount of information that may in vain be sought for in English or French medical literature.

Congenital changes in the human body are then considered. The causes of malformation are, according to our author, the following :

1. Abnormality of the generative material of one or both parents.
2. Abnormality of the maternal organism, pathological changes in the uterus, fallopian tubes, &c.
3. Diseases and abnormal conditions of the placenta, membranes, or cord.
4. Pathological affections directly affecting the foetus—diseases and mechanical injuries.

The different forms of malformations are arranged in eight classes, most of which are divided into orders and genera. The difficulty of condensing this subject within the limits of some forty pages will be at once recognized by those who are conversant with the voluminous works of Meckel, Geoffroy St. Hilaire, Otto, Gurlt, Vrolic, &c. The present chapter (although little more than a syllabus) contains infinitely more available matter than the works of any one of the authors just quoted.

The part now before us concludes with a chapter on the changes occurring directly after death. Like all that precedes it, it is well worthy of perusal, but contains no especial novelty either in the way of arrangement or material.

From our previous knowledge of the writings of Vogel, and from the high reputation that he has acquired during the last seven or eight years (and pathological histology is of no older birth), we took up our pen with a decided prepossession in his favour. If we have been slightly disappointed in not finding so many original observations as we expected, that disappointment has been more than counterbalanced by the philosophic spirit pervading the whole work. Unlike Lebert, whose work is noticed in our last Number, he strives not for glory but for truth, and every page shows his desire to render to all men their due. Here we have none of the self-gratulation and assumed superiority for which we were compelled to inflict a moderate castigation on the pseudo-Frenchman ; on the contrary, we can lay our hand on several pages in which M. Vogel desires to recant or modify opinions he had previously adopted ; and the truthful singleness of mind he displays cannot fail to have its due weight with his readers. In his knowledge of the foreign literature of his subject, we may express our decided conviction that he stands unrivalled. Carswell and Hope, Owen and Liston, Simpson and J. H. Bennett, are but a few of the British names that meet us in his pages. France, Italy, Holland, and Denmark,—all lay open their stores before him, and he gathers as freely from Cruveilhier and St. Hilaire, Cerutti and Bassi, Vrolik and Broers, Hannover and Steenstrup, as from his own countrymen Müller, and Henle, and Gluge.

We have had occasion, in several parts of this review, to notice the

\* Wagner's *Handwörterbuch*, &c., vol. ii, pp. 641-92.

Atlas; but if we spoke of it as a mere volume of illustrative plates we should be doing its author a great injustice. Of the 291 figures contained in it, 270 are drawn from nature, and accompanied with copious explanations extending over 128 quarto pages. Of the accuracy of most of the figures we are enabled to speak from experience.

We are glad to learn that this work is likely soon to appear in an English dress, from the competent pen to which we are indebted for the excellent work of Simon, recently issued by the Sydenham Society. We trust that Dr. Day will incorporate in it the most important cases from the 'Icones,' and have it properly illustrated. We do not doubt that a work of this nature, with microscopic delineations of morbid products, would supply a want which has been long experienced in this country.

#### ART. V.

*Geschichte der Findlinge in Oesterreich mit besonderer Rücksicht auf ihre Verhältnisse in Illyrien.* Von Dr. RAIMUND MELZER, K. k. Director der Staats- und Local-Wohlthätigkeits-Anstalten zu Laibach.—*Leipsig*, 1846.

*History of Foundlings in Austria, with especial reference to their Condition in Illyria.* By Dr. RAIMUND MELZER, Imperial Director of the State and Local Charitable Institutions at Laybach.—*Leipsic*, 1846. 8vo.

THE author of this able and learned work is unusually well qualified for the task he has set himself. Trained in the service as well as in the superintendence of the commission for inquiring into the state of the poorhouses and houses of correction, then acting in the capacity of professor of midwifery and physician to the lying-in and foundling hospitals, and, lastly appointed to the responsible post of director to the state and local charitable institutions, he has been diligently employed, during ten years, in the performance of duties which have familiarized him with the causes and effects of poverty. It was natural that the institutions with which he was most intimately connected, first as physician, and subsequently as director, should engage the greater share of his attention. Accordingly, it scarcely required the stimulus of the example of the prize essays of MM. Terme and Monfalcon, of which the reader will find elaborate notice in the Twenty-sixth Number of our Review, to urge him to the laborious inquiries which have issued in the work before us—a work for which his habitual occupations, and his access to public documents, peculiarly fitted him.

His mental qualifications for his task will appear of no mean order to any one who will take the pains to peruse his Introduction, in which he shows that he has not been a careless observer of poverty and the poor, and that the great and perplexing alternations which have beset all national efforts for the relief of indigence have not escaped him. These alternations have rarely been better expressed than in the following passage :

“Poverty has the great misfortune that it borders closely on vice, and easily passes into it. Nay, we may call vice a moral poverty. Two steps further, and



the pauper becomes the criminal. If charity is not circumspect, it will be deceived and cheated by vice or crime, and innocent poverty will be robbed of the assistance which was intended for it. How easily this may happen, may be inferred from the familiar fact that natural benevolence is easy of belief, and from the mistakes which philanthropic persons are continually making. As often as poverty is mixed up with vice it is in a state of resistance against law, for vice is in its very nature lawless. This circumstance is the source of many evils with which even the best administration of Foundling Hospitals has to contend, and is the reason why several governments exclude those establishments from the lists of charitable institutions, and esteem them a greater evil than the existence of those whom they are intended to assist."

Our author shall state in his own words the case of foundling hospitals, and shall exhibit the way in which the charity which presided at their foundation has been and is deceived and cheated :

"The Christian pity," he says, "which called these institutions into being, went so far in its anxious care for mother and child as in their reception to make itself blind. These institutions had not existed long before they became the dupes of vice. They were established to preserve purity of morals, and they soon began to undermine them. The stricter the principle of secrecy was preserved, and the further it was carried, the more shamelessly was it abused. Parents, bound by the ties of marriage, forgot their most sacred duties, and relieved themselves of children whom they regarded as heavy burdens; and they did this because the regulations of the institution tempted them to it—because they saw themselves thereby exculpated from a punishable offence, and even justified in their unnatural conduct. The foundling hospitals were crowded with applicants, and the expense increased with their increasing numbers till the evil reached an alarming height. To prevent this monstrous outlay from increasing, and, if possible, to effect a permanent diminution of expense, without endangering the original humane aim of the institution, formed the problem; the solution of which became the anxious wish of the government and of every friend of humanity."

And is not this the history of all charities without exception, and especially of all so-called national charities? Let us suppose a large town, in such a supposition be reasonable, in which no such thing as a beggar is to be found, but it is known that a large number of its inhabitants are poor, many in want, and some few, perchance, starving. A benevolent person, aware of these facts, proclaims them, and appeals to his fellow-citizens in behalf of the sufferers, insisting that they ought to carry purses in their hands, and invite the destitute to apply to them for alms as they walk the streets. What would be the consequence? Why, what other result could follow but that which has followed from the habit of dropping halfpence into the hands of street beggars—the creation of a band of masqueraders, who make mendicancy their profession, and come to think it to the full as honest as working with their hands, and somewhat safer than stealing, but who are much more likely to forget the danger of petty larceny than to be reminded of the safety and respectability of honest labour. And what is the total result of this misguided charity? This: to draw off, by the constant dropping of halfpence and pence into idle and unworthy hands, the fund which was destined to the support of honest labour, and which, so applied, would have rescued a greater number of deserving people from want, and saved more victims of threatened starvation than all the idle almsgiving in the world. It would probably

have prevented, and most certainly have contributed to prevent, that very distress which brought the idle habit of street-almsgiving into existence.

At length the public, though slow to learn, discovers the error into which it has been led, and street-begging begins to be discouraged. Naked feet, bandages, and tattered fancy dresses are at a discount, the Mendicity Society is in favour, and charity takes a new direction. She has been imposed upon in the street; she will try the houses. There, at least, she must be safe. She can inquire into the particulars of every case of destitution and suffering, and her purse-strings shall not be drawn till she has satisfied herself that her eyes are not deceived, nor her heart melted by the trick of the theatre. Poor dupe? Broken chairs, tattered and scanty bedclothes, and all the marks of domestic poverty and wretchedness, have replaced the rags and bandages of the street, and the affected piety of the home hypocrite and masquerader, the whine of the street-beggar. And here, too, the funds which might have ministered to the support of honest industry, and the relief of real want, are diverted from their proper channel by the treacherous arts of the house-beggar. Take one other case. This nation piques itself upon its poor-law, and on its generous determination that no man in the whole length and breadth of England shall be allowed to starve. Good. Britannia looks well in the attire of my Lady Bountiful. But is she quite sure that she does more substantial good by her annual expenditure of millions—that is to say, more good in proportion to the outlay—than the patron of street-beggars or home-impostors? Is out-door relief more free from imposition than street-almsgiving, or the union system much more free from abuse than the district visiting societies? Are not the millions expended every year as certainly drawn from the fund which would support honest industry and *prevent* destitution? and is the total good effected in any degree proportionate to the total expenditure? A fact was recently mentioned in the course of a debate in the House of Commons, which, strange to say, seems to have passed unnoticed by those who have the eye of a lynx for any abuse of the poor-laws, provided it be only traceable to the faults of their administrators. As many as four thousand pensioners were distinctly stated to have squandered their pensions in drink, and then to have entered the workhouse. Of course they did. What better could be expected of a certain considerable portion of society than that they should avail themselves of the houses so kindly opened for the encouragement of idleness, intemperance, and profligacy. Then what an instrument of rural tyranny that same workhouse system is! How convenient the threat that if wages, brought to the level of starvation, be not accepted, there is the workhouse hard at hand, where the honest labourer shall be introduced to all the idleness and profligacy of his neighbourhood, and supported (cruel mockery!) by the very funds which ought to have contributed to pay him a fair day's wages for a fair day's work.

We have been provoked to say more than our readers might expect to find in a Medical Review on this great question of so-called charity, individual and national, and to throw out these evidences of a scepticism as to the real good effected by its means, by our author's unjust censure of a man who, whatever his faults as a reasoner, cannot justly be accused of any

want of humanity, in spite of the severe and, in some respects, well-merited strictures of Sadler,—we mean Malthus.

The theory of Malthus was, doubtless, a false theory, and his fears of an increasing population idle and visionary; but his opposition to the poor-laws, and his assertion that no man has an abstract right to support, whether he will or will not work, may be, in reality, in its necessary results, far more humane than the easy and good-natured theory which it opposes. The question really is, whether the funds devoted to charity effect more in the way of cure than these same funds, employed in the support of labour, would accomplish in the way of prevention. Malthus may have been wrong in upholding the latter alternative; but it does not follow that his error, if it be one, was the offspring of an unfeeling disposition.

With this short protest against our author's disrespectful notice of the tenets of Malthus, and without stopping to qualify our own broad strictures on the evils of many of our modern charitable institutions, we proceed to a more intimate examination of the question of Foundling Hospitals. Considered in the light of charities, they are peculiarly open to the abuses which cling to all ill-considered plans for the relief of suffering and destitution; and it happens, fortunately, that the shrewd sense and right feeling of Englishmen have, very generally, condemned them; though with the narrow and exclusive views which, unfortunately, characterize highly practical nations as well as highly practical individuals, the merited censure they have received has not been extended to other so-called charities open to similar, if not greater abuses. At first sight it would seem an unmixed good, as well as an obvious Christian duty to succour and support such children as may be found abandoned by their unnatural parents; but this duty must have some qualifications, and this good some limits. What virtue or what blessing has not? Where, then, are these qualifications and limitations to be found? The answer is easy: when the practice of a duty, or what appears to be such, entails more evil and more suffering than it relieves, it is obvious that we have been acting under a mistake, and that we must abandon or modify our course of procedure. The point at which the evil predominates over the good is evidently the limit which we ought not to pass beyond. In the case under consideration, the object we have in view is the preservation of human life.

This just and sacred object we, undoubtedly, accomplish so long as in our reception of the foundling we hold out no encouragement to illicit intercourse, or the multiplication of deserted children. But no sooner do the measures which we adopt, whether as individuals or communities, act as an advertisement of our charitable intentions and actions, than deserted children begin to increase till, at length, they are equal to the utmost demand which our philanthropy has created. Let us take the extreme case, that children of the tenderest age are received at all hours of the day and night, without question or limitation, as was the case, till recently, in France, under the liberal system of *tours*; and it follows, inevitably, that we are the indirect means of destroying more lives than would have been lost, had every foundling been suffered to perish in the streets. For we contrive, at one and the same time, to hold out an

encouragement to illicit intercourse, and to the barbarous desertion of legitimate children; and by bringing so many children together under circumstances so unfavorable to infant life, we are the instruments of a more wholesale destruction of life than could have happened, had the comparatively few deserted children been left to the casual care of individual philanthropists, or to the fate to which their unnatural parents had abandoned them.

The suppression of the *tours* was, doubtless, a great improvement on the old system; but even with this improvement it admits of grave doubt, whether the encouragement held out to illicit intercourse, on the one hand, and the mortality incident to the best regulated foundling hospitals, on the other, do not lead to a larger number of deaths than would have happened had the matter been left to take its own course.

But we have entered quite as much at length into this subject as our limits will allow; we are bound to proceed to the further notice of the historical and statistical details prepared for us by the industry and research of our author.

The history of the establishment of foundling hospitals in Austria closely resembles that of France. A citizen of Laybach, one Peter Bellach, in the year 1041, devoted the whole of his property to the establishment of an orphan-institution, which was also to receive foundlings, and to train them to a trade. The example of Laybach was soon followed by other towns, and, at length, in Austria as in France, the foundling hospitals fell under the superintendence of government.

The history of legislation on this important subject forms the subject-matter of 170 pages, which we pass over, as it scarcely admits of being rendered interesting to the medical reader; all that can be of any importance to him will be found in the following notice of the statistics of the subject, which occupies the remainder of the volume.

*The number of foundlings*, in the several foundling hospitals of the Austrian dominions, has increased, during the twenty years from 1820 to 1840 in an arithmetical progression, and has doubled within that period. The average yearly number amounted to 49,317. In the year 1839 the population of Austria, exclusive of Hungary and its dependencies, was 23,389,959; the number of foundlings 59,026, being 1. in 396. The total in the twenty years amounted to 986,345, or little short of one million! In the years 1833–40, the total number of new-comers amounted to 128,658, or an average of 16,082. The numbers received in 1840, bore to those received in 1833 the proportion of 116 to 100; Galicia is excluded from this calculation.

In Laybach the increase of foundlings has been very considerable. In the year 1763 one foundling was admitted; in 1842, eighty years later, the number amounted to 201; in the first twenty years there were 77 admissions, in the second twenty years 962, in the third 1439, and in the fourth 3275. The increase of females received into the lying-in institution in the same town was also considerable and progressive, though not so great as in the case of foundlings; the out-patients fluctuated, but showed, on the whole, a tendency to increase.

What is the cause of the increasing number of foundlings? is a question of considerable interest to which our author devotes a short chapter. The

number of foundlings, as we might anticipate, is chiefly recruited from illegitimate births, which, as a general rule, increase with advancing wealth and civilization. In accordance with this acknowledged principle, it is found that the number of foundlings is least in the poorest districts; poverty, therefore, is not the cause of the abandonment of children. This position is abundantly proved by an appeal to facts. The most prosperous years are shown to be those in which the number of foundlings is greatest, while years of scarcity and war have often been marked by a decrease in number. Returning prosperity, also, is marked by an increase of foundlings, and the periods of the year in which there is the greatest distress, the winter months, are characterized by no marked influx of deserted children: thus, in Laybach, the number of foundlings in July and February was within an unit of the same. In Krain the number of deserted children was greatest in the month of May; March and April came next in order, then August. In the foundling hospital of Vienna, the months stood in respect of births, in the following order, beginning with that in which the number of foundlings was the greatest,—February, March, May, January, April, October, December, September, June, November, August, July; but the greater number of foundlings were, at Vienna also, received in summer.

It would appear then, that vice is more influential in increasing the number of foundlings than poverty, and that this species of vice at least increases with wealth and civilization.

The next question which Dr. Melzer examines is the proportion of foundlings born in wedlock and deserted by their parents. We were scarcely prepared for so startling a result as that which he has deducted from the records of the foundling hospitals of some of the Italian cities. During the period of ten years, from 1830 to 1840, there were received into the *tours* of the hospital at Milan 26,147 foundlings, of which 6,610, or 25 per cent., were legitimate; in Vienna, for the same period, there were only 111 in 3424, and in Brescia, 1744 in 20,148. In Protestant countries where there are no foundling hospitals and no *tours*, these children would not have been deserted by their parents; the Roman Catholic system, as our author observes, must, therefore, bear the blame of this increase of foundlings.

Another abuse of the foundling system is the abandonment of children by mothers, who offer themselves, immediately after, as nurses, in the hope of being paid for tending their own children; this abuse was pointed out, as occurring in the French hospitals, in the review to which we have already referred.

The mortality in childbed and that of foundlings next engages the attention of our author. The mortality in childbed at the lying-in institution at Laybach, has been very low compared with that of many other similar establishments in Austria. While it has been 6 per cent. in Vienna, 5 in Milan, 3 in Lemburg, 2 in Prague, Linz, and Venice, it was less than 1 per cent. in the Tyrol, and 80 in the 1000 at Laybach. This high mortality of Vienna is attributed, and with apparent justice, to overcrowding; and our author strongly recommends increased accommodation, quoting, for this purpose, the dictum of Dr. Ferguson, "a lying-in hospital should consist either of a series of cottages, or its spacious wards should contain very few patients."



The mortality of children is very minutely examined, beginning with still-born children; it appears that in the several lying-in institutions of the Austrian Monarchy, they bear to those born alive the following proportions;—Gallicia, 1 in 7; Dalmatia, 1 in 11; Lombardy, 1 in 12; Maritime districts, 1 in 13; Venice, 1 in 16; Upper Austria, 1 in 18; Lower Austria, 1 in 25; Steiermark, 1 in 26; Tyrol, 1 in 27; Bohemia 1 in 28; Moravian Silesia, 1 in 38; Carinthia, 1 in 129. The average is 1 in 24; the proportion of still-born males to females was 124:100. The autumn months were found to be the most fatal, then the winter and spring, and the summer least fatal; but to this general rule Vienna forms an exception, the greatest mortality occurring in summer. In the Laybach institution premature births occurred in the proportion of 1 in 44 mature births; of these premature births, the greatest number occurred in the eighth month. Out of 91 premature births 5 (3 boys and 2 girls) survived, and 86 were born dead. The still-births, were, therefore, to those born alive, as 17 to 1; and the proportion of males to females as 139 to 100. The proportion of twin-births, in one of the lying-in institutions, during a period of fifty-one years, was 71 in 4669, or 1 in 66. In France the proportion is 1 in 95; in Germany, 1 in 80; in England, 1 in 92; in Scotland, 1 in 95; in Ireland, 1 in 62. In these twin-births there was one male to three females; 17 twins in the 100 were born dead, and where one was born alive and the other dead, the number of still-born males was, to that of still-born females, as 1 to 2½. The accoucheur will find some interesting details of the relative frequency of natural and unnatural presentations, to which we refer him, while we proceed to the mortality of the inmates of foundling institutions. In a period of fifty-three years there was born in Krain, on an average, 102 foundlings, of whom, 67 attained the age of ten years, and 33 died before that age. In the first twelve years of life the deaths numbered 34½ per cent.; the mortality of the first year was 23 per cent., which fell short of the mortality in the community at large, which amounted to nearly one-third. The mortality of male children was to that of females as 6 to 5; but it must be borne in mind that the male children exceeded the females by 5 per cent. The mortality of males, therefore, supposing the numbers of the two sexes to be equal, evidently exceeds that of females.

If we divide the fifty-three years from 1789 to 1841 into two parts, it will be found that the mortality of the first half is less than that of the second; the cause of this difference is not stated; it would probably be found to depend on increasing numbers, without a corresponding increase of accommodation.

The mortality of foundlings in Vienna is minutely examined by the aid of figures. During twenty years (from 1821 to 1841) the mortality ranged from 12 to 23 per cent. In the first ten years the average mortality was lower than in the last, and in the three years, 1839-41, it was much greater than in the three years, 1821-23; the mortality of foundlings in Vienna has, therefore, increased. It appears that in different parts of the Austrian Empire there is considerable difference in the mortality of foundlings; thus, while in Lower Austria it is 18 per cent., in Bohemia 15, and in Steiermark 11, it is only 6 per cent. in Upper Austria and Illyria. This is the mortality of those brought up out of the foundling hospitals.



The mortality of foundlings is still further investigated for all parts of the Austrian dominions. This, as it would interest a small portion only of our readers, we shall pass over, and merely notice such facts as are of general interest.

It has been stated that the mortality of foundlings in Vienna has increased of late years; this is the case also in many other parts of the Austrian dominions, while in about an equal number of districts it has fallen off. The cause of this difference is not stated.

In illustration of the danger attending the exposure of the new-born infant to cold, some interesting facts are stated on the authority of Trevisan. Of 100 children born in Italy during the winter, 66 died during the first month, and only 19 survived their first year; while, on the other hand, of 100 children born during the summer, 83,—of 100 born during the spring, 48—and of 100 born during the autumn, 58 survived the first year.

Dr. Melzer's work abounds in statistical details, which want of space prevents us from noticing at greater length; we must, therefore, hasten to the concluding chapter of the work, in which the real value of the foundling-hospital system is tested by argument and fact. We have already touched upon this subject, but its importance demands a further notice.

The foundling system is differently ordered in Roman Catholic and Protestant countries. Though the object in both is the same—the preservation of the life of the innocent and helpless child, and the support of public morality,—they aim at accomplishing the object in opposite ways. The Roman Catholic system establishes lying-in and foundling hospitals, forbids any inquiry into the paternity of the foundlings, and loads the state with a heavy expense. The reception of the foundling is either by the *tours*, in which the concealment of the birth is the leading object—this is the Italian system; or the child is received *à bureau ouvert*—this is the French system. These are the two chief varieties of the Roman Catholic system. The Protestant system, on the other hand, pronounces lying-in and foundling hospitals to be an evil, and rejects them altogether; disapproves concealment, makes the mother answerable for the care of her child, and fines the father in the cost of its support. Where this system prevails illegitimate children are even more numerous than in Roman Catholic countries;\* but children are very rarely abandoned. In a financial point of view, of course, this system has a great advantage over that of Roman Catholic countries.

It is impossible that these two systems can be equally valuable; the one must be better than the other; nevertheless, though different in their principles, they show a certain reciprocity in their operation. In the ultimate results of each system we must seek for the proofs of its utility. We must weigh the profit of each system against the moral and pecuniary results, to determine which has the advantage over its rival; but in actual practice we do not find such a relation between the principle and its results as we might have anticipated. To exhibit the real state of the case is the

\* The proportion of illegitimate to legitimate births in England is 1 in 12; in France, 1 in 13; in Saxony, 1 in 7; in Würtemberg, 1 in 7-8; in Hesse, 1 in 5-6; in Prussia and Sweden, 1 in 13. (Springer's Austrian Statistics.)

object of political economy; but, in the mean time, let us compare the argument for and against each of these rival systems.

The supporters of the Roman Catholic system shall have the first word. According to them foundling hospitals were among the first-fruits of the divine principle of the love of one's neighbour. Religion, not powerful enough to banish vice, and fearing to stain herself with the blood of the innocent, threw over it the veil of compassion, and led back the unfortunate offender to the bosom of society, out of which the hand of her parents had thrust her. Grief, shame, privation, and despair of being able to preserve the life of her offspring, offer a strong temptation to desertion, child murder, and abortion. To force the father to marry the woman he has betrayed, would be a doubtful good, to oblige him to support his child, is a certain difficulty; but in the asylums which are open to the mother, and in which she finds every needful assistance, she is safely and secretly delivered of her burden, and returns in peace to the world; she escapes the shame and reproof which otherwise await her, conceals her first offence, and can resolve against its repetition. The foundling hospital completes what the lying-in institution had begun; it relieves her from a heavy burden, and prevents her from sinking to a lower point of degradation. The child, whom the mother has consigned to the care of the foundling hospital, is not merely supported, but educated. How many mothers, a prey to shame and destitution, and sorely tempted to the commission of crime, has this system saved from destruction! how many children has it rescued from death\* and from the fatal influence of bad example, and preserved to the state as moral and industrious citizens!

If we now turn to the Protestants, they admit the good intentions which led to the establishment of lying-in and foundling hospitals; but they look to their results, and inquire how far the sanguine expectations of their founders have been sealed by experience. These institutions were established to afford the poor destitute mother and child the assistance they so much require; but, however fine in theory, experience says little in their favour. Where such institutions exist, we are constantly meeting with the most melancholy results of such experiments,—men separated from their parents, strangers in their land, and brought up by hireling nurses and attendants. What might once have been a necessity, has long ceased to be such. The public opinion has come to pass a milder sentence on unchastity. By the tenderness which the law has shown to the shame of the mother, shame is at length banished, and with it all fear of consequences. If her poverty be assigned as a reason for our interference, the question arises, is the state bound to undertake the support of all the poor without exception? By doing this, it would step into the place of individual charity; it would give to poverty that which diligence and virtue ought to give it; and, by so doing, would encourage a mean spirit of dependence. The poor ought, it is true, to be supported, but the support offered by the lying-in and foundling hospitals is useless, at the same time that wisdom requires and offers better means. Among the poorer classes there are many married couples who look with envy

\* In Carinthia, which has no foundling hospital, 23-57 per cent. of children died under one year of age; in Krain, which has such an institution, only 20-36 perished.

on the illegitimate child and on her offspring, and on the liberal assistance and support which they have received in the lying-in and foundling hospitals. The child is supported at the public expense, and the mother admitted as wet-nurse into some rich or distinguished family. The conviction that, on the whole, the condition of the unmarried female is more tolerable than her own, that her own children can have no better care than that which the foundlings receive, is not far removed from the wish to turn those public institutions to her own use. This wish soon ripens into act, where the institution gives facilities for it. Hence the multitude of legitimate children who are taken out of the *tours*, and which increases as the fact becomes notorious. The mother has taken her legitimate child to the *tour*, the next step is to offer herself, after a few days, at the institution as wet-nurse; when she has the chance of receiving back her own child, and being paid for its support. To what abuses such a system may lead it is unnecessary to point out; the abuses of the system extend until large numbers of females, in a condition to support their own children, avail themselves of these public institutions. To return to the case of the single woman,—the support of her child, which, in the absence of these institutions, must have devolved upon her, might perhaps have saved her from a repetition of her fault; but the relief from these cares tempt her back to her former mode of life, and encourages her in her vicious course; and yet we hear of the moral uses of the foundling hospital, and of its favorable influence on public morals. It is asserted that lying-in and foundling hospitals tend to preserve the lives of children, and to prevent the exposure and murder of the new-born child. Experience, however, has shown how unfounded this statement is; it has proved that there are more exposures and infanticides in countries which have lying-in and foundling hospitals, than in those which are without them; and the same difference is found to obtain in the same countries before and after the establishment of their hospitals. Thus, in Krain, before the opening of the foundling hospital, the annual exposures amounted to  $3\frac{1}{4}$  per cent.; after the opening of the institution  $4\frac{1}{2}$  per cent. That criminal abortions and infanticides are not diminished by foundling hospitals, is also proved by tables to which we are obliged to refer the reader; neither is the assertion that foundling hospitals preserve the lives of children better founded. Facts are opposed to it; but for these also we must refer to our author.

The result of the careful inquiry into which Dr. Melzer has entered, seems to be unfavorable to the pretensions of what he designates as the Catholic system; and we are not the less inclined to accept the conclusion at which he arrives from its being couched in the language of the review to which we have referred. Foundling hospitals are mischievous, "because they have no influence on those evils which they were instituted to prevent; because the relief they proffer can be obtained only by a deliberate sacrifice of the best feelings of our nature; because they are liable to abuses, which it is almost impossible to prevent; and because, while they entail a great expense upon the country, they preserve the lives of but a very small proportion of their inmates."\*

We know not whether the writer of these judicious conclusions would

\* No. XXVI, April, 1842, p. 293.

be disposed to go to the length to which we have indicated our disposition to advance in the matter of public charities; but we are so far disposed to generalize that we would extend our condemnation to all public establishments, which are not in the nature of *prevention*. If any species of public charity ought to be supported by the government, assuredly hospitals and dispensaries are such charities, and yet these have been largely provided by the benevolence of individuals; and though, not by any means free from some of the abuses of foundling hospitals, there can be little doubt that they are much more free from objection, than if they were public institutions supported by the public purse. The real position in life and wants of their inmates is much better known to the subscribers, and those who have the privilege of recommendation, than it could be to government officers; and thus a part, at least, of the abuses to which all charities are liable is guarded against.

We cannot conclude this notice of Dr. Melzer's admirable work, without again reverting to his censure of our countryman Malthus; and we cannot help regarding the conclusion at which he has arrived, respecting the evils of foundling hospitals, as applicable *mutatis mutandis* to all systems of poor-laws, against which our distinguished countryman so earnestly protested.

A short and simple proverb, **PREVENTION BETTER THAN CURE**, is at the root of all these questions. "Donnez plus de moralité aux classes ouvrières et il y aura beaucoup moins d'enfants trouvés," (the concluding quotation of Dr. Melzer's work,) admits of extension in many directions. For instance, we would say to **ENGLAND**:—Give more intellectual and moral training, and you will have fewer criminals; commit less waste, and you will have fewer paupers; prevent disease and preserve health, and you will need fewer hospitals and workhouses; make the employers of labour responsible for life and limb, and you will have fewer accidents; look to the construction of ships and the education of masters, and you will have fewer shipwrecks; and so on through the length and breadth of your legislation. If this simple proverb will not suffice for your guidance, add this other homely saying, **WASTE NOT, WANT NOT**. If you will not take measures for preventing the refuse of our towns from flowing into the sea, then you must import manure, or corn and meat, or both. There is no other alternative. If you will not consent to that free competition of nation with nation, which, by stimulating all to exertion, makes all men prosperous, then must you take the consequences of your so-called protection,—sloth, ignorance, poverty, revenue laws, preventive service, law-made crimes, and all the waste of time and money which these entail. By your discountenance of foundling hospitals, you show that you are not ignorant of the true principles of political and Christian economy. We earnestly commend the poor laws to your best attention. You are the richest nation in Europe—you have the largest proportion of paupers. Can this be explained on any other supposition than that they are *law-made* paupers? Try the system of *prevention*: it cannot fail more egregiously than the system of *cure* has done. The present system has the condemnation of the past: its rival has all the promises of the future.

## ART. VI.

*Observations and Essays on the Statistics of Insanity ; including an Inquiry into the Causes influencing the Results of Treatment in Establishments for the Insane : to which are added, the Statistics of the Retreat, near York.* By JOHN THURNAM, Licentiate of the Royal College of Physicians of London, Resident Medical Superintendent of the Retreat, near York.—*London, 1845. 8vo. pp. 308.*

In the year 1841, a report, by Dr. Thurnam, including a series of statistical tables, exhibiting the practice of the Retreat, the asylum for the insane of the Society of Friends, for a period of 44 years, from its first establishment in 1796 to 1840, was printed for distribution by the directors of the institution. Of this valuable report a brief notice was inserted in our Journal at the period. The work now before us contains an enlarged and revised edition of the "Statistics of the Retreat," preceded by an elaborate investigation of the present state of the statistics of insanity, deduced from reports and other data furnished by hospitals for the insane in this country, on the Continent, and in America. This portion of the work, we are informed by the author, had its origin at the time he was engaged in drawing up the "Statistics of the Retreat," in the desire to compare the results afforded by the practice of that institution with those obtained in other asylums and hospitals for the insane.

The subjects treated of by Dr. Thurnam are arranged under three heads, the discussion of which is preceded by a concise exposition of the value of statistics as applied to the elucidation of insanity.

I. On the Statistics of Insanity in general. This may be regarded as constituting the body of the work, and is distributed in three chapters, which are devoted—1st, to the methods of deducing and exhibiting the results of treatment in institutions for the insane; 2d, to the various circumstances capable of influencing these results; and 3d, to the statistics of the principal asylums of this and other countries.

II. Essays on the liability to insanity in the two sexes, at different periods of life, and in the Society of Friends.

III. The Statistics of the Retreat, preceded by a sketch of its history and an exposition of its system of management, and of the methods of treatment pursued. To these are added, in an appendix, the history and statistics of the York Lunatic Asylum, and contributions to the statistics of the Society of Friends.

## I. ON THE STATISTICS OF INSANITY IN GENERAL.

1. In order accurately to exhibit the results of the practice of different establishments for the treatment of insanity, it is necessary that the terms employed should be clearly defined; that the registers should be correctly kept; that the mode of calculating the proportion of recoveries and deaths should be uniform; and that the data for calculation should be deduced from observations extending over a sufficient period of time. To the first requisite it is not necessary further to allude: in reference to the calculation of the proportion of recoveries and deaths, various methods have,

however, been adopted in different institutions, and the periods of observation have often been too limited to allow of any precise comparison.

Dr. Thurnam, following the plan adopted by Mr. Farr, in his pamphlet on the "Statistics of English Lunatic Asylums," published about ten years ago, has calculated the proportion of recoveries on the number of cases admitted. To this plan it has, however, been objected, that it exhibits results less favorable than the true proportion, as of the whole of the cases, some still remaining under treatment may admit of cure, and yet have no account taken of them. Mr. Farr, accordingly, in his paper on the "Mortality of Lunatics," in the Statistical Journal for 1841, adopted the method of calculating the proportion of recoveries on the numbers discharged, and in the Reports of the Glasgow Asylum and those of the State Asylum at Worcester, Massachusetts, U. S., the same plan has been followed. This mode, we think with Dr. Thurnam, is still less accurate than the former, the results being much too favourable; as, of the cases received into any institution, a considerable number remain in a chronic state, to swell, at a later period, its mortality. Mr. Farr has, indeed, met this objection by stating that, "if the mortality remained the same, the probability is that the patients to be discharged would, *cæteris paribus*, be discharged cured, relieved, and died, in the same proportion as those already discharged." Our author, however, shows that the results of observations do not confirm Mr. Farr's theoretic statement—of the patients remaining in an institution at any period, a much less number being in a curable state than its average proportion of cures. Thus, at the Retreat, at Midsummer 1840, the proportion of cases regarded as curable was only 15 per cent., while the average proportion of recoveries during the 44 years that it had been in operation, amounted to nearly 47 per cent.

Under either of these methods the results are not strictly accurate, the true proportions being intermediate between those thus obtained. The former plan, however, probably possesses the greatest exactitude, and this view is confirmed by the fact, that while, according to the latter method, the proportion of recoveries at any institution is in its early periods high, and undergoes a rapid diminution, that furnished by the former gradually increases at each quinquennial period for a long series of years.

The most accurate method of estimating the average mortality of hospitals for the insane is, doubtless, as shown by Mr. Farr, to calculate the proportion of deaths on the average annual population or mean number of patients resident in the institution. In the reports of some asylums, however, the mortality has been estimated by the proportion of deaths to the patients admitted, and in others, to those discharged, while in some, amongst the most recent of which may be mentioned that of M. Parchappe, on the asylum at Rouen, the error has been committed of calculating the mortality on the total number resident during each year. Our author shows that our judgment of the character of these establishments, as founded on the rate of their mortality, will be entirely reversed according as the proportion of deaths is calculated on one or other of these plans, and, we need scarcely observe, that, without the employment of uniform methods of estimating the mortality, no satisfactory comparisons can be instituted.



In estimating the relative mortality of acute diseases, which have generally a definite course and duration, the period of residence is of little importance, but in an affection so chronic as insanity, and of which the duration is so varied, no comparison can be made between the average mortality of different institutions, if time be not taken into the account. Dr. Thurnam, therefore, justly expresses regret that in so few of the reports of asylums has the average population, or mean number of patients resident in the institution been hitherto given. We would farther observe, that the same omission exists in the reports of many of our general hospitals, in which, though of little importance as applied to estimates of the relative mortality of similar acute diseases, the average population is a necessary item in all comparisons of general results, and calculations of an economical nature.

To insure accuracy in the comparison of the mortality of lunatic asylums, it is also necessary that the estimate should be founded, not merely on the total number of patients resident, but on the numbers at the several periods of life respectively:—since it is evident that the varying mortality at different ages may materially affect the general results in institutions receiving different proportions of patients at the several periods of life.

All who are familiar with statistical inquiries are aware of the fallacious results of calculations deduced from limited series of facts and short periods of observation; the period, however, over which it is necessary that the observations should extend, in order to insure accuracy in stating numerically the results of the practice of lunatic asylums, appears, from our author's researches, to be greater than would *a priori* have been anticipated. At the Retreat, he informs us, that the period of twenty years elapsed from the establishment of the institution, before the proportion of cures arrived at the point which it has since maintained during the entire period of its operation. The rate of mortality during the first five years amounted to 5·71 per cent., and gradually declined from that period, till, at the end of 25 years, it averaged only 3·71 per cent.;—it then again underwent an increase; and, after the lapse of 35 years, attained the rate of 4·71 per cent., which it has since preserved. From an investigation of the statistics of other institutions, Dr. Thurnam has also found that the proportion of recoveries has, in nearly every instance, gone on increasing for a period often of 30 to 40 years from their commencement, owing probably, to the large number of chronic cases admitted at the first opening of any asylum, and to the long period occasionally required for the cure of the disease. On the other hand, it appears that the mortality is generally more favorable in the early periods of the operation of an asylum, and gradually increases for 20 or 30 years, when, as more recent cases are admitted, and the older ones die off, it often presents a rate of 50 or 100 per cent. higher than that of the first few years. Dr. Thurnam, therefore, suggests that, in the published reports of asylums, a summary should be appended of the general results for a series of years, dating from the establishment of the institution, or, if any great change have taken place in its mode of management, from the period of such change.

2. In treating of the circumstances in the character of the cases admitted and the method of treatment pursued, as influencing the results

obtained in hospitals for the insane, Dr. Thurnam adopts with some modification the arrangement of Gavarret, into—1st, individual conditions; 2d, hygienic conditions anterior to the invasion of the disease; 3d, the form of disease, its duration, &c.; 4th, the hygienic condition during treatment; and 5th, the treatment itself.

Of the individual conditions and the hygienic conditions anterior to the invasion of the disease, the chief circumstances which modify the results of treatment are the sex and age, rank and previous habits of the patients, and the predisposing and exciting causes of the disease.

*Sex.* Writers have differed as to the relative frequency of recoveries from insanity in the two sexes. The generally received opinion has however been, that in females the probability of recovery is not only greater, but that the tendency to fatality is less. With these views Dr. Thurnam's conclusions coincide. He states that, with two exceptions, in all the institutions the statistics of which he has examined, and of which the periods of operation have been sufficiently extended, the proportion of recoveries in females has exceeded, often to a considerable extent, that in men. The excess in favour of females he finds to vary in different institutions, from 4, 5, and 7 per cent. in the asylums of Glasgow, Belfast, and Lancaster; to 20, 22, 23, and 28 per cent. at the Retreat, the asylum of Schleswig in Holstein, Charenton, and the York Lunatic Asylum. At the Senavra hospital, in Milan, the proportion of recoveries in females has only exceeded that in males by 2 per cent., and at Hanwell and the Bloomingdale Asylum, in the State of New York, U. S., the excess of recoveries is on the side of males, and amounts to 5 and 28 per cent. respectively. The results afforded by the experience of the Senavra may probably, it is suggested, be referrible to the cases treated in that institution being most generally complicated with the peculiar endemic of Lombardy—the pellagra. The great excess of recoveries in males at the Bloomingdale asylum is ascribed to persons labouring under delirium tremens being treated in that institution.

The difference between the mortality of insanity in the two sexes is greater than in the relative probability of recovery. Instead of the excess of 5 or 6 per cent., which obtains in the general population, our author's investigations show that, with the solitary exception of that of Belfast, the average mortality among males in the British asylums is uniformly very much greater than among females. At the York Lunatic Asylum and St. Luke's there has been an excess on the side of the men of 93 and 98 per cent. respectively; and, in the institutions where the difference is least, as in the asylums of Schleswig and Worcester, U. S., the mortality in males is 9 and 13 per cent. greater than in females. This difference applies to institutions admitting all ranks of patients in a nearly equal degree; being in the metropolitan licensed asylums for paupers 63 per cent.; and in those for private patients 57 per cent. At the Senavra, the mortality in females, during a period of 25 years, exceeded that in males by 13 per cent. This statement is sufficient to show the necessity for giving the average mortality of any institution in each sex separately, as it is evident that, by the respective proportion of males and females, the general results will be materially modified.

As regards the liability to relapse and recurrences of the disorder, the

influence of sex would appear to be partially reversed, females being somewhat more frequently the subjects of second attacks than men. The results obtained by our author on this point, as also on the frequency of second attacks of insanity, being highly interesting, and opposed to the views generally entertained, we shall lay before our readers. The method of ascertaining the frequency of the recurrence of insanity which has ordinarily been adopted, has been to calculate the proportion which the cases readmitted into any institution bear to the whole of those treated or discharged cured. Esquirol was thus led to estimate the relapses at 10, Pinel and Desperes at 17, and Farr at 30 per cent. of the recoveries. It is, however, obvious that a calculation of this kind is open to the fallacies that cases may be readmitted which were not recovered at the time of discharge, and that cases discharged cured may relapse, but not be again sent to the institution. At the Retreat the readmission of cases discharged recovered have amounted to 31·5 per cent. This must, however, our author remarks, be regarded as less than the true proportion of relapses; as, of those discharged, some may yet, after several years have elapsed, experience second attacks. In support of which, he states that five men, labouring under second attacks, had been admitted since the date of the report, and after 4, 5, 8, 14, and 19 years had respectively intervened since the period of their recovery from the first attack. A further correction must be made for those who, having been discharged from the institution cured, were known to have experienced subsequent attacks, but had not been readmitted. Dr. Thurnam, therefore, infers that, by this method of investigation, "the liability to relapse or recurrence of insanity, after recovery from a first attack, all things considered, can scarcely be estimated as at all less than 50 per cent., or as 1 in every two cases discharged recovered."

As stated by Mr. Farr, it is, however, only by following through life a large series of cases where recovery has occurred, that the frequency of second attacks of insanity can be satisfactorily ascertained. The Retreat affords peculiar facilities for such an investigation; as "not only is almost every case of second attack readmitted, but an opportunity may generally be found for ascertaining the subsequent history of all cases that have at any time been under care." Dr. Thurnam has thus been enabled to obtain full information as to the whole of the 244 persons who have at any time been patients in the institution, and who have subsequently died. Of these it appears that only 131, or 53·6 per cent. recovered from the first attack; and of these 45, or nearly one third (18·4 per cent. of the whole) continued permanently free from mental disorder, while 86 had one or more subsequent attacks, and only 20 were sane at the time of death. Of the 244 cases, therefore, only 65 (45+20), or 26·6 per cent. died sane.

"In round numbers, then, of 10 persons attacked by insanity, 5 recover and 5 die, sooner or later, during the attack. Of the 5 who recover, not more than 2 remain well during the rest of their lives; the other 3 sustain subsequent attacks, during which at least 2 of them die. But although the picture is thus an unfavorable one, it is very far from justifying the popular prejudice, that insanity is virtually an incurable disease; and the view which it presents is much modified by the long periods which often occur between the attacks, during which intervals of mental health (in many cases of from 10 to 20 years' duration) the individual has lived in all the enjoyments of social life."

Our author's observations show that the proportion of recurrences in women is somewhat greater than in men; being, in the former, 67·1, in the latter, 65·7 per cent.

*Age.* The greater curability of insanity in early life has been long established. Esquirol regarded the disease as most amenable to treatment in persons between 20 and 30 years of age. This result differs somewhat from the conclusions of our author; who, from the tables of the Retreat, York Asylum, and Bethlem Hospital, infers that the recoveries are most numerous at the earliest ages at which the disease prevails, and gradually decrease in number with the progress of life. The proportion of the recoveries under 20 years of age being 55·55 per cent. of the whole of those admitted between these ages; 51·77 per cent. between 20 and thirty years; 46·16 between 40 and 60; and 27·8 between 60 and 80. The decrease in the proportion of recoveries from 40 to 60 and 80 years of age, follows therefore, a rapidly increasing ratio, being 7, 11, and 40 per cent. respectively. The only exception to the increasing proportion of recoveries with the progress of age, is stated to exist in females; in whom the proportion of recoveries at 10 and 20, and 40 and 50, is less than in the subsequent periods of 20 and 30, and 50 and 60 years of age; exceptions of which the peculiarities of the sex afford a probable explanation. The mortality of the insane at different periods of life has been made the subject of an elaborate investigation by our author; and the table which he has compiled from the experience of the Retreat and York Lunatic Asylum, constitutes, we believe, the only approach to a life-table for the insane which has yet been published. The rate of mortality, as observed at the Retreat, has been adopted by the Medical Invalid Life Assurance Society, for the calculation of the rates of insurance on the lives of insane persons under favorable circumstances. From the table showing the estimated mean annual mortality at these institutions, as compared with that in the population at large, and in the Society of Friends, it appears that, supposing 100 persons were living at each decennial period of life, from 20 to 90 years of age, instead of the mean mortality at all ages being, as estimated by Edmonds, for the country at large, 6·03 per cent., or as deduced from the data given in the Appendix for the Society of Friends alone, 4·8 per cent.—the mortality of the York Lunatic Asylum is found to be 10·88, and that of the Retreat 7·32 per cent. While, however, there thus exists a great excess in the mortality of insane persons at all ages over that of the population at large, this excess is by no means equal at each period of life, being much greater at the early ages than in more advanced life. Dr. Thurnam remarks that—

“The mortality at the Retreat, especially among males, from 20 to 50 years of age, was double, treble, and even quadruple, what it is among the sane at the same ages; whilst at 70 years of age and upwards, it is only slightly, if at all, higher than in the population at large. And thus it appears not improbable that if the insane survive the 60th year, they are nearly as likely to attain advanced age as the sane. At this period of life, the disorder has in general passed into a fatuity, or, at least, into a chronic and often lively form of insanity; and the bodily functions are often remarkably active, if not also vigorous. The prevalent error of the insane being long-lived is, perhaps, to a certain extent, to be explained by this interesting and unexpected result.”

The influence of insanity on the duration of life is further illustrated by

tables giving the age of the patients, the duration of the disorder, and the period of death, in the whole of the cases which have proved fatal at the Retreat since its establishment. These cases are 139 in number, and the average age at which such of the patients as were members of the Society of Friends were attacked with the disorder was 39·19 years; of the others, 38·6 years; and the mean age at the period of death was 56·5 and 47·7 respectively. In reference to these results, our author remarks that, as at 38 and 39 years of age, the expectation of life is regarded as not less than 28 years, the mean ages attained should have been 67 and 66, instead of 56 and 47 respectively. "In those connected with the Society, therefore, less than two thirds, and in others not more than a third, the expectation of life at the time of attack was, on an average, realized." Notwithstanding the occasional occurrence of instances of longevity amongst insane persons labouring under the chronic form of the disease, the tendency of insanity to shorten life is thus shown to be very decided, owing to the high rate of mortality in the earlier stages and more acute forms of the disease. It is further stated, that in those who recovered from first attacks, and died subsequent to discharge, the ages at the time of death and at the period of attack corresponded with those observed in the institution; evincing that after recovery from insanity, as after attacks of most important diseases, the average duration of life is materially shortened.

3. Few subjects have recently attracted more deserved notice than the influence of distress and destitution, and their attendant evils, in increasing the prevalence of disease, and lessening the average duration of life; and it was to be anticipated that the experience of asylums for the pauper population, more especially of the large commercial and manufacturing towns, would present results less favorable, both as regards the recoveries and mortality, than institutions wholly or in part devoted to the reception of persons of the more opulent classes. This inference is confirmed by our author's researches; and he further thinks it probable "that the proportional difference in the rate of mortality in different classes of society is much increased under the operation of insanity, and that it is greater than under that of disease in general." In support of this opinion he contrasts the proportion of recoveries and mortality at the Retreat with their rate at the asylums of Wakefield and Lancaster, for the paupers of the West Riding of Yorkshire and Lancashire. In the former institution the proportion of recoveries, in persons connected with the Society of Friends, has been about 50 per cent., and the mean mortality 4·7 per cent.; whilst at Wakefield and Lancaster the recoveries have only been 43·6 and 41·4 per cent., and the mean mortality has attained the high averages of 15·7 and 16·5 per cent. respectively. A similar, though less marked, difference is observed in the metropolitan licensed private asylums; the mortality having been 20·68 per cent. in such of the houses as receive paupers, whilst in those for private patients it was only 10·94 per cent. In reference to the remarkable contrast thus exhibited, Dr. Thurnam remarks that it can scarcely be entirely attributed to the previous circumstances of the patients, but must, in part at least, be referred to the pauper patients not enjoying an equal degree of domestic comfort and as nutritious a diet as the rich.

Passing over the fourth and fifth classes of circumstances modifying



the results of treatment in asylums for the insane, we shall give our author's conclusions as to the influence exerted on the statistics of such institutions, by the duration of the disorder at the period of admission, and by the duration of residence or treatment.

The duration of the attack at the time of admission into hospitals for the insane exerts a double influence on the statistical results;—the probability of recovery being much greater in such cases as are brought under care at an early period, while the average mortality is less in the chronic stages of the disease.

“At the Retreat, the probability of recovery in cases brought under care within three months of the first attack, has been found to be as 4 to 1, and excluding cases complicated with serious bodily disorders, as 9 to 1; whilst in cases not admitted till more than twelve months after the attack, the probability of recovery is less than as 1 to 4. The proportion of recoveries now mentioned as having occurred at the Retreat, in uncomplicated cases of less than three months' duration, or that of 9 out of every 10 cases brought under care, is the same as that which, in cases equally recent, the late Dr. Willis and Dr. Burrows both stated as having occurred in their private practice. Though, both when first made, as well as since, Dr. Willis's statement has often been called in question, it may still be regarded as not in any degree improbable, and may, indeed, often apply to a small number of cases. But unless a selection be made of uncomplicated cases, it may be questioned whether so large a proportion will be found to recover when the experience is not a limited one. Much reliance, indeed, should never be placed upon the results derived from a limited sphere of observation, whether in public or private practice, as proof of which it may be here stated, that out of twenty cases admitted at the Retreat within three months of the first attack, during 10 years (1798-1808), 19 were discharged recovered.”

As before stated, the short duration of the disorder on admission exerts an unfavorable influence on the rate of mortality. During 48 years, the annual mortality at the Retreat averaged 7·3 per cent. in cases of first attack admitted within three months of the commencement of the disorder, and only 4·7 per cent. in cases whether of first attack or otherwise, and of more than twelve months' duration. These differences in the results of treatment in cases of insanity, according to the period at which they are admitted into hospital, show that to enable any satisfactory comparisons to be made between the results of different institutions, it is necessary that similar modes of classification of the cases as to duration at the time of admission should be adopted. The cases at the Retreat are arranged into four classes—1st, cases of the first attack, if not more than three months' duration at the time of admission; 2dly, cases of the first attack of more than three, but not more than twelve months' duration; 3dly, cases not of the first attack, and of not more than twelve months' duration; and, 4thly, cases whether of the first attack or not, and of more than twelve months' duration. This plan, or one almost identical with it, our author informs us, has already been introduced into several of the county asylums, and he suggests that a similar classification should be generally followed.

The duration of the treatment, or residence, may be modified by a variety of causes; as by the results of treatment being more or less favorable, and so having a smaller, or larger, proportion of cases in an incurable condition; by economical motives occasioning an early removal of the



patients from the institution ; or by special rules limiting the duration of their residence. At the York Lunatic Asylum, where the second cause operates, the patients, on an average, only remain  $2\frac{1}{2}$  years (2·52) ; at Bethlem and St. Luke's, where they are only permitted to remain twelve months, the duration of residence of curable cases averages only half a year (·57), and two-thirds (·68) of a year respectively. While at the Retreat, where, from the peculiar constitution of the Society of Friends, neither economical nor other motives operate to hasten removal, the mean period of residence has been nearly five years (4·8). In the latter institution one-third (34 per cent.) of the entire recoveries have occurred after the first year of residence ; and nearly one-sixth (or 15·3 per cent.) after the second year. Though, therefore, at the Retreat, in some of the cases the period of probation may have been unnecessarily prolonged, it is evident that the short duration of residence at the former institutions must operate unfavourably on their proportion of recoveries, and, by occasioning the premature discharge of cases, must lessen the probability of ultimate recovery. The long duration of residence at the Retreat not only proves favorable to its proportion of cures, but lessens the average mortality.

The influence exerted on the statistics of hospitals for the insane by the several particulars of treatment, is treated of under the two heads of the general hygienic conditions during treatment, and the treatment itself. Of the former class of modifying causes are—1st, the healthiness of the locality in which the asylum is situated, as influenced by climate, elevation, soil, drainage ; 2d, general adaptation and appropriate construction of the building ; 3d, means of exercise, occupation, and amusement ; 4th, internal economy and government, number of attendants ; 5th, ventilation, lighting, warmth, and cleanliness of the apartments, as modified by the numbers treated in the institution ; 6th, clothing and personal cleanliness, baths, &c. ; 7th, diet. Of these various circumstances our author takes a rapid survey, and weighs the probable effect of their good or bad arrangement on the proportion of recoveries and mean annual mortality of institutions for the insane. In reference to the internal economy and government of asylums, he agrees in the opinion, which we believe to be general at the present day, that the most efficient system of management is that in which the entire direction is vested in a resident medical officer, assisted by the advice of visiting physicians. Such a system, to be thoroughly carried out, presupposes the limitation of asylums to the accommodation of a moderate number of patients. Dr. Thurnam quotes on this subject the opinion of Jacobi, that the maximum number of patients to be admitted into one asylum ought never to exceed 200 ; probably, however, in pauper asylums, accommodation for 300 or 400 patients is not too great. Our author also expresses his approbation of the suggestion to erect *asylums* for the detention and care of the incurable and harmless patients, apart from the *hospitals* for the treatment of the more acute and severe cases of insanity ; and suggests that such institutions should be in the immediate neighbourhood of the latter, and under the charge of their medical officers. We believe that this change, more perhaps than any other, would conduce to further the efficiency of our larger public asylums ; many of which are at present so crowded with patients in a state of fatuity and in the chronic forms of the disease, as

to limit the accommodation for the more acute and urgent cases, and interfere materially with their successful treatment.

The number of attendants on the patients in institutions for the insane is of great importance, as affecting not only the comfort of those under treatment, but influencing a variety of circumstances by which the general efficiency of an establishment, and its consequent proportion of recoveries, and amount of bodily disease and mortality, are materially modified. The proportion which the attendants bear to the patients in different asylums seems to vary considerably. In 13 county asylums, in the year 1835, the average proportion was 1 attendant to 17 patients; and, in some of these establishments the number was much less. In the Wakefield Asylum there was only 1 attendant to 20 patients, and at Hanwell 1 to 26; but the proportion has since been increased in the latter institution to 1 to 18. In 8 of the 10 Irish district asylums there is 1 attendant and assistant to 9 patients; or, taking responsible attendants only, 1 to 13 patients. At the York Lunatic Asylum and the Lincoln Asylum, which receive both pauper patients and those of the upper ranks of society, the proportion of attendants is 1 to 11 or 12, and 1 to 9 patients. In the asylums of Montrose, Aberdeen, Glasgow, Dundee, and Perth, the proportion appears to average 1 to 10. At Siegburg, near Bonn, it is 1 to 7 or 8; and at the Retreat, 1 to 6 or 7 patients. Our author concludes the enumeration of the proportion which the attendants bear to the number of patients under treatment at different institutions by remarking that—

“In pauper *hospitals* for the insane, in which the patients are frequently able to render considerable assistance to the attendants, the proportion of the latter will not, perhaps, on the average, require to be more than 1 to 12 or 15; and in *asylums* for this class the proportion need not be half even this. In other institutions, the highest class of patients, on account of their previous habits, will often require a separate and distinct attendant to each case. This should, perhaps, always be the case in the early stages and more severe forms of the disorder; but in the more advanced stages, and more chronic and harmless forms of insanity, a limited number of this class, not exceeding five or six, may be properly enough associated together under the care of two or three attendants.”

The diet of the patients in the various county asylums for paupers differs considerably, both in the quality and quantity of the food; and, in some, the unfavorable results of treatment, as evinced by the low proportion of recoveries and high rate of mortality, is probably in part ascribable to a deficiency in the amount of nutritious food allowed. With a view to illustrate this subject, our author has given a table of the diet in seven of the principal county asylums, as it existed in 1835. These institutions, he shows, may be divided into two groups; in one of which, including the asylums of Nottingham, Stafford, and Gloucester, the diet was considerably above the average of similar institutions; while, in the other group, embracing the four asylums of Wakefield, Lancaster, Suffolk, and Middlesex, the allowance of food was much below the average. In the first group, the solid food, consisting of meat, cheese, puddings, bread, and other articles, exclusive of vegetables, amounted to 225 oz. per week; while in the second it was only 150½ oz. The amount of animal food alone was, in the first group, 46 oz., and in the second only 19½ oz. per week; and,

though the allowance of soup, porridge, milk, &c., was in the latter 15½ pints per week, while in the former it was only 10 pints, this excess could by no means compensate for the deficiency of more solid nutriment. In the first group, also, the quantity of beer allowed amounted to 2 pints daily; in the latter to not more than half a pint. The difference in the results as to recoveries and deaths between these two groups of institutions is very striking; in the three asylums with the more liberal diet, the recoveries having been 43·7 per cent., and the mean mortality 9·35 per cent.; while in the second group, with the more scanty allowance of nutritious food, the recoveries have averaged only 36·75 per cent., and the mortality has been as high as 14·54 per cent. It would be premature, on the limited information before us of the circumstances of these several institutions, to draw any positive inference as to the dependence of their comparatively successful treatment on the respective diets allowed; it seems, however, not improbable that a portion, at least, of the discrepancy may be referred to this cause. The observations call for a fuller investigation into the effect of diet on the treatment in pauper asylums; and it would be well that in these institutions, as in workhouses and prisons, fixed dietaries, apportioned on a liberal scale to the several classes of patients under treatment, and convertible so as to present varieties in the kind of food allowed, should be arranged by the commissioners, and required to be generally observed.

The remaining portion of this section of Dr. Thurnam's work is devoted to the consideration of the several objects to be held in view in the treatment of insanity, and the influence of the treatment pursued on the statistical results of hospitals for the insane. The section concludes with an illustration drawn from the history of the York Lunatic Asylum, prior and subsequent to its reformation in 1814. It is impossible to read this and other accounts of the state of our asylums a few years ago, without feelings of great gratification with the changes which have taken place. In reference to this institution, it is satisfactory to find that the abandonment of a system of management fraught with cruelty and abuse, and the adoption of plans of treatment more accordant with humane feelings, have been attended with a very decided improvement in the health of the inmates, as evinced by the decrease in the average mortality. Thus our author shows that, during the 37 years from the establishment of the institution to the period at which the change in the management took place, the average mortality was 11 per cent.; and during the six years which immediately preceded that change, and when the abuses in the asylum had attained their greatest height, not less than 14·8 per cent. Since this period, on the contrary, in the 26 years which elapsed to 1844, the mortality has been only 7·24 per cent., or 40 per cent. less than its rate during the entire former period, and fully 100 per cent. less than during the six years preceding the change. The mode in which the registers of the patients were kept before the reform precludes the possibility of any investigation of the relative proportion of recoveries; but there can be no doubt that the improvement in this respect has not been less satisfactory.

The third and concluding chapter of this portion of Dr. Thurnam's work contains the statistics of the principal hospitals for the insane in this country, on the Continent, and in America. An elaborate table pre-

sents us with the data furnished by the entire experience of 59 public asylums, including the observation of more than 125,700 cases of insanity; together with calculations of the proportion of recoveries in the numbers admitted, and the mean annual mortality estimated on the average population. We quote the following tabular statement of the results deduced from this laborious investigation:

	Proportion of recoveries.	Mean mortality.
9 English county asylums, receiving paupers	36.95	13.88
6 English county asylums, receiving paupers and private patients	46.87	10.46
8 English asylums, supported by charitable contributions, and receiving pauper and private patients	40.94	8.93
7 Scotch asylums, receiving pauper and private patients	42.37	7.52
10 Irish district asylums for paupers	48.33	8.7
5 asylums in the United States for paupers and others	46.82	9.56

From this table it is inferred that, in asylums which have been established for a considerable length of time, the proportion of the recoveries to the whole of the cases admitted should be considered low, if amounting to much less than 40 per cent., and high if exceeding 45 per cent. On the other hand, the mortality in an asylum for the more opulent classes of society, as well as for paupers, calculated on the average numbers resident, may be considered highly favorable if under 7 per cent., and decidedly unfavorable if exceeding 9 or 10 per cent. In asylums wholly appropriated to paupers, a mortality of much less than 10 per cent. must be regarded as very low, and one exceeding 12 or 13 per cent. as decidedly high. While, however, the sources of fallacy in estimating the relative efficiency of different institutions by the average mortality in the resident population are fewer than those which attend conclusions drawn from the proportion of recoveries, great caution must be exercised in adopting either of these standards of comparison.

## II. ESSAYS ON THE LIABILITY TO INSANITY.

1. *Relative liability to insanity in the two sexes.* It has been the generally received opinion since the time of Aretæus and Cælius Aurelianus, that females are more subject to insanity than males, and this Esquirol, from an extensive statistical investigation, believed to be correct, inferring that 38 females became insane to 37 males; a view which has since been maintained by most writers on insanity. In estimating, however, the relative liability of the two sexes to this disease, Esquirol took no account of the relative proportion of the sexes living at the periods of life when insanity is most frequent; an omission which, with the known preponderance of females in the general population above 15 and 20 years of age, goes far to explain the results he obtained.

In this country Dr. Thurnam states that the only institutions which maintain a much larger proportion of females than males are those in the neighbourhood of the metropolis, Bethlem, St. Luke's, and Hanwell; and in the latter there has been of late years a gradual diminution of the excess in females. Their greater proportion in these asylums may perhaps be explained by the preponderance of females being still greater in the population of London than in that of the country at large, amounting in the former to 118 females to 100 males from 20 to 50 years of age, while in the latter there are only 108 females to 100 males. The greater propor-

tion of insane females in these institutions may also be in part referrible to peculiar circumstances connected with the metropolitan population. The excess seems only to exist in the inferior ranks of society, as in the licensed metropolitan asylums for private patients, males are in the largest proportion.

Esquirol also, in his statistical investigation on this subject, estimated the relative proportion of the two sexes in the *existing*, not in the *occurring*, cases of insanity; a method of inquiry which, when the relative mortality of the disease in the two sexes is considered, is obviously fallacious. Dr. Thurnam has shown that, while in the general population of this country the mortality of adult males exceeds that of females only by 7 or 8 per cent., the excess of mortality in insane persons of the male sex is not less than 50 per cent., and consequently that the gradual accumulation of incurable females in our asylums is, in this proportion, greater than that of males. It is obvious, therefore, that the question of the relative liability of the two sexes to mental disease must be decided by the relative proportion of occurring cases in each; or, as the only available means of ascertaining this, by the relative numbers admitted during a term of years into our hospitals and asylums for the insane. With this view, Dr. Thurnam has compiled a table of 48,103 cases admitted into 82 different asylums in this country, America, and on the Continent, from which it appears that in 24 of these institutions the admissions of males were the most numerous, the excess in the whole number amounting to 13·7 per cent., and in some of the English institutions being not less than 25, 30, and 40 per cent. The only English county asylums where this excess does not exist are those of Dorset and Norfolk. From a still larger number of cases, including persons of all ranks of society, he elicits similar results; and it would appear from the greater excess of males admitted into the asylums of the metropolitan district for private patients, that in the upper ranks of society men are more liable to insanity than women, in a still higher proportion than obtains in the pauper population. It must, however, be borne in mind, that among the wealthier classes a larger portion of insane females may possibly be detained under the charge of their friends.

The experience of the Society of Friends, as ascertained by the admissions into the Retreat, would appear at first sight opposed to these conclusions; the excess of females admitted since its commencement amounting to 18 per cent. It is, however, probable, that amongst adults the excess of females in this community exceeds that of males in a larger proportion than in the general population, being, our author infers from data with which he presents us in an appendix, not less than 30 to 35 per cent.

In most respects, therefore, as regards insanity, females are more favorably circumstanced than males. They are less liable to the disease; and when seized, the prospect of their recovery is greater, and the tendency to death less; they are, however, apparently somewhat more exposed to relapses and recurrences of the disorder.

2. *On the relative liability to insanity at different ages.* Esquirol, proceeding on the same erroneous method of statistical analysis, as in estimating the comparative frequency of insanity in the sexes, inferred that

the tendency to the disease underwent a constant increase with the progress of life. Quetelet, on the contrary, concludes that insanity is most frequent between 40 and 50 years of age, and Burrows between 30 and 40.

It is evident that to estimate correctly the relative liability to insanity at different ages, a comparison must be instituted as to the number of *occurring* cases at the different periods of life, and the relations which these bear to the numbers living at the same ages in the general population. The materials which at present exist for instituting a comparison of this kind appear, from our author, to be scanty. The only published data on the subject, extending over a considerable period of time, being prior to those given in the '*Statistics of the Retreat*,' a list of 566 cases, by Dr. Jessen, of Schleswig, and the report of Dr. Woodward, of Massachusetts, U. S. Dr. Thurnam, from these and other data, has compiled a table, showing the age at attack in 5122 cases; and a second, from the reports of the Hanwell Asylum for 5 years, 1840-44, from which he infers that the greatest liability to insanity is between the 20th, and 50th and 60th years. The attacks being most common between the 30th and 40th years, and undergoing after this period of life a gradual decrease; a conclusion opposed to that of Esquirol, but harmonizing with the *a priori* inference, that persons at the period of life when the intellectual and physical energies are at their height, and exposure to the influence of disturbing causes the greatest, would be most subject to the disease.

In some of the American asylums, statistical observations show that there is a greater prevalence of the disease between 20 and 30 years of age than at any other period; a fact referrible probably to the peculiar circumstances of the population of that country. The same, from the observations made at the Retreat, would appear to be the case in the Society of Friends. The proportion of cases admitted between 20 and 30 years of age being nearly one-third of the whole, and the admissions after this period of life declining.

3. *Liability to insanity in the Society of Friends.* Our author in entering upon the discussion of this subject, refers to the difficulty of estimating the liability to and prevalence of insanity, either in the general population, or in a particular community like the Society of Friends. In order to determine the liability to disorder in any community, he shows that as in the former instances the calculation must be founded on the number of occurring cases, as a variety of causes affecting the curability of the disease, the tendency to relapses, and the proportion of deaths, will materially affect the number of insane persons living in a given population. From the observations made at the Retreat, it appears that for the 20 years, 1820-40, the number of admissions averaged 12.35 annually; of which 5.45 were males and 6.9 females. Returns recently obtained show also that the mean population of the Society during this period was about 17,900, of whom 8137 were males and 9763 females. From these data it appears that 6.9 cases of insanity are admitted into the Retreat annually out of a population of 10,000 persons. It is further assumed that 2 other cases of insanity occur annually among members of the Society which are not sent to the Retreat, giving a total of 8.9 cases; or, making a deduction of cases readmitted into the Retreat which had not been discharged *cured*, of 8.1 cases out of 10,000 persons, or 1 in



1234. Of the 274 cases received into the Retreat during the twenty years, 163, or nearly two thirds of the whole, viz. 79 males and 84 females, were admitted with first attacks; deducting therefore the cases readmitted, Dr. Thurnam concludes that the actual cases which occur annually in the Society average 5.5 per 10,000 persons, or 1 in 1790.

The cases actually admitted into the Retreat and similarly corrected, amount to 4.55 per 10,000, or 1 in 2196. The males being 4.55, the females 4.3; showing an excess of males, as compared with the relative number of the two sexes in the Society at large, of 13 per cent.

At the asylum of the Society of Friends, at Bloomingdale, near Dublin, the cases admitted are in the proportion of 6.28 annually to a population of 10,000, or 1 in 1590 persons. At the Friends' Asylum, Frankford, U. S., they amount to 7.9 per 10,000, or 1 in 1265 of the population. In these instances it is not known what correction should be made for the cases not sent to the institutions; but the proportion admitted into the Frankford Asylum may perhaps justify the "surmise that mental disorders are somewhat more frequent in the Society of Friends in the United States than in the same community in Great Britain and Ireland."

For these statistics of the prevalence of insanity in the Society of Friends our author claims only an approximative value; they are, however, we may conclude, from the facilities for obtaining information on this subject afforded by the limited population of the Society, more trustworthy than any calculations we possess as to the frequency of the cases in the general population of this country. Till more accurate information in reference to the latter be obtained, the question whether, as supposed by Dr. Burrows, the members of this community be especially subject to attacks of insanity, or whether, as asserted by Dr. Haslam, they enjoy a peculiar immunity from mental diseases, must remain *sub judice*. The infrequency, however, of habits of intemperance among them, which in the population at large forms so fruitful a source of insanity, renders it probable that the prevalence of the disease in the Society of Friends will prove to be below the average in the general population.

The number of existing cases in the Society, as ascertained by the admissions into the Retreat, and corrected for cases not sent to that institution, appears to be in the proportion of 525 to 10,000 of the population, or 1 in 190 persons. This is undoubtedly a high proportion, but it is probably referrible to the favourable hygienic conditions in which the insane of the Society of Friends are placed; and, as suggested by Dr. Thurnam, to slighter cases being placed and continued under treatment than is generally the practice.

### III. HISTORICAL SKETCH AND STATISTICS OF THE RETREAT.

Of the concluding division of Dr. Thurnam's work, space will only allow of our offering a very concise notice. The suggestion for the establishment of an institution by the Society of Friends, to be specially devoted to the reception and treatment of the insane members of that community, emanated from the late William Tuke, who, together with his son Henry Tuke and Lindley Murray, were the most active agents in its formation. To all who are familiar with the state of our asylums for the insane prior to the establishment of the Retreat, and with the influence

which the mode of management adopted in this institution has had in the diffusion of more humane methods of treatment, the names of the Tukes, and especially of Samuel Tuke, the present treasurer and author of the 'Description of the Retreat,' published in 1813, will ever be held in esteem.

"By a singular and interesting coincidence," remarks our author, "it was in the spring of 1792, the very year in which the celebrated Pinel commenced the amelioration of the treatment of the insane in France, by the truly courageous act of unchaining nearly fifty supposed incurable and dangerous lunatics at Bicêtre, that the establishment of the Retreat was proposed to the meeting referred to by the late William Tuke. The proceedings of Pinel, however, were not, until long after, known either to the directors or managers of the Retreat; and it thus appears that the reformation in the treatment of the insane had an independent origin in the two countries at the same period."

The institution was first opened for the reception of patients in 1796, and the committee appear to have been singularly fortunate in the choice of its first officers, Dr. Fowler, the visiting physician, and the late George Jepson, the resident superintendent. The testimony to the merits of the latter individual we regret our inability to transcribe in full. "Rarely, we believe, has it happened that a project so enlightened has met with an agent so efficient for the carrying of it out."

We shall conclude our notice by quoting the report of our author on the experience of the medical officers of the Retreat, on the *questio vexata* of this branch of practical medicine—the employment of means of personal restraint:

"The subject of coercion during a refractory, violent, or otherwise dangerous state, or, in other words, the personal restraint of the insane by mechanical means, has of late attracted much attention. At the Retreat, from a very early period, it has been regarded more or less in the light of a necessary evil; and it has been one of the objects of the managers of the institution to resort to it as seldom as possible. The treasurer of the institution, who published his 'Description of the Retreat,' so early as 1813, when detailing the means of personal restraint then employed, observes, 'with regard to the necessity of coercion, I have no hesitation in saying, that it will diminish or increase as the moral treatment of the patient is more or less judicious.' But he immediately adds, 'we cannot, however, anticipate that the most enlightened and ingenious humanity will ever be able entirely to supersede the necessity for personal restraint.'

"In 1841, when the first edition of the 'Statistics of the Retreat' was printed, I made the following observations on this subject:—'Within the last two years the officers of some institutions have attempted, and in some instances apparently with great success, to conduct the management of even large hospitals for insane paupers, without resorting to such means of restraint. The important experiment of this description, if experiment it can now be called, which Dr. Conolly is conducting at Hanwell, must on all hands be regarded with extreme interest, as even if it fail in establishing that personal restraint can in all cases be abolished, it has already fully shown that it may be much more frequently dispensed with, not only with safety, but with advantage to the patient, than has hitherto been generally, if at all suspected. The officers of the Retreat have not hitherto thought it right, in every case, to dispense with the use of all mild and protecting means of personal restraint; believing that, independently of consideration for the safety of the attendant, they may, in some instances, be regarded as the least irritating, and therefore the kindest method of control. But though this is the case, they readily admit that they have derived advantage from the full conside-

ration of the subject, which the attempts at Hanwell, Lincoln, and elsewhere have induced, and that they remain open to further evidence upon the subject.'

"It is now more than two years since the foregoing was written, and I have the satisfaction of adding that, in practice, personal restraint has by degrees been almost entirely abolished. Whilst we hold ourselves free to direct the use of any means which the necessities of the particular case appear to call for, I am fully convinced that, in a well arranged and properly governed public institution, the instances where personal restraint can at all be considered needful are in truth very few, and that they will be found, almost exclusively, to consist of old or mismanaged cases.

"I have indeed no hesitation in stating what can hardly I think be doubted, that restraint of every description, not absolutely called for, has a tendency to excite in the insane the angry and vindictive passions to which they are only too prone, and thus to prolong the continuance of the disorder in curable cases, and to aggravate its character in incurable ones. The instructions to the attendants on this subject have already been given; and during the current year there has only been a single example of the application of personal restraint, and that was of the mildest possible kind. There has been no instance of its application since the 25th of January, 1843."

In a note at p. 105 of the first part, we are further informed that in 4 cases slight personal restraint had been had recourse to between the 25th of January, 1843, and the 8th of February, 1845—and in a letter recently published in another periodical, it is stated that no instance of its employment in any form has since occurred; so that the use of coercion in the institution may be regarded as virtually abolished. Dr. Thurnam adds—

"Whilst on the one hand I cannot doubt that the course and duration of many cases have been mitigated and shortened, and the character of the disorder rendered less virulent, by the disuse of restraint, I must here state that occasional inconvenience in the shape of alarm, and of interruption to the quiet of other patients, and also as regards the destruction of clothing and the breakage of glass, have, on the other hand, been connected with it. On the whole, however, that greater vigilance and forbearance on the part of the attendants, which under competent superintendence the comparative disuse of personal restraint more or less necessarily implies, have, I feel no hesitation in saying, been attended with a decided increase of comfort and decrease of irritation in those divisions of the establishment in which instruments of restraint were formerly not unfrequently resorted to."

We must now take our leave of Dr. Thurnam's volume, omitting, from want of space, any notice of the concluding portion—the '*Statistics of the Retreat.*' The value which our readers will attach to this work, will probably much depend on their views as to the importance of statistical investigation in general. For our own part, though fully alive to the cautions necessary in the application of numbers to the elucidations of questions of medical science, and to the fallacious conclusions which may be drawn from hasty, inaccurate, or too scanty observations, we believe that there are numerous points regarding the natural history of diseases, and the various causes which may modify their symptoms, progress, and results, and the effects of remedial agents, which can only be satisfactorily solved by the employment of the numerical method. This we think peculiarly the case as regards insanity, and we may appeal to Dr. Thurnam's work as an example of the interesting and novel results which may thus be deduced; and which, though they might be suggested by ordinary individual experience, are only capable of being established as facts, by calculations

founded on a large number of observations. Those only who, like ourselves, have been somewhat extensively engaged in statistical researches, will be able fully to appreciate the amount of labour and care which, to ensure the accuracy which throughout characterises Dr. Thurnam's work, must have been required in its compilation; and we cannot but regret that, by adopting a more simple and lucid arrangement, and condensing his matter into a smaller space, he had not presented his observations in a more attractive form. To the medical officers of hospitals for the insane, and indeed to all those engaged in the cultivation of medical statistics, his work will afford much valuable information as to the methods to be adopted in the numerical registration of the results of their experience, and the errors to be avoided in generalizing from them. To such we believe the abstract we have given will be a sufficient recommendation. To the general professional reader, works of this kind must necessarily be less attractive than those of a more practical description, and we have hence been the more anxious to present as full an analysis of it as our limits would permit. We feel convinced, however, that those who are engaged in the cultivation of this branch of practical medicine will find sufficient in the work itself to reward them for its perusal.

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#### ART. VII.

1. *Practical Surgery*. By ROBERT LISTON. Fourth edition.—*London*, 1846. 8vo, pp. 582.
2. *Lectures on the Operations of Surgery, and on Diseases and Accidents requiring Operations*. By ROBERT LISTON, Esq. F. R. S., &c.; with numerous Additions, by THOMAS D. MÜTTER, M. D., Professor of Surgery in Jefferson College, Philadelphia, &c.—*Philadelphia*, 1846. 8vo, pp. 566.

I. In the Fifth Volume of our Review, p. 457, we gave a somewhat lengthened analysis of the first edition of Mr. Liston's '*Practical Surgery*,' and extracted what appeared to us the most novel and interesting of his observations. The two next editions were allowed to pass almost unnoticed, their rapid succession being a sufficiently decided expression of the spirit in which the work was received by the profession, and amply bearing out the very favorable opinion we originally passed upon it.

The appearance of a fourth edition reminds us that opportunity has been given for the further development of the work; and, accordingly, after a careful perusal of the new volume, we propose to lay before our readers the chief additions to facts, and modifications of opinion, which we find therein contained. Such changes are looked for with interest, as the result of several years' additional experience to a surgeon who occupies a prominent and distinguished place in the ranks of his profession, and possesses large opportunities of pursuing practically the subject on which he writes.

The object, plan, and general arrangement of the work remain unchanged; the amount of information contained within its pages has, however, been increased; facts have accumulated, and the illustrations of particular forms of disease and methods of treatment have been enriched

by the addition of several interesting and pertinent cases, and many useful and admirably executed woodcuts.

Of the object the author has in view we need say little, and it would, indeed, be equally idle to insist here on the importance of the manipulative branch of surgery. Mr. Liston has the credit of having perceived and supplied a defect in the surgical literature of the country. Since the publication of Mr. Liston's work the deficiency it supplied has been pretty generally acknowledged, and more than one rival treatise has started into existence, proving, we believe, the increased attention drawn to the subject, and the greater anxiety of the junior members of the profession to fit themselves for the management of cases occurring in country practice, which, in years gone by, were at once and as matter of course transferred to the hands of the London surgeon.

Mr. Liston's volume may be described as a series of essays on the principal injuries and diseases which require manual interference on the part of the surgeon, together with directions as to the modes of proceeding which he has himself found most convenient and successful. As a guide to the advanced student, and as suggesting practical observations of the highest value to the practitioner, it is unsurpassed; though, as a complete text-book of practical surgery for the younger student, it is capable of improvement.

Mr. Liston tells us, in his Preface, what is and what is not to be expected from his work:

"The young surgeon ought, without doubt, to understand well the history of his profession, and this is only to be acquired by a perusal of the leading surgical works from the time of Ambrose Paré downwards to Wiseman and Heister. . . . The reader of this work would not be much wiser, however, or more capable of undertaking the management of difficult cases, were the author to enter into a long detail of what this or the other of the moderns have recommended, or what he has found by experience in practice to be useless or hurtful. It has been the writer's aim to present a practical work, to which the young surgeon, when in difficulty, can refer, in hope of finding concisely stated and without conflicting opinions, that which he himself has, after a good deal of experience, seen and ascertained to be the wisest and safest course to pursue under various circumstances." (Preface, p. v.)

In the general spirit of these remarks we fully concur. We are no advocates of the practice, too prevalent in French Manuals, of cramming professedly practical works with detailed descriptions of the particular operation of this surgeon, the modification of that, or the proposal of another, for arriving at the same end. It may be well to put on record, and keep for reference, in dictionaries and encyclopædias, these numerous and often small modifications in practice, as proposed and carried into operation at various times and in different countries; and though practically many such propositions may have been long superseded by better modes of operation, they still form interesting records of the amount of intellect and attention turned to the subject. Thus collected, they will be consulted by all well-educated surgeons, and may serve sometimes as a means of humbling the pretensions of many a candidate, too eager for distinction, who would produce as a new discovery some exploded theory or practice. But to introduce such particulars into a work intended to guide a

practitioner in the best management of particular cases, is to run the chance of producing confusion instead of giving confidence, and has the appearance, to say the least, of an ill-timed exhibition of learning. All this we are prepared to admit; yet we think Mr. Liston might have gone further into detail on some points: for although it would be a waste of time to detail every thing "he has found in practice to be useless or hurtful," it might often be worth while to give concisely his chief reasons for adopting and recommending this or that particular mode of procedure in preference to any other. The young practitioner, who consults the works of a practical man more in the hope of finding a decidedly expressed opinion to guide and strengthen his judgment, than with the view of refreshing his memory on the history of improvements in practice, will not consider the weight of the opinion lessened by a short statement of the grounds on which it is based.

We venture to throw out these suggestions because we are anxious that a work so worthy of its author should have its utility extended; and we are convinced that a brief statement, perhaps in the form of notes, of reasons which decided the operator in preferring one method to another, would be invaluable both to the practitioner and to the student.

We now pass on to a more detailed examination of the chapters which have received additions or undergone some change.

Appended to the first chapter, which treats of *the division of parts by the knife, and the means of suppressing hemorrhage*, we find remarks on the employment of astringents and escharotics, in which the author points out the preparations he has found by experience best adapted to fulfil certain special indications.

"Some will be found to serve one purpose well, others another. In fact, as far as I can apprehend, after some experience, every caustic has its own peculiar properties and uses, and no one of them can supply the place of others satisfactorily. If an escharotic is plainly wanted, the suitable one must at once be brought into play; they cannot with propriety or advantage be used indiscriminately." (p. 26.)

This is a point which may seem small and unimportant, but it is one which we have reason to believe does not receive the attention it deserves. On the one hand, it is too much the habit of would-be surgeons to trifle with caustics, and to apply them as a matter of routine practice in every case where they find a sore halt in the process of cicatrization; and, on the other hand, it is comparatively of rare occurrence for a practitioner to be called upon to use an escharotic to considerable extent, yet with an exact precision. The uncertainty of the action of caustics is, with many, almost proverbial. In one instance the surgeon is obliged to repeat the application again and again, in order to produce an effect he hoped to attain by the first use of caustics; whilst, in another case, he is perhaps annoyed to find, when the sloughs separate, that the soft parts have been more widely and deeply destroyed than he intended or expected. These are accidents which must frequently come under the observation of those who have an opportunity of witnessing much practice, and we are inclined to infer from them, not that the escharotics themselves are so uncertain in their action, but that there is very frequently a want of anything like a



fixed principle to guide in their selection and application. Mr. Liston finishes his observations on this head by the following characteristic advice:

“ In conclusion, if tissues are to be destroyed, let it be done at once, by the use of a proper and efficient caustic ; and this is much better for all parties concerned than the practice of piddling from day to day with little or inefficient applications, as is often done in lupus and other diseases. The patient's patience is worn out, and the disease thus often deteriorated.” (p. 28.)

In the chapter on *injuries of the bones* (part of which, as on fractures of the spine, for example, might with advantage be extended), we would draw attention to Mr. Liston's opinion on the importance of an early adjustment and elevated position in cases of fracture of the long bones. This he expresses in the following strong terms:

“ The object of the surgeon in the treatment of all fractures must be to obviate pain and suffering, to put the parts in the most favorable condition for being repaired, and to preserve the limb of its normal shape and length. All these indications are fulfilled by the same means, viz. instant coaptation, and retention of the broken ends in the most perfect possible apposition. The earlier the means are adopted, the greater and more immediate will be the patient's relief from suffering, and the less the surgeon's anxiety and labour ; excess of bloody and serous effusion will thus also be prevented, and the excited action kept within bounds. Prevention, it is admitted, is always better than cure ; and if the above recommendation is attended to, all necessity for local abstraction of blood, and for the use of lotions to cool the part, will be obviated. If, on the contrary, the limb is laid loosely on a pillow, in an easy position, as it is by some thought and said to be, muscles relaxed, &c., and no efficient means are employed to prevent the spasmodic action of the muscles, the startings of the limb, the jerkings of the broken ends, and the displacement of the fragments, then assuredly, in spite of all local and general measures, there will arise frightful swellings, pain, tension, and heat ; the intermuscular tissue will be gorged with blood, and the circulation of the limb roused to a dangerous and alarming degree. No application of soap plaster, in strips, gently around the limb, ‘ by causing secretion underneath it,’ can reasonably be supposed likely to diminish either the irritability of the muscles or the urgency of the inflammation ; nor will it. The splints, when resorted to, must be put upon a swollen and distorted member.” (p. 62.)

Experience and observation have already proved to us the propriety of this advice, and its soundness as a safe principle of treatment ; though, in this as in every other case, particular exceptions may demand a deviation from the general rule. In country practice, for example, where possibly both patient and surgeon have to travel some distance before the injury can be attended to, where consequently a considerable interval is unavoidably allowed to elapse before the necessary materials for “ putting up the fracture ” can be collected, and where the habit and constitution of the patient render him more prone to the early occurrence of inflammatory action, the surgeon may sometimes find it advisable to resort, in the first instance, to fomentations, evaporating lotions, &c., the limb being elevated, however, and just so far confined as to prevent it from starting, or the ends of the bones from riding over each other. But in town practice, and generally in hospitals, where competent assistance is always at hand, and where the exact nature of the injury can probably at once be satisfactorily made out, we believe that the necessity for delay in the adjustment of the fracture and the application of splints is of extremely rare occurrence. It surprises one who has been in the habit of following this

method to hear the surgeon of a metropolitan hospital recommend that a fracture should be left on a pillow for a few days, enveloped in strips of soap-plaster. We might ask what object is attained by the plaster that would not be equally well, or better, effected by the ordinary apparatus? A question of equally difficult solution is, what objection can be made to the application of splints and a moderately tight bandage, that does not equally apply to the employment of strips of plaster? The young surgeon who has been taught to expect the supervention of inflammatory symptoms as the natural consequence of breaking the bone, is rather confirmed in his erroneous impression by finding that, in spite of what he is led to believe the best precautions against it, inflammation sets in. It perhaps never occurs to him to suspect that the very precautions he observes may tend to produce that result. Let him, however, treat an equal number of cases on the principle of immediate adjustment and elevated position, and we feel convinced he will be surprised how seldom the evil he apprehends will actually manifest itself.

We must refer our readers to the work itself for much interesting matter in the chapter on *injuries and diseases of joints*. The indications of treatment are laid down clearly, and the subject is handled concisely and with strong common sense. One division of the chapter is headed "Resection of Bones," but the only operations described are those on the elbow and shoulder-joints. The methods of proceeding for the resection of the upper and lower jaws and clavicle are detailed when treating of tumours and morbid growths; and excision of the articular surfaces composing the knee and hip-joints, is not considered by Mr. Liston sufficiently encouraging in its results to demand any detailed notice from him. The value of the operation as applied to the elbow and shoulder-joints, is now generally admitted, though there is still a want of statistical information (more particularly as regards the arresting diseased action and preserving the after utility of the limb), a deficiency which can be supplied only by following up the history of persons so treated for several years after the operation. It would, however, appear to be established by facts, that the result of excision of the joints is most favorable in children; and this we think might have been expected. Nature at this early period of life has more power to repair extensive injuries, and to make up for deficiencies produced either by the ravages of disease, or by the means taken by the surgeon to arrest its progress. The material by which the uniting of the several ends of the bones is effected, after excision of the joint of a child, is more elastic and pliable, and forms a better substitute for a capsular ligament than the tough and irregular deposit that forms the medium of connexion in the limbs of older persons. The development of the parts, the growth of particular muscles, and the habits of the patient in using and managing his limb, are all probably more easily and thoroughly modified in the instance of a young person. Even in an adult, however, under favorable circumstances, a very serviceable limb may be the result of the operation; and with a careful selection of cases, there is every reason to expect that the operation will continue (as it has already done for the last few years) to rise in the estimation of practical surgeons. Happily, it is not so likely as some more brilliant operations to be too generally or indiscriminately employed. In the case of the elbow, for example, the

method of excision is so much more tedious and painful, so much more difficult of execution, and so much more severe in appearance than amputation of the arm, that the greater number of surgeons naturally lean towards the latter alternative, without perhaps sufficiently considering that the patient may feel amply repaid for extra suffering in retaining his forearm and hand.

Any undue preference of this kind will be gradually corrected as our information on the subject accumulates, and in the meanwhile we believe that the operation of excision has a better chance of being appreciated than if, instead of being received with hesitation, it had at once been over-praised and indiscriminately adopted. It might be said that excision of the joints has retrograded in consequence of being originally thus over-valued, because this method of treatment remained for many years comparatively unnoticed, until again urged upon the attention of the profession by Mr. Syme. But we conceive that other causes have been in operation to produce a fairer estimate of excision; for, as now modified and limited in its application, that mode of treatment is already on a very different footing from that on which it was placed by Park and Moreau. Speaking of the circumstances under which the operation has the best chance of success, Mr. Liston remarks :

“Such operations may be resorted to with propriety when the patient's general health has not suffered much, or when, from keeping the joint in a state of quietude, the discharge has abated, and the system recovered from the effects of the irritation and constant drain. When, however, the soft parts are much diseased, when, as is often the case, the disease is not limited to the articulating surfaces, or when the patient is reduced to a very low state by hectic, it may not be very prudent to try the experiment; for it has happened that, after all, the proceeding has not been followed by removal of the whole disease, or subsidence of the constitutional disturbance; and in the end amputation has been deemed advisable. And this latter operation, which alone and in the first instance, might have been borne up against, has at that stage been followed by a fatal result.” (p. 152.)

Excision, as employed in the case of the elbow, is seen in its most favorable application. It should be borne in mind that this joint is, for many reasons, particularly well circumstanced to ensure success. As regards the operation, the elbow is so situated and constructed as to render comparatively easy the full exposure and safe removal of the diseased surfaces of bone; and in reference to the final result a very useful limb is preserved, even in cases where but little motion remains in the new joint.

In performing this operation Mr. Liston always adopts the same lines of incision. The first is made in the direction of the length of the limb, and close to the radial side of the course of the ulnar nerve; the second extends from the centre of this incision, across the joint, to the outer condyle, and the two flaps marked out are then dissected from the bones and turned upwards and downwards. The advice given by some writers, to expose the joint by slitting up sinuses where they exist, appears to us more plausible than sound. Such a proceeding saves the patient very little, if any, pain; it may seriously hamper and embarrass the surgeon in the subsequent steps of the operation; and, lastly, it is not absolutely required, because all these sinuses readily and rapidly close when the cause of irritation is removed, and when a more direct and free exit for the discharge of pus is

established. The soft parts being reflected from the bones, Mr. Liston directs that the forearm should be flexed on the arm, and the ligaments at once freely divided. In this position he finds no difficulty in dislocating the bones, and examining the surfaces which enter into the formation of the joint, without first sawing off the end of the olecranon, as practised and recommended by Mr. Syme and others.

Cases are recorded of excision of the elbow, even in adults, in which the use of the forearm was retained in an extraordinary degree; and so far they would tend to encourage the early employment of passive movement of the limb after the operation. It is possible, however, in attempting too much, to lose all the benefit of the operation; and, considering the insufficient data we at present possess, it must be regarded as a delicate question how far the surgeon should aim at reestablishing the motions of flexion and extension of the forearm. On this point Mr. Liston appears to us to convey a wholesome caution, by not speaking in quite such sanguine terms as some writers have done:

"Unless the operation on the elbow be performed when the patient is very young, and the portions of bone removed be very small, the motions of that joint must of necessity be ever after very weak and unsteady. In the operations which I have performed on the adult, amounting in all to eight, for thorough removal of the ends of all the three bones, I have aimed at bringing about a kind of ligamentous ankylosis, by steadying the part for a long time by means of leathern splints. In fact, the patient, in order to use the forearm and hand to any advantage, will require for a time at least, to have some permanent contrivance to fix the forearm at a right angle with the upper arm." (p. 152.)

"In more favorable cases the patient will acquire very considerable power of motion in the false joint. He will be able to straighten and flex the arm almost as completely as before the operation. The union in these cases is not usually effected by ankylosis, but a dense fibrous tissue passes between the ends of the bones at all parts, and the whole is surrounded by strong ligamentous substance, to which the muscles are inseparably united. The fibrous tissue between the bones is longer or shorter as more or less motion is permitted." (p. 154.)

In Chapter VI, *On injuries and diseases of blood-vessels*, some interesting and instructive cases will be found of hemorrhage from ulcers and abscesses, produced by an extension of ulceration, from without inwards, through the coats of the larger vessels. Both arteries and veins are subject to this accident; but the latter class of vessels are more frequently so affected in the case of ulcers. Several instances, however, have been collected by Mr. Liston of perforation of the coats of arterial trunks and branches, to the extent of effecting a free communication with the sac of an acute or chronic abscess; and these have been noticed by us on a former occasion. (*British and Foreign Medical Review*, Vol. XV, p. 155.) The following case, which occurred in the practice of Mr. Cheyne, of Leith, is added:

"H. R., æt. 6, Nov. 1828, was treated for chronic peritonitis. Two years before this attack he had suffered from fever, with pulmonary symptoms, for five weeks. After recovering from this disease, he enjoyed tolerable health till within four weeks of his death. He was then seized with remittent fever, but no region or organ appeared particularly affected till the end of the first week, when the belly rather suddenly became tympanitic, and the child complained much of pain on pressure. The remissions became more distinct; and during the fourth week a dry and rather severe cough troubled him. At the end of this week, one day

about noon, he vomited a large quantity of blood; rallied a little; but in an hour after, upon vomiting more blood, he rapidly sank. On dissection, the peritoneal surface bore all the marks of intense inflammatory action having existed; the membrane was much congested and thickened. In the stomach about a pound of clotted blood was found. At the upper part of the descending aorta there was a small ragged perforation, leading to a cavity that might contain a walnut; this cavity, filled with coagulated blood, communicated with the œsophagus by an orifice capable of admitting the little finger. It was situated in the midst of the bronchial glands, which were more or less changed into a texture of a cheesy appearance. The lungs adhered pretty generally to the thorax by bands of old formation. In their substance there was a yellow tuberculous mass, of the size of a marble, and a few smaller ones in its vicinity." (p. 192.)

After describing the operations for applying a ligature to the external iliac and femoral arteries, Mr. Liston mentions the treatment of aneurism by pressure over the arterial trunk at some convenient point between the tumour and the heart, in the following terms:

"Within the last few years the treatment of aneurism of the middle of the thigh and ham by pressure, in a modified form, has been revived with very encouraging results. In all, some ten or a dozen cases, have up to this time (1845) been so managed. The practice was first employed by Mr. Hutton, and followed by some of the Dublin surgeons. A few cases have also been cured in this country; two of them under my care in the University College Hospital. The object in applying the pressure is not to stay entirely the current of blood, but merely to weaken it, so as to permit coagulation of the contents of the sac to proceed gradually until this process is completed. For this purpose the femoral artery has applied over it a pad secured by a sort of C-shaped piece of steel, jointed in the middle, and with a larger pad by which counter pressure behind is effected. . . . The skin over the vessel should be protected with plaster on the back of the thigh, by a well-fitted leather splint. The patient may have almost the entire control over the instrument, screwing it up or relaxing the pressure according to his feelings. It is by no means a bad plan to apply two such tourniquets, which can be tightened or relaxed alternately. By adopting this method a cure may pretty confidently be expected, and the patient is certainly saved from considerable risk, as from secondary hemorrhage and inflammation of the vein." (p. 222.)

The advantages of the treatment by pressure appear to us to be by no means limited to the avoidance of the dangers which are incurred by the application of a ligature in the usual manner; such as secondary hemorrhage from the separation of the ligature, and phlebitis. In the first place, the plan by pressure is one likely to be much more readily submitted to by patients than what is known to be a more severe and hazardous, and is believed (although erroneously), to be a more painful proceeding: and thus not only may many cases be brought under treatment that would otherwise be allowed to advance gradually to a fatal termination, but many more may be attacked at an earlier period than is now permitted under the contemplation of a different alternative.

The general principles on which the treatment by pressure is now based are so simple, and the precautions necessary to be observed so few and easily understood, that we may state facility of application as another recommendation—not, indeed, to the accomplished operator, but to the more numerous class of surgeons who, with fitting cases for pressure under their charge, are unwilling to grapple with the difficulties of an operation which requires in the operator a practised hand and confidence in his knowledge of the anatomy of the parts concerned.



Again, the mode of treatment by pressure has this advantage; it is capable of being interrupted for a time, or entirely stopped, if necessary. On the other hand, when a ligature is applied on the main trunk of an artery the step is irretrievable; and if the arrest of the circulation through the limb should prove to be too complete, and the usual signs and symptoms of mortification should in a few hours begin to appear, there is little to be done but to await the more unequivocal indications of failure, with the alternative of mutilation, or almost certain death, should the symptoms increase. Lastly, there is every reason to regard pressure as so safe a proceeding, that it may be applied in instances where the attendant circumstances forbid a more hazardous course of treatment.

It is perhaps rather premature to affirm that, even should the treatment by pressure fail, the patient is left in no worse position than before the attempt. The number of cases at present on record is hardly sufficient to justify so unqualified an assertion. If the case be of such a description that an operation by ligature is considered unjustifiable, the failure of the plan by pressure to produce a cure certainly implies no aggravation of the evil; whilst so far as even a temporary arrest of the progress of the disease has been effected, the patient must be considered to have gained by the treatment. But if, on the contrary, the case be of a nature which offers, at first, a fair prospect of success to the operation by ligature, it is possible that long-continued compression may produce such an alteration in the condition of the main artery, and of the parts immediately surrounding it, between the seat of pressure and the tumour, as to throw difficulties in the way of the subsequent application of a ligature, which would not have existed had not pressure been resorted to. And this leads us to remark that in cases of the kind last referred to, it may be a principle worth holding in view to apply the pad of the instrument at such a point as to leave sufficient room above for the proper application of a ligature (should such a procedure be found, after all, advisable) to a portion of uncompressed artery.

One of the most interesting and valuable parts of the volume is that which is devoted to a consideration of *morbid growths and enlargements*. As the principal object is the description of the variously modified operations that are required for the removal of these growths from different situations, Mr. Liston classes tumours according to the localities in which they arise; and only touches incidentally upon their differences in structure and general character. Speaking of some of these diagnostic signs between simple and malignant tumours—between those he would remove and those he would leave untouched, Mr. Liston takes occasion to reprobate, in strong terms, the frequent and indiscriminate use of grooved needles and exploring trocars:

“The surgeon, by studying well the nature and progress of growths in this situation (the upper jaw), and, in fact, in all parts of the body—by losing no opportunity of making examinations, and thus gaining the requisite tact, will be able to predict their internal structure with tolerable certainty. He will not be under the necessity of resorting to what, in many cases, proves a very mischievous and hurtful influence, the thrusting into their substance of a long, flat or round and grooved needle. I was consulted very lately by a gentleman labouring under a tumour occupying the floor of the antrum, who had shortly before suffered very severely in consequence of this idle curiosity; the action of the diseased part had



been roused, and for some time after considerable increase of bulk had taken place. A day or two after this there was presented at the hospital a case of tumour over the distal end of the ulna, of firm consistence and very painful nature, involving and compressing the dorsal branch of the ulnar nerve. There was cicatrix resulting from a punctured incision upon the skin covering it. The nature of the case was represented to the young woman, and extirpation recommended. She returned to have this done in two days, with a fresh mark of puncture upon the tumour. The operation was performed, and inquiry then made as to how this second mark had been made. She confessed that she had, in the interval, gone to another hospital for advice, had there the swelling explored, and suffered in consequence, as she declared, much more pain than that caused by the extirpation." (p. 314.)

Mr. Liston, however, admits the value of the information derived from this mode of examination, and countenances its employment in obscure cases :

"I had recourse to the practice lately in a very obscure case. A middle-aged woman was admitted into the North London Hospital, labouring under a large tumour situated in the lower part of the abdomen, and filling the pelvis. The bladder was displaced, and occasional retention of urine took place ; difficulty was always experienced in introducing a catheter, and it was necessary to depress the handle so as to convey the point of the instrument close to the symphysis. Indistinct fluctuation, it was supposed, could be detected by pressing alternately on the tumour above the pubes, one finger being placed on the projection in the vagina, the other on the external swelling; the os tincæ could neither be felt nor seen by the use of the speculum. A large grooved needle was introduced into the vaginal tumour, and a small quantity of pus was perceived. This was immediately followed by a large flat trocar, and nearly three pints of well-digested pus evacuated. The patient ultimately died ; the cyst was found connected with one of the ovaries, and contained, besides matter, a large ball of hair—no uncommon occurrence." (p. 315.)

We have not room for more than a passing reference to Mr. Liston's directions as to the various operations for the removal of tumours. The manner in which the whole subject is handled is well calculated to impress the reader with the conviction that the author is speaking from large personal experience and frequent trial of the particular modes of practice he recommends.

The first object in the performance of these irregular, and often extremely perplexing and difficult, operations is to make sufficiently free and extensive incisions through the skin, to ensure the complete exposure and most ready dissection of the chief attachments of the morbid growth ; and this "good beginning" is all-important, not only because it really shortens the suffering of the patient, by giving every possible facility to the after steps of the operation, but because it enables the surgeon to satisfy himself more thoroughly as to the entire removal of the diseased tissues. The next object, and one often of no slight importance, is so to arrange these incisions as to leave as little disfigurement as possible, either from loss of integument or from unseemly and conspicuous scars.

These considerations seem constantly present in Mr. Liston's mind ; and much ingenuity is displayed in his plans to carry out both objects. A reference to his directions for the removal of parts of the upper and lower jaws, on account of tumours affecting the bones, and ligature of erectile tumours not involving the skin, will sufficiently illustrate these remarks.

Nothing is said yet of the plan for the *subcutaneous* applications of ligatures, as lately proposed in Paris by MM. Ballard and Rigal, for the destruction of tumours in the thyroid gland, and applicable, it may be supposed, to cases of nævus. We can readily conceive, however, that the cases which would admit of this somewhat more complex proceeding are by no means numerous or common.

The chapter on *amputations* has received further illustration by several new and well-planned wood-cuts, but is otherwise little altered. Mr. Liston is well known to be a strong advocate for the method by flaps, and for leaving the dressing of the stump until all oozing of blood from the cut surfaces has ceased. In amputation of the leg he seems to prefer dividing the tibia and fibula to separating the limb at the ankle-joint; the latter operation, though lately so much recommended, is not described or noticed by him. Amputation at the elbow-joint is advised in cases of injury involving a great part of the forearm.

Several good cases will be found in the next chapter, illustrating the *lodgement of foreign bodies in the windpipe*, &c.; and a successful case of extraction of a button from the bronchus is also recorded. The patient, a boy eight years old, allowed the button to slip into his trachea whilst at play, and was instantly seized with extreme difficulty of breathing, threatening suffocation. On the tenth day after the accident, the irritation having in a great measure subsided, and simpler means having failed to dislodge the foreign body, the windpipe was opened by Mr. Dickin, of Middletown, the opening extending from the crico-thyroid membrane downwards for about an inch. A pair of long and slightly bent forceps being introduced, the foreign body was struck, and, on a second attempt, seized and extracted. Another case is related, in which a bullet was dislodged from the bronchus by the sudden inversion of the patient, who was strapped to a common chair and slung to the rafters of the ceiling. He was turned over three times and repeatedly shaken, and the third time the bullet rolled from his mouth.

*Lithotrity*. Though known to be a very skilful and successful lithotomist, Mr. Liston appears to have recognised and practised the operation of lithotrity at an early period; and, as far as we can judge from the terms in which he speaks of this mode of treatment, his confidence in its efficacy, within certain limits and under certain restrictions, is increased by additional experience, and by the improvements that have been effected in the construction of the crushing instruments. After shortly comparing the advantages of each plan under different circumstances, he adds, "of late years I have scarcely been obliged to have recourse to lithotomy at all in private practice." It is hardly necessary to remark that such an admission is of peculiar value, and comes with additional weight from a man who, with large experience and a high reputation as a lithotomist, may naturally be supposed to have a certain, even unconscious, leaning towards an operation in which he feels his strength, and of which the difficulties are universally acknowledged. Mr. Liston's estimate of the value of lithotomy and lithotrity is, we think, fairly and candidly stated. The accidents that may follow the operation of lithotrity, far from being overstated, might with advantage be dwelt upon more fully. These accidents—including inflammatory congestion of the parts around the neck of the

bladder and membranous portion of the urethra, hemorrhage, cystitis, impaction of fragments in the neck of the bladder, and distressing spasmodic action of the parts in the neighbourhood, retention of urine from this latter cause, or from the presence of coagula in the bladder, or from partial paralysis of that viscus, arrest of pieces of calculus in the urethra, swelling of the testicle, and even suppression of urine—should all be fully considered, not only for the purpose of detailing the appropriate means of treatment, but with the view of fairly setting before the surgeon who undertakes lithotrity the accidents which he may meet with, and must be prepared to avoid or overcome. The two methods of operation, as witnessed in one or two ordinary and perhaps successful cases, leave very different impressions of the dangers to which the patient is exposed—impressions which, we may add, are quite disproportioned to the real risk incurred. French lithotritists have, with great power, drawn a vivid picture of the horrors of lithotomy, in order, apparently, to induce to the belief that lithotrity, because it does not present to the eye all those startling accompaniments, is without its dangers. But this is a short-sighted policy; even assuming that these supporters of lithotrity are thoroughly convinced that the measures they recommend are much more frequently applicable than is generally supposed. This disingenuousness, as is the case with every, even the slightest, exaggeration or suppression of facts, produces a twofold mischief. The immediate effect of inculcating such prejudices on an inexperienced mind may be to lower the estimation of lithotomy; but the ultimate effect (and one is sure to follow the other) is an equally unjust depreciation of lithotrity, when it is found unexpectedly that a surgeon may, in attempting to crush a stone, occasion a more serious mischief than perhaps at once appears. In order that strict justice should be done to lithotrity, no less than that the surgeon should be able to apply it judiciously and effectually, the unfavorable as well as the favorable side of the picture should be exposed, and the operator put on his guard as to the difficulties he may encounter.

The chapter on *hernia* we only refer to in order to point out a decided advance in opinion on the subject of dividing the stricture in cases of strangulated hernia, without opening the sac—a change of opinion which is based on the most satisfactory of all foundations, namely, actual experience after a fair and judicious trial. All who are aware of the frequent occurrence of peritonitis after the ordinary operation for strangulated hernia, and reflect on the causes chiefly concerned in such cases in the production of so unmanageable and fatal a disease, must admit as important and reasonable the proposal made by Petit, considerably more than a century ago, of avoiding, if possible, the exposure of the peritoneal surface. As limited in its application by Sir Astley Cooper, namely, as applied to old and large hernia, the principle is now recognised by most surgical authorities; but the further extension of the principle, as argued for by Petit and Monro, and more recently by Mr. Key and others, has met with strenuous opposition from some, and neglect from many more, who were content, perhaps on theoretical rather than on practical grounds, to throw doubts upon the soundness of the suggestion. First, the possibility of effecting the object, then the expediency of making the attempt,

was called in question; and the result has been to delay the period at which, after a sufficiently extensive trial, an impartial judgment might be passed upon the practice. However, there are few improvements in medicine or surgery the expediency of which has been, from the first proposal, so manifest as to have escaped altogether a similar opposition. Indeed there may be said to exist a kind of professional *vis inertiae* which can be overcome only by degrees, because the objections it raises must be met, not by argument, but by facts; and these require time to be collected in sufficient number.

Six years ago Mr. Liston expressed himself doubtfully, and, on the whole, unfavorably, of the plan of dividing the stricture without opening the sac. "It is," he remarked, "rarely possible to effect the object, although there is no harm done by trying it in some cases; but in others, constituting the vast majority, the attempt must prove very unsafe." Now, however, after a longer trial, he recommends the method more decidedly and unhesitatingly. After describing the preliminary incisions for exposing a strangulated femoral hernia, for example, he proceeds as follows:

"If the surgeon aim at trying to give relief without opening the sac, as he always ought to do in cases of recent strangulation, he will now pass his narrow blunt-pointed bistoury betwixt it and the fascia, upon a director insinuated under the sharp and tight margin of the crural aperture (the director may often be dispensed with); then turning the edge forwards, slightly towards the mesial line, and raising its handle, he will divide the resisting fibres. He may now try to reduce the contents of the sac by pressure, taking especial care not to push back sac and all—rather a serious accident. . . . I have fortunately succeeded in effecting this object in a considerable number of instances within these few years; and it is a proceeding which I most strenuously advise the adoption of when nothing contraindicates it. The attempt can do no harm, it causes little or no delay; and if it is not successful, the sac, after all, is opened, and the operation completed. If it does prove successful, the surgeon's mind is relieved of an uncommon load of anxiety." (p. 557.)

In conclusion, it is scarcely necessary to repeat our earnest recommendation of Mr. Liston's work. Having on a former occasion expressed ourselves strongly on the subject, we can only add, that the present edition is, as it should be, even more worthy of our praise than its predecessors. The already practised surgeon will know how to value it; but the student and young practitioner stands more in need of a little warning to prevent him from forming a false estimate, not indeed of such works, but of his power to profit by them. The simple perusal of works of this high order, unless the student of surgery is careful, at the same time, to test his dexterity and correct his ideas by frequent and repeated trial on the dead body, will assuredly only prepare disappointment for himself and others. We are led to make this remark from having observed a tone which, more or less, pervades the whole of Mr. Liston's work, namely, that of rather under-estimating the difficulties of the measures he recommends. In one sense it would be unfair and unjust to assert that Mr. Liston's book is calculated to encourage an undue love of operating. A reference to his observations on excision of ovarian cysts, operations on cleft palate, division of muscles in cases of deformity of the spine, removal of malignant tumours, and

many plastic operations, will satisfy the candid reader that he speaks much less sanguinely of the results of surgical interference in what may be called doubtful cases, than many writers on operative surgery. There is, however, a difference between sanctioning the attempt at interference in cases that ought not to be meddled with, and affording to ill-qualified persons a certain encouragement to undertake a method of treatment which, though in itself perfectly justifiable, they are not prepared to carry out. The terms "easy" and "difficult," as applied to different manipulations, may, when measured by the writer's own standard, be perfectly justifiable, and yet may mislead the inexperienced reader, and tend to engender in him a wish to operate, founded on a false confidence in his own judgment and skill. We believe that there are many, and we fear the tone we deprecate is likely to increase that number, who, when they know the kind of operation called for in a particular case, and can repeat methodically the different steps of the proceeding, feel themselves competent to undertake it. To these we can only say, in the words of our universal authority, "If to do were as easy as to know what were good to do, chapels had been churches, and poor men's cottages princes' palaces."

II. The second of the two works, the titles of which stand at the head of the present article, is a republication, "with numerous additions," by Professor Mütter, of Philadelphia, of some nineteen Lectures delivered by Mr. Liston, at University College, London, and reported in the '*Lancet*,' during the years 1844 and 1845.

We have already said enough to convey an idea of the respect with which we regard Mr. Liston as an authority on the subject of operative surgery.

These Lectures, collected from the above source, embrace indeed many of the topics just noticed in his treatise on '*Practical Surgery*;' they are treated here with much spirit; are illustrated by cases in the lecturer's hospital practice, which must have been under the observation of many of his hearers: and are occasionally enlivened with a somewhat freer discussion than he allows himself in his more formal work, of the lines of practice adopted and recommended by other surgeons. The style is characteristic, forcible, and well adapted to excite interest and arrest attention. Several points are only slightly touched on, or are passed over altogether; some of these are taken up by Professor Mütter, who thus adds [within brackets] about two hundred and fifty pages to the general body of the work.

Except typographically, we cannot say that these two elements are dexterously blended. Some of the additions are themselves excellent and highly practical; but, taken together, there is a want of harmony with the Lectures themselves, not only in style (which might be expected), but in tone and spirit, in arrangement, and in the mode of carrying out the plan. Though, of course, the result of much reading as well as of actual practice, the lectures of Mr. Liston are stamped with a certain individuality; the lecturer so contriving to look at and weigh even the discoveries of others, through the medium and by the scale of his own experience, that the whole of the information imparted comes with much of the freshness of originality—a quality which does not generally characterize the additions of Dr. Mütter. The frequent recurrence of phrases, such as the following:—"The great majority are in favour of as a general rule;"—"Most surgeons



prefer ;"—"The weight of authority is in favour of ;"—"It is considered best ;"—or, such and such circumstances "render this operation a favorite with most surgeons ;"—give a character of feebleness to the writing, which no one who is acquainted with Dr. Mütter's high reputation as an operating surgeon, would think in any way applicable to the writer. Nor is this effect lessened by the detail of measures which are afterwards unceremoniously dismissed with the remark, that the proposal is merely mentioned "as one of the novelties of the day, and wholly unworthy of our confidence ;" or again, that "no surgeon of any note would condescend to employ so puerile a means ;" or, "it will soon be remembered merely as one of the idle whims of some inventive genius."

The Lectures are rapid sketches of the principal points of interest in a wide field ; many of the additions are representations, begun on a more elaborate plan, but hurriedly finished, and less striking in the general result. Mr. Liston, it may be observed, presupposes a certain amount of knowledge of the general principles of surgery, as already given at a former part of the course by his colleague, Professor Cooper ; and to this previous instruction he takes occasion to refer from time to time, when he passes lightly over abstract points, and adheres more closely to subjects of a strictly practical nature. Dr. Mütter proceeds on a different plan, and divides his subjects elaborately, as if giving the outline merely of a complete treatise. We can conceive two methods of oral instruction on such a science as surgery, each useful in its way, and yet each on such a perfectly distinct plan as to make it difficult (and only possible in an extended series of lectures) to unite the benefits of both. According to one plan, the lectures might consist of clear and graphic sketches of disease and its appropriate treatment, such as it exists in what may be called its typical forms, and always so connected with the relation of actual cases as to arrest and fix attention by rousing the hearer's sympathy. According to another plan, lectures might be made use of, less as a means of imparting particulars and minutiae, than as an opportunity of directing the mind of the student in the exercise of thought ; of pointing out to him the best and most credible sources whence to derive facts on the several subjects which present themselves ; of giving him the valuable habit of arranging his knowledge in the best and closest possible manner, and of helping him to analyse, and to form a just estimate of the comparative value of information derived from different sources. These two objects might, indeed, be borne in mind at the same time, and carried out together by one lecturer, step by step ; though we doubt the possibility of its being done in an ordinary course, in which a vast quantity and variety of matter is crowded. It would be a work of still greater labour to blend well together the results of two different minds directed to such different ends. We believe that Professor Mütter, therefore, was in error in wishing partially to combine elements of so different a nature and so difficult of combination ; and in attempting to make these lectures (what they were never, to all appearance, intended to be) a complete and independent work.

The most important and interesting of the additions made by Dr. Mütter are those on the subject of plastic and other operations for the remedy of deformities and deficiencies, both congenital and acquired, of the soft parts. As the arrangement followed by Mr. Liston is based merely on the locality



of the surgical diseases of which he treats, these observations and details of special operations are necessarily scattered through the work. This branch of operative surgery has been cultivated most perseveringly and with great success, by several surgeons in America, amongst whom Professor Mütter occupies a prominent position. His contributions to science, on this head more especially, are as important to the profession as we learn his practice has been creditable to himself and beneficial to those who have fallen under his hands.

The principle most clearly established by the repeated and varied trials of plastic operations appears to be that of making as broad and even a pedicle as possible to the transposed flap of skin, in order to leave an efficient vascular and nervous communication between the disturbed and the comparatively non-disturbed parts. The method of twisting the flap upon itself, (as in the Indian operation for the formation of a new nose) has, we believe, been adhered to longer and carried out further in this country than the success attendant on it has warranted. A much more successful plan (as Dr. Mütter shows from his own experience and from that of other surgeons) consists in cutting the flap in such a situation, that the direction it is made to assume is inclined at only a small angle from that of the surface from which it is taken. The pedicle need not then be twisted, and may be made broader than when this is requisite; or, still better, where it is practicable, separating the deep attachment of the skin, for some distance on either side of the cicatrix or part to be filled up, sliding the then moveable integument over the subjacent parts until the defective surface is completely covered; and, lastly, securing it in its new position by the usual means. As an illustration of this principle, we may follow Professor Mütter in some of his descriptions of plastic operations.

In the fifth Lecture, when treating of the mode of partially restoring the alæ of the nose, we find the following account of an operation performed by the Professor in the case of a gentleman who had lost "the whole of the right ala, as well as the adjacent soft parts, as high up as the os nasi of the same side," leaving a transversely oval opening, which measured three quarters of an inch across and half an inch in its vertical diameter.

"Seating myself in front I commenced the operation by making, with a small convex-edged bistoury, an incision extending from a few lines above the *superior* border of the orifice, to a short distance *below* its inferior, and directed *downwards* and *outwards*. It did not penetrate to the bone, but was sufficiently profound to allow a flap about *three* lines in thickness to be readily detached. This incision was completely on the *outside* of the cicatrix, a portion of which was subsequently removed, in order to prevent its hardened edges from irritating the raw surface of the flap, which was to be placed immediately upon it. One or two arteries were cut across, but the hemorrhage from them was arrested by pressure, until the *second* incision was made. This commenced at the *terminal* extremity of the first, and extended *horizontally outwards* about an inch. A *triangular* flap was thus marked out, and immediately detached from the subjacent bone by dissecting with the edge of the knife, held nearly parallel to the surface of the cheek. In the execution of this part of the operation two or three arteries of some size were necessarily cut across, and required the application of the ligature. The *third* incision, which extended from the *initial* extremity of the *first* to the point of the nose, was made with a pair of strong scissors, those being preferred to the scalpel, in consequence of this margin of the orifice being, to a certain extent, loose and unsupported. The triangular piece of cicatrix included between the superior ex-

termities of the first and third incisions, was then removed with the scalpel and forceps, and the sharp margin of the inferior portion of the opening also pared off, for reasons already stated. The hemorrhage having been arrested, and the parts properly sponged, the next step of the operation was undertaken. This consisted in the approximation of the first and third incisions, and the application of such measures as were calculated to retain the flap in its proper position. From the free dissection and the yielding character of the subcutaneous cellular tissue of the cheek, no difficulty was experienced in placing the edges in contact; and in order to ensure their perfect and close approximation, *four* stitches of the *interrupted* suture, made with saddler's silk, waxed and doubled, were passed. In addition, two or three small adhesive strips were applied to the spaces between the sutures. Finally, in order to prevent adhesion between the *flap* and the raw surface beneath it, and to give a better shape to the former, a small roll of soft lint, well oiled, was introduced into the *new nostril*." (p. 173.)

The sutures were not removed until the end of the fourth day, when union by the first intention was found to have taken place throughout. About the third week the lower margin of the flap was cut with a pair of scissors to give "the proper curve" to the ala. In consequence also of the septum of the nose being a little drawn to one side, apparently by the contraction of the flap, "the line of union between the base of the flap and the cheek was divided, cutting from within with a small scalpel, held parallel to the surface of the cheek, to the extent of three or four lines." The plug of lint also was increased in size, and a strip of plaster carried across from the tip of the nose over the sound cheek, so as to draw the whole organ in the opposite direction to that towards which it was inclined.

"At the end of the sixth week from the day of the first operation, it was determined to execute the 'third step' in the treatment. This consisted in the division of the skin and cellular tissue at the base of the flap, in a *semicircular* direction, the convexity of the curve looking outwards. The object of this incision was to give the peculiar rounded margin of an original ala, to diminish the fulness of the cheek where the natural depression should exist, which depression had of necessity been destroyed by the tension of the flap, and to permit a return of the perpendicular position of the deviated septum nasi. The incision was made with a small scalpel, and extended to the bone. In order to prevent the union of its margins, a small roll of oiled lint was introduced into the cut, and a strip of adhesive plaster applied to the tip of the nose, and fastened on the cheek of the sound side. . . . . On the third day the dressings were removed, and it was found that the margins of the incisions were nearly cicatrized and beautifully rounded off. . . . . This latter change was a very favourable circumstance, as it produced a depression in the exact spot at which it was required, in order to give a proper expression to the face. Had it not taken place there would have remained a *sort of inclined plane* from the bridge of the nose to the outer portion of the cheek. At the expiration of the ninth week my patient returned home with scarcely a vestige of his deformity remaining." (p. 176.)

Truly, American surgeons have very far outstripped their English brethren when they can take into account the "proper expression" of the face. We can, from this, better understand the contempt with which another of their surgical writers, remarking on the subject, designates "the fruits of the dexterous manipulations of most nose-makers" as being, "most frequently, little else than pug-shaped, flabby, and moveable knobs, that look more like small shrivelled potatoes than *bond fide* human noses."

Under the head of cleft-palate, concerning the operations for the relief

of which Mr. Liston speaks very discouragingly, Dr. Müller introduces some admirable, short, and practical observations on the chief sources of difficulty and danger, both at the time of and after the operation; together with a brief detail of the means available for obviating the effects of such untoward and often perplexing circumstances. Dr. Müller lays great stress on the advantage of simplicity in the form and construction of instruments to be used in these operations, and on the expediency of frequently handling the parts, for some days before operating, so as to "familiarise" the fauces to the presence of foreign bodies.

In the eighth Lecture is introduced an interesting account of a case of cancer of the lower lip, in which Professor Müller removed the diseased parts, and at the same time restored the lip by a plastic operation. The latter step, which consisted in detaching and drawing up from the chin two symmetrical side-flaps, which were then united along the middle line, was ingeniously planned, and appears to have succeeded admirably. The patient remained well when seen after an interval of two years. We doubt, however, whether surgeons in this country would not have been satisfied, in the first instance, with getting rid of the disease, and deferring any further proceeding until the tissues near the excised part should have cicatrised, and remained for some considerable period free from any appearance of return of the disease.

The treatment of deformities resulting from burns is also much illustrated in this volume by Dr. Müller. The author, as is well known, has published, in a separate form, several valuable essays on this subject; but as the principal of these were noticed by us in a recent number (Vol. XXXVIII, p. 396), we must refer to our former article for an account of the improved mode of practice recommended by him.

Dr. Müller agrees with Mr. Liston in strongly reprobating the performance of "ovariotomy" in the present state of our knowledge of the diagnostic signs of ovarian dropsy. He here takes occasion also to introduce part of one of his own lectures, in which this opinion is expressed, with the candid and prudent reservation, "that should the difficulties about to be stated ever, by subsequent observation and research, be removed," he will be ready at once to change his present views, and rank himself among the advocates of the operation. Then follows a good summary of the most important objections that have been urged against the proposal.

Large, and we are inclined to think disproportionate, additions are made on the subjects of hemorrhage, cataract, anchylosis, club-foot, and division of tendons, which our limited space obliges us to refrain from noticing further.

Dr. Müller's volume ends very abruptly. By way of completing the last subject (the treatment of calculous complaints), Mr. James Arnott is laid under contribution; and an account of the merits and principles of lithectomy, borrowed from that surgeon's paper, occupies rather more than ten pages. Unfortunately, Mr. Liston is not heard to the end; for his concluding lecture on lithotomy in the male, stone in the urethra, and on calculous and other diseases in the genito-urinary organs of the female is not here published. On referring to the "*Lancet*," we find that this lecture was reported in two parts, of which one appeared November 15th, the other December 13th, 1845; but neither, we suppose, reached Phila-

delphia in time for insertion in this volume. In his preface, dated January, 1846, Dr. Mütter says, "It will be observed that this volume contains all the lectures published up to the present date, but does not conclude the course. Another volume will be issued hereafter, should the publication of the lectures be continued." We should not have remarked on this little fact, had it not been a part only of the evidence the work affords of being composed with ill-judged haste; a haste which clearly originated in an anxiety on the part of the publisher to secure the profits of a promising speculation,—making it, as it would seem, quite a secondary consideration whether or not the work should be as worthy as possible of the two distinguished names under whose authority it appears. For this we do not conceive, nor would we for one moment be understood to imply, that the learned Professor of Jefferson College is answerable; but we cannot conscientiously acquit him of all responsibility. In default of a better and juster code of international law for protecting the fair interests and vested rights of authors (years of thought and study vested in print and paper), and defending them from the privateering system of the trade, we should like to see men of Dr. Mütter's character and position in the profession and in society, take their stand on the highest ground, and decline to append their names to hurried productions such as the present undoubtedly is. Nor is it expecting too much from them to do so, for lookers-on can see evident proofs of the ill effect the present system has upon their writings and upon themselves.

#### ART. VIII.

*Die Acute Entzündung der Serösen Häute der Gehirns und Rückenmarks. Nach eigenen Beobachtungen am Krankenbett geschrieben. Von Dr. JOSEPH NEISSER, Prakt. Arzt zu Berlin.—Berlin, 1845.*

*Acute Inflammation of the Serous Membranes of the Brain and Spinal Cord; described from Personal Observation at the Bedside. By Dr. JOSEPH NEISSER, Physician at Berlin.—Berlin, 1845. 8vo, pp. 454.*

AN apology to our readers is, we think, scarcely necessary for introducing to them a practical work on inflammatory affections of the brain. A correct diagnosis and treatment have an important social as well as a technical value. Insanity is considered by many to be a greater evil than death; imprisonment in a lunatics' hospital more distressing than entombment. We are convinced that a more correct diagnosis and treatment in this class of cases, would not only prevent the supervention of that form of chronic cerebral disease which constitutes insanity, but would obviate the serious mistake sometimes made by practitioners in the removal of cases of acute arachnitis to lunatic asylums as cases of mania. We know of more than one family on which the stigma of hereditary tendency to insanity has thus been fixed, and, we believe, we know also cases which have been maltreated in consequence of this mistake.

Dr. Neisser's work is divided into three parts: the first part, termed

prolegomena, is a general consideration of the pathology of serous inflammation; the second is a detailed account of seven cases of cerebral and spinal disease; the third (the epilegomena) is an essay on the pathology and therapeutics of acute affections of the cerebro-spinal axis.

The history of these cases is, we think, peculiarly instructive. The symptoms as they arose from day to day are considered in their pathological, physiological, and therapeutical relations; the prognosis, diagnosis, and treatment are carefully deduced from a detailed review of previous experience or empirical knowledge, and from physiological data; and the whole forms both a useful contribution to medical art and a valuable criticism on medical science. It is, in short, an excellent monograph on the class of diseases investigated and treated by the author.

The serous membranes have little physiological interest; their function is only of a secondary character; but when attacked by disease they become of immense importance from the great influence they exercise over the contained organ. Inflammations of the serous membranes within the thorax, for example, are amongst the most common and, at the same time the most dangerous diseases; they are also much more frequently the seat of inflammation than the subjacent organs, if we except the lungs; they are justly considered by the practitioner as the most important class of inflammatory affections that he has to treat; for, independently of the primary diseases in organs to which they give rise, they are fruitful in secondary or sympathetic affections.

The structure of serous membranes is compound: they consist of a substratum of cellular membrane, well supplied with blood-vessels, and an unvascular epithelium. The substratum of cellular membrane is made up of several layers of fibrous tissue, the fibres of which cross each other, and have a regular arrangement. Whenever the vessels of the substratum are congested, the unvascular epithelium is raised, and projects into the cavity which it invests. The superficial distribution of the vessels of serous membranes renders the latter liable to rapid inflammation when the former are congested; and facilitates effusion through the parietes of the former into the serous sac.

The external surface of a serous membrane is attached to the organs or muscles external to the sac by the subserous cellular tissue; its anatomical peculiarities differ according to the anatomical relations of the true serous membranes, and the peculiarities give rise to characteristic pathological differences. The more plentifully the parts to which this subserous cellular tissue is attached are supplied with blood-vessels, the more is the serous membrane itself predisposed to inflammation, on account of the free anastomosis which takes place between the vessels of the contiguous structures. Thus the pulmonary pleura exhibits the most intense inflammatory action at the roots of the lungs, where the great vessels and bronchi enter; and at the fissure between the pulmonary lobes, where there is a layer of loose vascular tissue beneath the pleura. For the same reason, inflammation of the arachnoid membrane is more frequent on that side in connexion with the vascular pia mater than on that next to the dura mater.

A somewhat similar anatomical relation to the connecting subserous

cellular tissue determines whether the inflammation be limited to the membranes or be extended to the organ it invests. Pulmonary pleuritis is rarely confined to the pulmonary pleura, and generally implicates the lungs, because its subserous cellular tissue is in immediate relation with the cellular tissue which connects the air-vesicles. Inflammation of serous membranes in direct connexion with fibrous tissues less frequently implicates the subjacent organs, because although the fibrous tissue is formed out of cellular membrane, it is comparatively less vascular.

The first stages of serous inflammation are the same as in other organs. Irritation being applied, the capillaries first contract, and so retard the circulation through them; then they are dilated and congested. Consequent on this is an effusion of serum through the parietes of the vessels, and the first result of serous inflammation is produced. The unyielding nature of the membrane itself facilitates the effusion as the dilatation of the capillaries is thereby restricted. If the inflammation ceases spontaneously, or is checked by art, the effused serum is readily taken up by the lymphatics; but if the inflammatory action continues, or increases in intensity, the blood-globules are heaped up in the capillaries, lose their colouring matter, and change their form; the blood then begins to coagulate within the capillaries; the lymphatics are no longer capable of performing their office, and the first stage of the disease, or the stage of inflammatory exudation, is accomplished.

The character of the exudation differs, however, with the progress of the disease. At first, when the capillaries are but slightly dilated, it is only watery serum; then plastic lymph or fibrin is effused, then pus; for the coats of the vessels become thinner and more porous as distension of them increases.

The products of serous inflammation differ in amount and character, from various circumstances which we need not enumerate, as these are well stated by systematic writers, and no doubt well known to our readers. False membranes are much less usual in the cerebral arachnoid sac than in the pleural or peritoneal; they are seldom, if ever, seen as a result of spinal arachnitis. Here the plastic power is at its minimum; in the serous membrane covering the heart and lining the pericardium it is at its maximum; the two being often strongly and intimately united by organized fibrin. The most remarkable exudation is that in which the colouring matter of the blood is effused, and sometimes even the blood itself. This variety is rare; it has been observed in inflammation of all serous membranes, but most frequently in that of the pericardium. Its true pathology is not yet expounded; but it indicates an intense and rapidly fatal disease.

Dr. Neisser discusses the pathology of serous inflammation as elucidated by microscopic researches, and especially the pathology of false membranes. The question of their formation is intimately connected with the physiology of development, and especially with the new doctrine of development by cells. There can be no doubt that researches of this kind will lead to new views both in pathology and treatment. What is the reason that the false membranes on mucous surfaces, as, for example, those formed in croup and in dysentery, never exhibit traces of organization? Why is the tendency to the formation of organized membranes



so great in pericarditis, so insignificant in arachnitis? Dr. Neisser also takes occasion to discuss the question whether there be really a *vis medicatrix*, and resolves it in the negative: the processes developed in disease differ in no respect from those by virtue of which the embryo is developed and the tissues repaired. In reality, these processes lead to death; as, for example, in pericarditis and pleuritis, in fatal cases of which we often find the so-called healing process has established a close and intimate union between the contiguous membranes; constituting in fact one of the most incurable results of serous inflammation.

Dr. Neisser discusses at length the semeiology, etiology, and complications of serous inflammation: he is evidently intimately acquainted with this theme, and makes many interesting remarks. We do not, however, propose to enter into this subject at present, and we shall therefore proceed at once to a notice of the cases. The first shall be introduced in our author's own words as an example of clinical inquiry.

“**FIRST CASE.** *Thursday, 16.7.40 [July 16th, 1840].* Gotthold, of young and vigorous constitution, aged 20 years, was received yesterday into the inner division of the Charité hospital at Berlin, in an unconscious state. We have no further information respecting him or his previous condition. The patient lies on his back, his head turned towards the right side, with closed insensible eyes, very red and turgid cheeks, and almost altogether unconscious. Respiration unequal and irregular,—a couple of short and superficial expirations, then a deep sighing inspiration; considerable feverish heat; quick and full pulse—104 powerful strokes. (?)

“*Hemorrhagia cerebri, apoplexia sanguinea, complicated with inflammation and fever.*

“**I. *Ægesis of the symptoms***; absolute significance of the symptoms of the functional anomalies considered alone. The first glance at the morbid condition leads to an indefinite opinion; the seat of the disease is within the cranium. The untaught readily distinguish cerebral diseases; he lies helpless there, his eyes closed, &c. We approach him, and investigate his condition; he mutters, but scarcely moves. There is sensual and mental disturbance.

“The disturbance of the intellect presents a passive form; he is insensible, and appears neither to hear nor see; no loud delirium, no excitement; no violent movements. Left to himself he remains apathetic; he speaks not, nor does he move. If questions be addressed to him, he certainly exhibits some tokens of consciousness, but these consist in a stammering of unintelligible words, which only demonstrate his delirium. There is a lively agitated movement of the right hand and foot to be seen occasionally, but the left extremities are never used. That is very significant; in fact, when the left hand is raised up, it falls mechanically as if by its own weight. If the *left* arm is pinched, he moves the right hand as if to prevent it. There is clearly also hemiplegia of the left side, and (as often happens) sensation is less impaired than motion. No reflex (involuntary) movements are excited by touching or pinching the affected limbs. The temperature is equally increased on both sides of the body. The countenance is unaffected, is not drawn. The eyes when opened are in the same axis, but insensible to light; the pupils are unaffected. No deviation of the point of the tongue,

either to the right or left. He does not swallow what is given him to drink, —a circumstance indicative of a dangerous degree of seizure.

“The respiration is affected in a remarkable manner; at the first glance we hear a slight sighing and groaning of the patient, as if from great grief.

“On a more minute examination we find it as follows: the patient makes 1, 2, 3 respirations, which follow each other at equal intervals, but are quite superficial; we see only the first pair of ribs slowly rise and fall. Then, however, there occurs a full, deep inspiration, with distinct expansion of the thorax, and very often accompanied by a sigh. Again 1, 2, 3, 4 superficial inspirations, interrupted by a deep and loud one, and so on. At the same time, it may be observed, that the left half of the thorax remains behind the right, and that the movements are effected almost altogether by the latter. Further, respiration *inæqualis*, unequal in the two halves; very abnormal in rhythm (*respiratio irregularis*), inasmuch as several superficial acts of breathing follow each (*resp. sublimis, superficialis*), then one very different, a deep full inspiration; *resp. profunda*, and almost *suspiriosa* besides. On the whole, the interval of time between two inspirations is rather prolonged, so that the respiration is not frequent.

“This manner of breathing is distinct from other anomalous modes of respiration, is easily distinguished from them, and is always easily recognized. It is so characteristic, that it is known in semeiology as *respiratio cephalica*, a symptomatic individualized form and indication, to be known at once without hesitation. It is a sign of diagnostic value.” (pp. 65-7.)

We have given the preceding as an example of our author's style. It is deficient in conciseness; it is in fact conversational. This, however, we conceive to be rather an advantage than a defect, because it detains the current of ideas upon each symptom, and renders the impression on the mind more distinct and lasting. As our pages do not admit of diffuseness, we must compress the author's views and strip them of their wordiness.

This *respiratio cephalica* is in truth an important symptom in diagnosis. It is sometimes absent, it is true, but is generally found to accompany pressure on the brain from whatever cause. It is pathognomonic of pressure on the motor centres, whether induced by serous or purulent exudation, hemorrhage, heterologous formations, as fungus, tubercles, &c., or direct injury to the skull, causing depression. If observed in children suffering from arachnitis, it is proof that exudation has already begun and advanced.

*Special diagnostic synthesis of the affection from empirical pathology.* Nothing is known of the previous history of the patient; no one can say anything about him, and he is unable to speak. It is true he is suffering from an affection of the brain, but what is the precise nature of the affection? It must have come on suddenly, and not long ago; his turgid, uncollapsed face shows that.

If it had come on at an earlier period, he would have been brought sooner to the hospital. It may be supposed to be apoplexia sanguinea, for the sudden hemiplegia, the loss of consciousness, and the character of the respiration point to this affection very distinctly. Opposed to this view, we have the increased temperature and the febrile pulse. Apoplexy from hemorrhage is characterized by a slow pulse and diminished temperature. If it be apoplexy there must be some complication, and the position of the head is in this respect a striking symptom. An unconscious apoplectic

patient lying supine, never holds his head in a constrained position. And on examining the muscles of the neck they are found to be tense, swollen, and so tender to the touch, that if but slightly pressed the patient's countenance expresses pain, and he utters loudly some unintelligible words. Now rheumatism of the nuchal muscles not unfrequently extends to the vertebræ, and even to the spinal arachnoid membrane, inducing inflammation and its sequelæ. This, then, may be a cause of the accompanying fever.

In cases of apoplexy there is a stage in which the pulse becomes quickened and febrile symptoms arise; namely, the stage of softening. But these occur in from five to ten days after the attack, and the pulse is small and weak, as well as frequent. The face is also collapsed. We cannot infer softening, therefore, in this case. Sometimes a primary and sudden softening of the nervous centres is accompanied by hemiplegia; but it occurs either in very young or very old subjects, and principally the latter. There is also more consciousness than in this case, and the countenance is pale and sunken, the pulse weak.

As there is, therefore, cerebral hemorrhage, in what part of the brain has it occurred? Opinions differ widely as to the functions of the different parts of the brain, and as to the result of injury to different portions. Serres, Bouillaud, Foville, and others, have laid it down as a theoretical principle that injury of the thalami induces paralysis of the upper extremities; injury of the corpora striata paralysis of the lower extremities. As these parts decussate, the seat of the extravasation in the case before us must be referred (if we adopt Serres' views) to the right thalamus and its contiguous corpus striatum. The numerical method has shown that in about half the cases of apoplexy, the thalamus first, and then, with an increasing hemorrhage, the corpus striatum and the parietes of the ventricles are implicated. Hemorrhage on the surface of the brain is very rare in adults; hemorrhage at the base of the brain determines that rapidly fatal and incurable form termed by French writers *apoplexie foudroyante*. It is inferred, then, that the seat of the hemorrhage is at the points indicated.

*Physiological and practical analysis of the symptoms, with reference to the seat of the hemorrhage.* Having derived from experience, or empirical pathology, what information we can, it remains to examine the symptoms in their physiological relations. The physiology of the brain is so imperfect, and the views regarding its structure, relations to the nerves, and the functions of its various parts, so discordant and contradictory, that little can be done by this method. We will follow our author, however, through his attempted analysis; and, firstly, we have to consider the mental disturbance—the suspension of consciousness. Since the gray substance of the hemispheres is considered as the seat of the mental functions, we have in this case an impression radiated from the corpus striatum and thalamus to the hemispheres. Secondly, the activity of the senses is abolished. This may depend upon the loss of power in the hemispheres themselves; but we have proof, in the insensibility of the pupils to light, that the function of the visual centre is affected, for the reflex action through the ciliary nerves is interrupted. This fact does not, however, point exclusively to the thalami as the seat of the extravasation, because in affections of the corpora quadrigemina and geniculata, and other deep portions

of the hemispheres, these seem connected with the sense of vision. It is also certain that this insensibility of the pupils is sometimes sympathetic, and the result of an injurious influence radiating on the visual centres.

Thirdly, the nerves, both of motion and sensation, are affected, though in different degrees. Indeed as all nerves, whether sensitive, motor, or sympathetic, end in the brain, we may expect that all will be affected in diseases of the brain, as in fact they are. We will take the nerves in order. *a.* The Facial nerves. These seem unaffected in this case; there is no dragging of the face to one side. In many similar examples this symptom occurs. It is to be noted, however, that it is often but of short duration; often in a few hours after the fit, the lateral deviation of the facial muscles disappears. Hence it is to be supposed that it is sympathetic and dependent on a radiated influence. It is usually on the same side as the extravasation; sometimes, however, on the opposite—a circumstance not yet elucidated by physiology. *b.* The Spinal nerves. The paralysis of the left arm and leg has already been noticed. It is worthy of remark that in most cases the leg regains its power of motion more quickly than the arm, and that the function of the nerves of sensation is less affected than that of the motor nerves. Dr. Neisser thinks this fact proves that the central axis of the sensitive nerves is not the same as that of the motor. He is of opinion that they end in the medulla oblongata, and that the disturbance of their function is sympathetic and dependent on a radiated influence. But even the motor nerves may be unequally influenced. A young man was placed under Schönlein's care, in the clinical hospital at Zurich, with incomplete hemiplegia of the left side, and lateral deviation of the facial muscles. The attack was acute; he had headache, &c., and it was pronounced to be a case of cerebral hemorrhage with congestion. A few days after, Professor Arnold was examining the affected muscles more minutely, and found that while the brachialis anticus was paralysed, the biceps could be freely moved. Now the two muscles are supplied with branches from the same nerve, and, therefore, Professor Arnold urged that it was a peripheral, and not a central or cerebral affection. All the symptoms of the case were, however, opposed to this view, as well as the known fact that individual nerves, and they only, may be affected in diseases of the brain. The respiratory nerves are derived from the medulla oblongata, and their origin is, therefore, far distant from the seat of extravasation. We, however, find them affected in the case under examination; and the probable conclusion is, that they suffer in the same way as the hemispheres, namely, by irradiation. It is worthy of special notice that the respiratory muscles in this case are hemiplegic; for the left side of the thorax is motionless. *c.* The Sympathetic or Organic nerves participate in the sensorial disturbance; the bowels are constipated; the urinary bladder becomes distended; and the activity of the cardiac movements is diminished. Whenever the brain suffers from compression, the pulse becomes slower; the more indeed the former is compressed, the more the latter is retarded, becoming at the same time larger and fuller. This is the *pulsus cephalicus* of the ancients. To the experienced practitioner the condition of the pulse is of great importance in the diagnosis. It gives often the first warning of commencing cerebral disease. In those cases in which the hydropical effusion takes place slowly and insidiously, without any symp-

toms referrible to the head, the retardation of the pulse is the first symptom that indicates the impending danger.

*Etiology of the affection.* What is the cause of the apoplexy in this case? It cannot be ossification of the cerebral arteries, or softening of the brain, or the apoplectic predisposition. These belong to advancing years. Young individuals may have apoplexy from two causes; namely, hypertrophy of the left ventricle, and ectasis or dilatation of the cerebral arteries.

*Ectasis vasorum cerebri* is a form of disease of which little information has hitherto been gathered by systematic writers. Dr. Neisser has seen it in two young men, aged from 20 to 30, and in a female with amenorrhea, aged 19—all characterised by a plethoric habit, short neck, thick head, stunted growth, &c. The disease most frequently attacks the basilar artery and its branches. These are simply dilated equally along their whole length, and not in aneurismal pouches. The blood moves, consequently, more slowly along them; in fact, the disease is not easily distinguished in its early stage from active congestion dependent on other causes. The patient is readily heated, complains of overflow of blood, headache, sensual disorder. These symptoms recur continually, in spite of energetic and methodical treatment; and at length become inveterate, and excite anxiety. The patient complains at last of vertigo, and a peculiar heaviness and dulness of the feet. He has neither pain nor formication, but walks with a tottering, uncertain step, staggering first to one side, then to the other, as if intoxicated. He is often so affected with vertigo that he falls, particularly towards evening, and in the dark. All the symptoms are aggravated at night, and remit in the morning. There are partial sweats on the head and face, and in some cases amblyopic symptoms. Dr. Neisser has found an energetic antiphlogistic and revulsive method perseveringly carried out, and under favorable circumstances, to be entirely inefficacious. Death is generally sudden. There is no reason to presume the existence of either hypertrophy of the left ventricle, or ectasis of the basilar artery, in the case under consideration. The probability is, that the extravasation is connected with the rheumatism of the nuchal muscles, and that there has been a metastasis to the brain or membranes.

*Prognosis of the case.* The impression on first looking at the patient is unfavorable. The intensity and duration of the stage of unconsciousness is, perhaps, the best prognostic sign. If an apoplectic patient lies utterly insensible to all stimuli, breathing stertorously, with open mouth and turgid cheeks, passing stool and urine unconsciously, &c., death is inevitable. On the other hand, a transitory loss of consciousness, with slight paralysis, by no means augurs an unfavorable termination. It is in the mixed cases that the greatest uncertainty arises. Here the patient answers, although unintelligibly, when spoken to. Neither is he worse than he was twenty-four hours ago. So far the prognosis is favorable; but the inflammatory action is a serious complication, and renders the termination doubtful.

*Treatment.* Venesection to 12 ounces; ice to the head; internally, nitre with sulphate of potass. The bleeding to be repeated, if the symptoms are aggravated.

Friday, 17th. Let us accompany Dr. Neisser on his visit to his patient,



and see what has been the result of the treatment. Bleeding was performed forthwith; the blood drawn had a firm clot, with two teaspoonfuls of serum. Immediately after the bleeding the patient opened his eyes; the pupils acted, and the paralysis of the left arm disappeared. A paralysis to disappear in two days! Was there really, then, extravasation of blood, or was the diagnosis of yesterday wrong, and there was only extreme cerebral congestion? Was it an apoplexia intra vasa, a paraplexia, a coup de sang? No; the paralysis is opposed to this opinion. In all forms of paraplexy, whether in plethoric young persons, or occurring as apoplectic molimina, there is very seldom, *if ever*, hemiplegia or complete paralysis. But the readiness with which the paralytic affection has disappeared points out another locality for the extravasation than that fixed on yesterday. Cases of sanguineous apoplexy are dangerous in proportion as the locality of the hemorrhage is distant from the surface of the brain. Hemorrhage at the base of the brain is the most dangerous and most fatal; hemorrhage on the surface of the convolutions is the least dangerous and least fatal. The latter occurs the most frequently in children, and constitutes the apoplexia neonatorum. The hemorrhage is usually consequent on exudation through the capillaries, or, according to Cruveilhier, from the smaller venous trunks—at least in children. In these examples, the apoplectic symptoms are much less dreaded and more gradual, than in other forms. The symptoms of the case in hand are of this character: the paralysis has been easily removed, the loss of consciousness is incomplete; the extravasation is not into the right thalamus and corpus striatum, but on the surface of the left hemisphere.

After a clyster, the patient had three motions; retching and vomiting took place whenever he drank liquids; he was restless, and had no sleep during the night. To-day he tosses his head about, sometimes his hands, sometimes his legs; he answers questions hastily and imperfectly, in a sort of half-delirious manner. The face is red, the eyes injected, the head and cheeks hot, the pupils contracted. It is found, however, that the nuchal muscles are no longer tense, and the head is moved about much more freely, and without pain; but there is considerable pain on pressure in the region of the third, fourth, and fifth cervical vertebræ. The breathing is quite otherwise than yesterday; it is frequent, deep, regular; the vomiting continues; the urine is scanty, and strongly saturated; the skin is hot and dry; the pulse is 92, large, and strong, but its rhythm is irregular; a few beats go quicker than the preceding, and then the rhythm becomes slower again—a significant symptom; a symptom to be carefully noted in all cerebral affections. The symptoms are now altogether changed; the diagnosis is difficult, but the grounds must now be stated for pronouncing it a case of acute arachnitis at the base of the brain, with commencing exudation of pus, and complicated with meningitis spinalis in the same (or purulent) stage.

What are the reasons for this diagnosis? The leading symptoms indicate to-day a state of cerebral irritation; we need not repeat them, but they were masked yesterday by the pressure on the brain. There was no restlessness, no active delirium, no shouting; the brain was in a state of torpor. Venesection removed the cause of this, and the true characters of the disease appeared. Now that this state of irritation is inflammatory



is manifest from the febrile symptoms, and that it is not encephalitis is probable, because that disease is rare; and, further, the rheumatic affection of the nuchal muscles indicates the serous membrane as the locality of the inflammation. It is to be noted, too, that a rheumatic affection of the nuchal muscles is a prominent circumstance in the natural history of arachnitis.

The nature of the inflammation is such that it must have continued three days; the period, then, for serous exudation is past; it is the period of puriform exudation. Systematic writers generally describe the two stages as being distinctly marked, the second being the stage of compression; but this, Dr. Neisser declares, is quite wrong. There is always a transition-stage which complicates the diagnosis much, because it complicates the symptoms. The symptoms of compression are at first mingled with those of irritation, and it is only near the fatal termination of the case that the latter disappear. In the case before us the sensibility of the pupils, the active delirium, the expression of pain, and the vomiting, are all marks of irritation. Vomiting, like the peculiar pulse, is a specially important symptom in arachnitis. It is usually preceded in children by a tendency to yawn and by nausea; when fully established it is easily excited by the ingestion of medicine or food. The matter vomited is generally a watery mucus, occasionally mixed with bile, and it is rejected without much effort. Andral limits the symptom to the first, or purely inflammatory, stage of arachnitis; Dr. Neisser has found it frequently present in the second stage. He, therefore, concludes that the vomiting of yesterday indicated that the arachnitis had already attained the second stage, and that morbid products were formed. The febrile symptoms then observed were dependent upon the active inflammation going on; the compression of the brain masked the general symptoms of arachnoid inflammation.

The prognosis is now most unfavorable; there is no hope for the patient except in active treatment; arachnitis, if not checked, invariably conducts to death. In proposing a repetition of the bleeding, Dr. Neisser makes several practical remarks on that operation, for which we regret we cannot find room. Schönlein thinks that if the product-stage has arrived, all remedial means only hasten on the fatal termination. Dr. Neisser thinks, however, that cures of arachnitis in the second stage are sufficiently numerous, and that Schönlein is wrong in his therapeutics. Andral, Abercrombie, Portal, &c., forbid venesection, but recommend leeches. Dr. Neisser thinks that no rule can be laid down, but that leeches only can be of but little use in so intense and dangerous a form of inflammation as acute arachnitis. It is certain that bleeding in the latter stages may hasten on dissolution; the pulse becomes suddenly weak and fluttering, and the patient sinks rapidly. A careful diagnosis will, however, indicate the cases in which general bleeding is inadmissible. In the present example, Dr. Neisser prescribed bleeding to eight ounces, twenty leeches to the neck, ice to the head, calomel and jalap to act on the bowels.

Saturday evening, the 18th. The patient's relatives have stated what happened to him. On Tuesday he complained of hemiplegia, and on Wednesday he fell into the unconscious condition in which he was brought to the hospital. This circumstance confirms the diagnosis that the form and site of the hemorrhage was as last indicated; namely, capillary exu-

dation on the surface of the left ventricle. The palsy preceded the loss of sensibility, and came on gradually—an important distinction. The patient did not, as was expected, experience any evil results from the venesection. The pulse was less irregular; the vomiting had ceased. In other respects the symptoms varied little. There was excessive pain in the neck, but more composure of manner; sleeplessness during the night, and active delirium. We shall not follow Dr. Neisser through his estimate of the symptoms; but the result is a different diagnosis to that on the preceding. Now it is arachnitis of the cervical cord extending to the medulla oblongata and base of the brain; in the first or inflammatory stage, the prognosis is more hopeful, but doubtful, on account of the persistent sleeplessness. These opinions seem principally based on the greater rationality and mental composure of the patient, a state of things incompatible with the more advanced stage of the disease. The increased cardiac action is manifest in the exquisitely inflammatory pulse, and indicate the necessity of a repetition of the bleeding, which is ordered to the extent of one pound, but without leeches. We give the result verbatim:

“Sunday, 19th. We have a foreboding of evil as we approach the bed; there is an ambiguous character of repose and passiveness, an inexpressive eye. The pulse hurries on at the rate of 130, small, and weak. It is all over; the patient is dying. It is the last stage, the stage of exhaustion, the herald and forerunner of inevitable death. . . . . After the bleeding there was a gradual abatement of the excitement. The pulse lost its tension, but remained strong and good, and the whole appearance and condition of the patient was satisfactory. An evening exacerbation was to be expected under the existing circumstances, and an increase in the symptoms. It came, was rather severe; so that bleeding to six ounces was again resorted to. A satisfactory remission followed, and an improved condition. The patient also fell into a quiet sleep, which continued for two hours. Towards morning he again became restless, but weakly, and without energy. He muttered deliriously, the pulse sank, became frequent, profuse diarrhoea set in, and the stools were passed involuntarily.

“Such is the history of the circumstances that now appear before us. He lies in a constrained position, apathetic, muttering to himself, whether addressed or not, unintelligibly, inarticulately; no trace of consciousness; the eyes dull and staring; the temperature neither increased nor diminished; the pulse as described.”

Such is the closing scene of the pathological drama which Dr. Neisser has written for the instruction of his readers. To them, at least, the catastrophe is in the post-mortem. It is as follows:

The dura mater was injected, the inner membrane dark red; much congested; could only be raised up in large patches, and with difficulty; and a quantity of reddish serum was collected beneath the arachnoid. There was an extravasation over nearly the whole of the convolutions of the left hemisphere, consisting of a thin layer of blood, partly coagulated and partly fluid, and dark. Both the pia mater and the compressed convolutions were at this point much congested, but no arterial or venous lesion was perceptible. The cortical substance was very vascular, and the medullary showed numerous red points. The parietes of the lateral ventricles/ the thalami, &c., were all healthy; the ventricles contained some bloody serum. At the base of the brain, and particularly about the medulla oblongata, and the adjoining portion of the cerebellum, there was a

thick, dirty yellow fluid, a purulent exudation between the arachnoid membrane and pia mater, the latter being much reddened and infiltrated with a watery fluid. In the dorsal region of the spinal cord there was a well-marked reddening of the membranes, which contained between them a collection of a yellow, thick, putriform fluid. The adjoining portions of the cord were affected, but the pia mater was much less reddened and infiltrated in the cervical region than in the dorsal.

*The Epikrisis.* We have now before us a tolerably complete statement of all particulars connected with this instructive case. It remains to institute an epikrisis. We must review the symptoms and see where the observer was right, where wrong. In the first place, the opinion as to the existence of a cerebral hemorrhage, and the judgment founded on the gradual accession of the paralysis and loss of consciousness, or the less degree of their intensity, and on their ready removal, as to the seat of the hemorrhage, are both confirmed by the inspection of the corpse. It is also apparent that the hemorrhage was not from an opening in a vessel (diabrosis), but was an exudation from the capillaries (diapedesis), consequent probably on the febrile excitement and inflammatory congestion. This mode of hemorrhage is also indicated by the *gradual* accession of the phenomena; if consequent on the rupture of a blood-vessel, the fit is more sudden and instantaneous. The paralysis was on the same side as the hemorrhage; this is an interesting circumstance, and is certainly an exception to the general rule. We do not yet know sufficiently the relations of the various parts of the brain to explain the differences. It is at least worthy of notice, that when there is decussating hemiplegia from hemorrhage into the thalamus or corpus striatum, it happens that the facial nerves are affected as often on the same side as on the opposite. Now the facial nerves take their origin above the decussation in the pyramids; why then are the facial muscles not subject to the same rule of paralysis in hemiplegia as those of the trunk?

The patient was restored to consciousness immediately after the bleeding; it could not have effected a removal of the exuded blood, because no bleeding could have effected that. It must then have been by relieving the congestion. Various considerations lead certainly to the opinion that pressure alone cannot always or indeed usually induce the unconscious state. It is more probably the "shock." We know that in cases of injury to the head, the brain becomes accustomed to considerable pressure without any aberration of function. So also in chronic arachnitis (hydrocephalus) and in diseases of the brain characterised by hypertrophy.

The condition of the pulse in different stages of the cases is worthy notice: on the first day it was 104, inflammation predominant; on the second day 94, and arrhythmical—a mixed stage of inflammatory action and purulent deposit on the brain; on the third day 105, tense and regular—pure, intense inflammation; on the fourth and last day 130, and weak—the brain exhausted.

There was dorsal meningitis, as intense as the cerebral, yet the most prominent symptom did not exist; there was not the slightest affection of the spinal nerves. Probably the attack was inevitably fatal. We believe, however, that the treatment was bad, and that it hastened the mortal

event. The bleeding was carried much too far. We are of opinion that if the learned doctor had kept his reasoning to himself, and left Nature to her own devices, there would have been a better chance for his patient. He will know better ere he is threescore. But we must pass on to the second case, of which we must make shorter work than the last.

"February 15th, 1839.—We come to the bedside of a young man who is unconscious, with his eyes closed, speechless, motionless, his head and neck spasmodically drawn to the left side. From time to time there is a spasmodic agitation of the hands and fingers. Respiration quick; pulse sunken, 144. *Peracute arachnitis at the base of the brain*. The inflammation is remarkably intense, and has attained already to considerable purulent formation. The disease has done its worst; it has exhausted the vital powers of the organism; the assistance of art is too late. It is the beginning of the end.

"The patient, a young man aged 19, of delicate constitution and weak muscularity, was brought last evening to the Charité Hospital. delirious and unconscious. We learn that already for several days he has been in a delirious unconscious state, and has had a rose eruption on the feet. A physician was called to him yesterday, who ordered venesection and leeches to the forehead, and sent him here. After his arrival last night, he was attacked with opisthotonos for two hours, and was delirious the whole night. He is girt with a broad padded belt, to which soft leather rings are attached to receive the hand; a circumstance that convinces us he must have been raving violently, as these means of restraint are only used here as the last extremity, and when danger is apprehended.

"Let the reader imagine a young man stretched on the back, his head turned to the left side, the eyes closed, the countenance placid, as if he were asleep. Only a loud frequent respiration and convulsive movements of the hands and fingers break at short intervals the singular stillness which impresses the mind. The extraordinary appearance of such a patient rivets the eyes, and shocks the senses as well as the feelings. The impression made by the morbid phenomena is so powerful, because the uncommon quietude, the death-like insensibility of the patient, together with the unchanging unnatural position of the head, contrast frightfully with the powerful convulsions and the hasty elevation of the hands; the patient appears inwardly fettered by some morbid power, and the soul of the spectator is filled with irrepressible sensations. . . . He gives no answers to our questions, he takes no notice of us. . . . The face exhibits no capillary congestion, nor any thing particular; it is neither remarkably hot, nor red, nor pale; the tone of the facial muscles is normal; neither the lips nor teeth are spasmodically compressed; there is no distortion of the mouth, no spasm of the eyelid, no lateral deviation of the face. We open the eyes—the patient is insensible to light, but the pupils are normal, and contract. The nuchal muscles are not swollen; if, however, we attempt to straighten the neck or the head, there is an expression of pain on the countenance, and the head is restored to its unnatural position. The respiration is loud and feverishly quick, but rhythmical, full, and deep; no vomiting; abdomen soft and natural; region of the bladder free. The extremities extended, but not paralysed. . . . We can say nothing of the secretions (except that there is a perspirable skin), for the patient passes urine and fluid stools involuntarily." (p. 109.)

We have not space to follow our author through his diagnostic analysis of the phenomena in this case, his prognosis, his etiology, or his physiological estimate of the symptoms, in which he discusses several points in cerebral physiology, but go on to

*The treatment of arachnitis by cold affusion and the actual cautery.* Of course, the nature, intensity, and stage of the affection in this case show

that it is hopeless, but Dr. Neisser takes occasion to discuss certain means most likely to be successful in arachnitis. These are two: the actual cautery and cold shower-bath.

The actual cautery has the appearance of being a most painful and severe remedy. But it is in fact quite the contrary. It is surprising to observe, when applied without previously acquainting the patient with the intent, how little pain it gives. Dr. Neisser observes, that he has seen even children endure it without a cry, or appearing conscious of its application. We have ourselves been astonished to hear no expressions of pain when the living flesh has been hissing and frizzling beneath the glowing iron. Dr. Neisser states, that he has known men in perfect possession of their natural sensibility describe cauterizations several inches long as causing simply a sensation as of warm water being applied. Its remedial power by no means consists in a sudden shock to the system, for this it does not effect. It is an active derivant and counter-irritant. Dr. Neisser has found the actual cautery to act like a charm in checking deep-seated inflammation and exciting absorption, or rather in affording an opportunity to the so-called *vis medicatrix* to come into play. In diseases of the bones and articulations, its utility is great. Dr. Neisser founds an opinion as to its *modus operandi* on the well-known tendency to metastasis, exhibited by inflammation of serous membranes. The artificial irritation determines a metastasis of the original inflammation to the part irritated. It is necessary, however, that there be a thickness to muscle, or of cellular tissue between the cauterized and the inflamed part; otherwise it will only aggravate the existing inflammation. Its application to the head, or to the elbow, knee, feet, and fingers is inadmissible. Nor if the fever be violent should it be used, as it will, under such circumstances, be more injurious as a direct irritant than useful as a counter-irritant. It ought not, therefore, to be used in cerebral affections accompanied with active delirium and great irritability.

The effects of the cold douche, or shower-bath, are remarkable. Applied to a patient who is unconscious and insensible (as in the example under consideration), its first effect is a powerfully shuddering impression. The patient appears terrified, is aroused for a moment from stupor, sees, hears, and sometimes speaks; he complains, is restless, &c. Children especially exhibit great anxiety, they shiver, their teeth chatter, they cry aloud, weep, &c. If repeated, the same phenomena again appear, and the surface becomes cold, the skin contracted (*cutis anserina*), and the blood seems concentrated in the heart and lungs. The respiration is hurried, the pulse sinks. If the patient be rubbed dry and carried to a warm bed, in a moment or two all these urgent symptoms gradually disappear; the pulse regains its tone, the skin becomes warm, but instead of its former burning heat, it is now moist and agreeable. The kidneys secrete urine profusely, and the patient feels so comfortable and so much relieved, that he sinks into a gentle slumber, and will often request a repetition of the douche.

The cold douche is, however, a most potent depressing remedy, and should be used with great caution. Dr. Neisser says, that if continued long enough, it will induce so great prostration in the strongest man, that reaction will not easily be brought about. The frequency of its repetition,



and the quantity and temperature of the water are to be decided by circumstances. The greater the heat of the patient, the cooler may be the douche. For an adult, from three to eight pailfuls of water may be used; and for a child, the same number of pitcherfuls, continued from two to fifteen minutes, according to the strength of the patient and the effect produced. A child should be placed in a tub half filled with warm water. The skin, if it perspires, should be carefully wiped dry, and the chest and neck wrapped up in a napkin to prevent the cold water touching them. This precaution should never be omitted, but it is doubly necessary when the disease is complicated with a thoracic affection or tendency to tubercular deposit. A little water should be first sprinkled on the child's face, and then poured in a broad stream on its head; an interval of a few seconds should elapse between each affusion. When the operation is completed, the child should be quickly rubbed dry, wrapped in a warm blanket or flannel, and put to bed. Dr. Neisser thinks the practitioner should himself perform the operation, or, at least, personally superintend it, and see the patient shortly afterwards.

Moderate cold affusion is a useful remedy in many cerebral affections. In passive hyperæmia or venous oppletion, such as is met with in drunkards, in delirium tremens, in narcotic poisoning, in congestion consequent on exanthematic fevers, and in low fevers (*febris nervosa stupida*). It is also a very valuable remedy in irritable states of the brain with active congestion, and in the premonitory stage of apoplexy. It is strongly contraindicated, however, in all cases where the debility is great; it is, therefore, inadmissible in the last stage of arachnitis. The end of the first, and the transition stage to the second, are the periods when it is most likely to be useful in that disease. In mania, and in all cases in which erethism of the brain is intense, the slightest degree of cold affusion is strongly antiphlogistic, and may be used and continued, until that state of prostration is attained to which we have before alluded. Care, however, must be taken with this, as with all other lowering means, that it be not carried too far.

After this long digression (and we are bound to say that Dr. Neisser makes a much longer, for he discusses at length the physiological action of cold affusion) we will return to the case in hand. It is apparently hopeless; but the zealous practitioner is true to his life-saving *métier*—"while there is life there is hope," is the maxim most deeply impressed on his mind. To gain time in acute affections is a leading indication; to push on the clogging weary wheels of life is his main object, until mayhap the cycle of disease is completed, and the struggle ends in victory.

The treatment Dr. Neisser adopted in this case was cold affusion, and the actual cautery to the nucha; ethereal oil of mustard to the scalp; poultices sprinkled with camphorated spirit to the whole surface of the leg. The patient died on the 16th.

*Post-mortem examination.* The dura mater was sound; the vessels filled with fluid blood. On the convex surface of the brain the membranes had a red appearance, in consequence of injection of the vessel of the pia mater; a small quantity of reddish serum was effused between the latter and the arachnoid. The substance of the brain was firm; the ventricles containing serum, but not distended beneath the arachnoid membrane at



the base of the brain; there was a layer of a whitish green puriform fluid, which covered the under surface of the anterior lobes of the brain, the fissura Sylvii, the olfactory nerves, together with the pons, medulla oblongata, and the adjoining surface of the cerebellum. The subjacent brain was reddened, but not softened; the pia mater was not adherent to it. The spinal arachnoid membrane was filled with a flocky serum; the posterior surface of the upper cervical portion of the cord was covered with a purulent exudation, like that found at the base of the brain, and (like it) contained between the pia mater and arachnoid, the former membrane being tumefied and infiltrated.

The leg was inflamed and infiltrated with pus and dark red serum.

*Complications of arachnitis.* The next case is interesting, as an example of arachnitis complicated with certain morbid changes in the brain. The patient was a confirmed drunkard, had already suffered from delirium tremens, and was sent to the hospital by the attending physician as labouring under that disease. The characteristics of the delirium, peculiar to the latter disease, were wanting, while the other symptoms pointed at an inflammatory affection, but so doubtfully, that it was manifest there was some complication necessary—some complication of the disease which ought to be unravelled. There are several complications: the first is arachnitis with inflammation of the cortical substance of the brain; this occurs in about one half the cases of arachnitis; it does not, however, alter the series of symptoms. Another complication of arachnitis is well-marked congestion of the brain. Congestion of the cortical substance is probably a complication of nearly every case of arachnitis, on account of the intimate and direct vascular connexion between the pia mater and the brain; but this again makes no change in the symptoms. It is quite otherwise with the much rarer complication of arachnitis, namely, congestion of the medullary substance. In these cases the symptoms of excitement are masked by the congestion until after bleeding, when active delirium sets in, and the patient is apparently worse, although the change is really due to the removal of the pressure.

There are examples of congestion of the capillaries complicating arachnitis; but there is also a venous congestion, a hyperæmia of the cerebral veins. This complication is not often alluded to by systematic writers, but it is an important one; it is usually seen as the sequel of all habitual congestive states of the brain, but is never so constant as after the abuse of alcoholic drinks. In death from delirium tremens, the venous sinuses are not only turgid with blood, but the veins generally even to the small branches in the pia mater are congested and turgid, often giving rise to a thin lymphatic exudation, which is to be distinguished from inflammatory exudation, and not confounded with it, as has been done by some writers. A similar exudation takes place to a more or less extent in all parts of the brain as well as into the ventricles. The symptoms in the case under consideration tally with those which might be expected to occur in such a pathological state, but with this difference, that an actively inflammatory condition has aggravated the state of congestion, while the symptoms of the former have been masked by the latter. The diagnosis then is purulent arachnitis at the base of the brain, with serous effusion into the ventricles, caused by hyperæmia of the brain.

The prognosis was of course in such a case highly unfavorable. The treatment was as follows: 20 leeches were ordered to be applied to the head, and four grains of calomel and ten of jalap every three or four hours; on the next day 15 more leeches, and the cold affusion carefully applied; on the third day of treatment the symptoms became more threatening, and the patient gradually sunk until the evening of the seventh, when he died comatose. We will now proceed to detail the post-mortem examination, and see if the diagnosis was correct.

*Post-mortem appearances in the brain of a drunkard dead of arachnitis.* A quantity of clear serum was found effused between the two serous membranes; these were quite distended with serum, the arachnoid having a bladder-like appearance, besides being full of blood, in consequence of the highly congested and distended state of the veins over the whole convex surface of the hemispheres. Between the pia mater and arachnoid, besides a clear serum, there were found delicate fibrinous coagula in the form of narrow and broad threads adherent to the veins. Some of the venous trunks had a true varicose appearance. The cortical substance of the hemispheres was dark red, but otherwise healthy; so also the first slices through the medullary substance. On going deeper, however, the corpus callosum, with the adjacent septum pellucidum, and fornix were found much softened, and fell under the touch into a pulpy mass of a dull tinge, neither yellow nor red. The lateral ventricles were enormously distended with serum. The corpora striata and thalami were normal. At the base of the brain the arachnoid membrane had a dull pearly appearance, and beneath it a quantity of cloudy flocculent serum was collected; upon the medulla oblongata and cerebellum there was a quantity of milky puriform fluid. The spinal arachnoid membrane in the dorsal and lumbar regions was distended with a flocculent cloudy serum. At two points of the spinal cord, in the cervical and lumbar regions, the pia mater was injected, and the corresponding portion of the cord was softened, so that the knife passed through it as if it were gelatinous. The liver had undergone the fatty degeneration; the mucous coat of the stomach was softened; the small intestines exhibited slight intussusception, which were however easily unravelled by the finger.

It is to be remarked in this case, that there was softening of the brain and spinal cord; a condition not indicated by the symptoms. Dr. Neisser observes, that in this respect it is analogous to one half the cases of hydrocephalus, in which proportion there is a similar softening of the central parts of the brain, without any corresponding symptoms during life. Dr. Neisser supposes that this morbid change occurred a day or two before death, during the period of collapse.

The next case is one of a different character to any of the preceding. It is that of a journeyman weaver, aged 46, admitted into the hospital on July 31st, 1839. Dr. Neisser describes his appearance thus:

“His complexion and appearance is that of health; his expression intelligent and vivacious. Left to himself, there is nothing in his position, demeanour, or expression which would attract the observer's attention. To our inquiry as to what he suffers, and how long he has been ill, he answers very quickly in a loud tone, just as if he had been accustomed to converse with a deaf person; but he hears perfectly well, indeed listens attentively, and what he says is perfectly in-

telligent and connected. He states, that about fourteen days ago, he was chilled while bathing, experienced rigors and heats, and had suffered from headache, and particularly from sleeplessness and restless nights; in other respects he is healthy. The hands are hotter than natural, dry and rough. This, in conjunction with the state of the pulse (96-100), shows a certain amount of general febrile action. Tongue, thorax, and abdomen offer no symptoms." (p. 305.)

Dr. Neisser remarks on this case, that it is a remarkable example of disease in this respect, that only two or three days remain to elapse before the patient will be on the brink of a precipice, and yet he has almost all the appearances of health. The inexperienced practitioner would possibly consider it (recollecting the alleged cause) as a rheumatic fever, and would treat it with diaphoretics. He would hasten the fatal termination, and would be praised for his unremitting attention.

The peculiarities that distinguish this case from the preceding are the exalted sensibility of the sensorium, the quickness of the sensual perceptions, and the hastiness of manner. These, together with the other symptoms, point out a cerebral affection differing considerably from those previously narrated. On a more minute examination it is seen that there is some little injection and swelling of the eyelids; this symptom is significant, it indicates congestion of the head of some duration. There is also dilatation of the pupils, which continues even when the patient is opposite to a strong light. When the patient is raised and made to sit up, he complains of vertigo, and experiences heaviness and confusion of the head, so that he cannot keep himself up. There is, however, no tendency to vomiting. There is no symptom of paralysis in any of the limbs; the breathing is deep and regular.

The symptoms manifestly point to an inflammatory affection of the encephalon. The inflammation is not, however, peracute, having already existed 14 days; it will not terminate in the production of puriform exudation, for if the inflammation were of this character, death would already have ensued. It is therefore a subacute form, accompanied by and ending in a serous exudation. The increased temperature, the frequent and regular pulse, the regular breathing, the general irritability, all indicate the first stage of inflammatory excitement to be predominant. There are other symptoms which show that effusion has commenced, and the stage of oppression entered upon. The headache is replaced by vertigo and confusion of the head, experienced especially when the patient raises the latter. The dilatation of the pupils is a noticeable symptom, although Dr. Neisser thinks that too much pathognomonic importance has been attached to it. In fact, we are not yet certain under what circumstances the pupil contracts or dilates. In cases of this kind the effusion goes on for some time before it is indicated by well-marked symptoms. The brain (already excited) resists the oppressive effect of the effused fluid longer than it would if in its normal condition, so that the symptoms are not in proportion to the effusion. There is, indeed, a sort of interval, a point of counterpoise between the two agencies, which may be detected by the equal balance between the two classes of symptoms.

*Prognosis.* If the disease be left to take its course, and the patient be abandoned to the healing powers of Nature, he dies. The *vis medicatrix* is here a fiction. The pulse is weak, yet the patient talks strongly,

breathes freely, moves actively, has no expression of debility about him. There would appear to be a contradiction here; nevertheless, the pulse is to be taken as a true index that the stage of exhaustion is not far off. This circumstance renders the prognosis unfavorable, and the more unfavorable, because the means necessary for the removal of the disease are such as will still further reduce the powers of the patient.

*Treatment.* The disease being still in the inflammatory stage, no absorption of the effused serum can take place; on the contrary, the quantity is hourly increased. This inflammatory action cannot be removed without the loss of blood, and the question arises as to the mode; that is to say, whether by leeches or venesection. Now the weakness of the pulse forbids general bleeding. In no class of diseases is this remedy so dangerous as in meningeal inflammation. Dr. Neisser explains it physiologically after this manner. Every period of activity continued and prolonged, without the interruption of an antagonistic period of repose, must necessarily lead to exhaustion. Repose is necessary to the repair of the vital mechanism. In meningeal inflammations the patient (that is, his brain) is deprived of this necessary repose, as in the case under notice. For this and other reasons which will occur to the reader, bleeding from the arm is not advisable. The treatment, then, is ten leeches to the temples (five to each) toward evening, to anticipate the evening's exacerbations; ice-cold applications, but no counter-irritants, as these will only add to the existing irritation. Action on the bowels and kidneys should be induced; and for this purpose a tablespoonful of the following mixture is to be taken every two hours. We give the prescription verbatim: "R. Magnes. sulphur, ʒss.; S. in aq. comm. ʒv. Adde—Oxym. squill, ʒj.; succ. liquor, ʒiij., M." The result of this method we will also state in the author's own words:

"Thursday, August 1st. In spite of the leeches and the ice-cold applications, there was febrile exacerbation, an increased heat of skin, and a pulse at 112, but no perceptible result of increased congestion. About ten o'clock the patient became very vivacious, sang a good deal, spoke much and loudly, then slept till one o'clock, when he awoke, and began to sing again. There have been four watery stools, but little urine passed.

"To day the pulse is 92. When addressed the patient answers in good spirits; 'he is perfectly well, ails nothing at all, has no pain, and will soon be well,' &c.; and while he is thus talking, the pulse becomes stronger, hard, more frequent; but so soon as he is quiet the pulse falls again. Nevertheless, he is not worse than yesterday, but somewhat better." (318.)

Dr. Neisser infers from the nocturnal exacerbation and the state of the pulse, that there is yet considerable cerebral action, and orders ten more leeches. The bowels having been well moved, he would now act on the kidneys by means of squills, as in the following formula: "R. Acet. squill. ʒij.; kali carb. q. s. ad perf. saturat. Cui adde Aq. font.; syr. alth. ana, ʒj."

This mixture, we presume, was taken as the previous one.

In the evening no exacerbation took place, the pulse fell to 50, with a good night and several hours of sleep. On the day following, the pulse at 60, the patient lay slumbering, and in breathing puffed his lips and cheeks; a serious symptom, as indicative of incipient paralysis. Cold

effusion was now ordered instead of ice : a large blister to the nape, and an aperient. In the evening of this day vomiting took place twice, and the patient slept a couple of hours only.

On August 3d, the symptoms were more threatening ; the patient puffed his cheeks still in breathing, the pulse only 52, the respiration cephalic ; that dangerous form already noticed. The patient is less vivacious, but the intellect is little affected, and the disturbance being trifling in comparison with the other symptoms of compression, it indicates the effusion to be rather in the periphery than the base of the brain. The douche to be repeated, the blister kept open, the bowels and kidneys to be more stimulated. As the stomach is irritable, the squill must be omitted ; calomel and jalap are, for this cause, inadmissible also ; the following is a better dose : *R.* Tinct. colocynth,  $\mathfrak{D}\text{ij}$  ; Aq. font.  $\mathfrak{Z}\text{ij}$  ; Syr. de spin. cervin.  $\mathfrak{Z}\text{j}$  ; a tablespoonful to be taken every two hours, according to circumstances.

From this period the patient began to recover. On a slight recurrence of the symptoms of compression, the following epispastic was sedulously applied to the head. *R.* Olei sinap. æther. ; gtt. xxx ; Spir. vini rect.  $\mathfrak{Z}\text{j}$ . M. The urgent symptoms gradually subsided, and in eight days the patient was convalescent.

*Hydrocephalus acutus* is the next case. The patient was a lithographic printer, aged 37. His symptoms were headache and darting pains through the head ; weariness, loss of appetite. The complexion is muddy, the expression thoughtful and anxious ; when raised up he could scarcely support his head ; was affected with nausea, vertigo, and little or no fever ; pulse 72, irregular ; skin dry, urine scanty and high-coloured. The patient reeled and tottered as he walked, so much so that he would have fallen unless supported : this is an important symptom, because indicative of pressure on the brain. The symptoms enumerated constitute the prodromi of acute hydrocephalus. The disease is frequent in adults, and not confined to children exclusively, as has often been stated. In the latter, in addition to the symptoms enumerated, there is often tenderness of the abdomen, which, in conjunction with the vomiting, sometimes leads to an erroneous diagnosis. Dr. Neisser thinks the first stage of hydrocephalus consists in general meningeal congestion : in the next stage, inflammation sets in, and the subsequent stages are as in arachnitis. It frequently happens, that the effusion takes place suddenly and with great rapidity.

Dr. Neisser began the treatment in this case with a general bleeding to sixteen ounces ; purgation with compound infusion of senna ; ice to the head, and a warm bath, temperature not higher than  $26^{\circ}$  Reaumur ( $90^{\circ}$  Fahr.), the patient wearing an ice-cap while in it. After the bleeding, &c., the irregularity in the pulse disappeared, with the tendency to vomit. The bowels were freely moved, but the bath had no effect on the skin. Towards evening the patient complained of excessive pain in all his limbs, but striking particularly from the head through the neck and back to the feet, whenever he made the slightest attempt to sit up, or to move the head from one side to the other. There was considerable fever ; the pulse 120, and active delirium, so that it was necessary to restrain the patient. In the morning the pulse fell to 84, the delirium and pain had ceased, and nothing remained but a sense of soreness in the nuchal muscles. There was no tenderness to the touch. Dr. Neisser attributes the apparent ex-



acerbation of the symptoms to the removal of pressure from the brain by the bleeding and other measures, and thus allowing the results of the cerebral irritation (previously masked by the compression) to show themselves. The pain in the back and feet was dependent on irritation from congestion of the spinal meninges. Remedial means: cupping to seven or eight ounces (fifteen cupping glasses), then a bath, and if an exacerbation comes on at eight, a small general bleeding. Internally to take nitre and tartar emetic. The day following, the pain of the neck had gone, the pulse was 72, skin normal, mind composed. The previous night had been restless. The treatment was expectant: on the evening of the third day, symptoms of collapse began to show themselves; there was also a return of the pain in all the limbs, and in the neck and back; and that he might not increase it, he remained motionless. On attempting to raise him, he complained much, but there was no spinal tenderness. The pulse was 72, and little fever. Dr. Neisser now noted the case, as hydrocephalus complicated with arachnitis spinalis. It is worthy remark, that this apparent irregularity in the course and development of that disease, is in fact its normal course. Dr. Neisser now prescribed a bath with three pounds of carbonate of potass, and the following mixture: *R.* Liq. kali acet 3j; Oxym. scill. 3ij. Aq. font. 3iij: a tablespoonful every two hours. The following ointment as a counter-irritant to the spine: *R.* Ung. mercur.—Ung. antim. tart. ana 3ss. M.

From this period the patient began to recover, and was dismissed convalescent on the twelfth day of treatment.

The next, or 6th case, is one of arachnitis with effusion, four weeks after a fall from a stool. The 7th case is one of arachnitis spinalis with serous effusion, inducing paraplegia. We regret that our extended analysis of the preceding histories of disease renders any notice of these impossible. We will therefore pass on to the epilegomena, and cull a few practical or other interesting remarks.

*General notanda of arachnitis.* The first stage of an acute arachnitis is characterised by developed cerebral function in consequence of irritation; when the inflammation is passing into suppuration or exudation of pus, the symptoms of irritation are at first mixed up with symptoms of pressure on the brain: they then gradually disappear, and at last the latter only remain.

A difference in the degree of inflammatory action determines the nature of the inflammatory product: the subacute is characterised by serous exudation, the peracute by purulent. The subacute form its more gradual and imperceptible in its course to a fatal termination; in the peracute the successive stages of the disease rapidly follow each other, and the symptoms of cerebral excitement and the general inflammatory reaction in the system is more strongly developed. The earlier stages of the subacute disease are often insidious, and considerable effusion takes place before there is any very manifest disturbance of cerebral function. Occasionally, indeed, when the inflammatory action is both subacute and circumscribed, symptoms of pressure on the brain are the first indicants of the disease. If, on the other hand, the inflammation be general, but of low intensity, it constitutes another distinct and well-marked variety of arachnitis, known as the premonitory stage of acute hydrocephalus.



The complications of arachnitis are—1. With venous hyperæmia of the brain. This is usually seen in drunkards, and leads to a mixed form, in which there is neither an exclusively serous nor puriform exudation, but a combination of the two. If, however, the inflammation thus complicated be peracute, that dangerous form of disease is developed, known as apoplexia serosa. A second complication of arachnitis is with hemorrhagic apoplexy, or, in other words, an extravasation of blood into the substance, or on the surface of the brain. In this case, the symptoms of irritation are repressed by the pressure on the brain, and are only developed when the cause of that pressure is removed. A third complication of arachnitis is congestion of the brain. In this the symptoms vary according to the degree of congestion, and the case is most intense; they are analogous to those of arachnitis complicated with serous apoplexy. These general views are applicable also to spinal arachnitis.

The physiological inferences deducible from these cases of arachnitis are various. Firstly, it is shown that the symptoms of pressure on the brain are not urgent in proportion to the amount of extravasated blood or effused fluid, but have a relation to the rapidity or suddenness with which it occurs. This is a general principle, applicable to all agents injuriously acting on the brain, as moral impressions, &c. Secondly, there is a greater loss of consciousness when the inflammation and its product are situate at the base of the brain than when on the convex or lateral surfaces. It is thus also with cerebral hemorrhages; hemorrhage at the base induces the most profound and incurable coma; hemorrhage into or near the ventricles induces a less profound and more curable coma; the coma from hemorrhage on the periphery is the lightest and the most manageable of all. This general fact is capable of a satisfactory solution, if we adopt the doctrines of *reflex cerebral action*; and, so far from being opposed to the general opinion, that the mental acts originate in the hemispheres, may be made to harmonize with it. The incident excitor action, which excites the changes in the hemispheres necessary to mental acts, is cut off in its course from the periphery to the centre by the pressure on the conducting fibrils as they pass up from the cord along the base of the brain. A pressure here, indeed, induces a state of the hemispheres analogous to that in sleep; the stimulus from without is stopped. Now, when there is pressure from effusion on the hemispheres, it is only a counter-pressure that is exercised on the base of the brain; and so soon as this is alleviated, the channel of incident excitor impressions is again opened, and pressure is now only exercised on the upper convolutions, a considerable portion of which we know may be injured, and even removed, without any impairment of consciousness whatever. Coma, then, results rather from interruption of function, consequent on an interruption of the flow of the natural stimulus to the brain from the periphery, than on a derangement of structure; it is a result of pressure on the conducting media, rather than on the acting fibres. The same views will of course explain those phenomena (as irregular action of the heart, of the respiratory mechanism, &c.) which are dependent on an obstruction of the cerebral stimulus, in whatever that may consist; while the pressure on the conducting structures at the base interrupts the course of the incident excitor stimulus to the brain, it cuts off the supply of reflex cerebral stimulus to parts below. There is

some consideration due, however, to the fact, that inflammations at the base of the brain are usually the most intense, and the extravasations the greatest, and consequently that the pressure under these circumstances will be greatest. But this circumstance is opposed to the well-known tolerance of injury which the hemispheres exhibit, so that, after all, the inference must be, that the greater the effusion, the greater the pressure on the conducting strands at the base of the brain.

To give the results more in detail. If there be apoplexy at the base of the brain, there is profound and persistent coma, paralysis of all the limbs, paraplegia, and generally a fatal termination. If there be inflammation at the base of the brain, violent spasms, namely, opisthotonos and trismus, followed by deep coma. If there be structural disease at the base, sensorial disturbance, great tendency to vertigo and syncope, titubation, &c.

Paralysis of the extremities may disappear, even when softening of the brain or spinal cord has taken place. A sufficient number of accurately observed cases have now established this principle, which sets aside the more current opinion, that in all cases in which paralysis or spasm is transient, there is *no* structural change in the nervous centres. At the same time, it cannot be denied, that in cases of arachnitis, such lesions of motility are sympathetic, and depend on an irradiation of morbid influence. It is a practical principle that spasm generally indicates a state of irritation in the nervous centres, and paralysis the contrary.

A circumstance worthy consideration is, that while the bowels are torpid in diseases of the brain, the stomach is irritable. Vomiting, as has been seen in the cases detailed, is an usual symptom in arachnitis. It generally accompanies concussion of the brain, and those chronic diseases of that organ which induce pressure. The physiological explanation is, that the vagus is affected at its roots by irradiation from the diseased portion; but then comes the question, why are the gastric branches only affected? Why, in short, is there cerebral vomiting, but not a cerebral cough? There must be some as yet unknown anatomical arrangement of the twigs making up the vagus.

The termination of the sympathetic nerves in the brain is demonstrated by pathology. The heart, intestinal canal, and urinary organs, all participate in the disturbance of the system caused by changes in the central axis. The pulse becomes irregular, the intestines torpid, the sphincter ani and bladder paralysed. The connexion between the brain and sympathetic system leads to remarkable modifications in the symptoms of certain diseases. Thus, in abdominal typhus, when active delirium sets in, the diarrhea, tenderness of the abdomen, &c., subside. If, in the course of a phthisis pulmonalis, cerebral disease occurs, and the patient becomes furiously delirious,—a complication we have occasionally observed, the cough, expectoration, and dyspnea disappear. We have heard a phthisical patient under these circumstances vociferate loudly, talk incessantly, be free from cough and from all rational signs whatever of disease of the lungs, when a post-mortem examination, made within forty-eight hours, has revealed the most extensive tubercular disorganization, so extensive, indeed, that there did not appear six inches square of healthy lung in the whole chest. In the same way we have observed extensive pneumonia to run through its course altogether unobserved and unsuspected by the

practitioner in ordinary, until the stethoscope has demonstrated to him the presence of great and irremediable disease within the thorax. Again, we have observed a peracute gastritis to coexist with arachnitis: the stomach has been found after death so injected as to resemble a piece of red velvet, and yet during life there was not one symptom observed that could be traced to a morbid condition of that viscus.

Some of these complications may accelerate the fatal termination in arachnitis. The influence radiated on the pulmonary branches of the vagus may be so great as just to arrest irritation of the incident excitor nerves, and to prevent fits of coughing, but may not paralyse the organic nerves, and stop secretion. Then the mucus accumulates in the bronchi, and the patient dies at last suffocated. This state of things may not exist during the waking condition, but may, and indeed often does, supervene with sleep. In these cases sleep is dangerous, and, if the patient be not roused from it from time to time, will terminate in death.

Dr. Neisser insists that encephalitis never occurs except as the result of mechanical injuries, or as a complication of arachnitis, when it is confined to the portion of brain adjoining the inflamed membrane. The same remark applies to myelitis. But, under the term arachnitis, he means to describe a disease which has its true and primary seat in the pia mater, and which implicates the arachnoid membrane consecutively. The latter he looks upon as pathologically the cellular tissue of the pia mater. These membranes conjoined form the substratum of all idiopathic inflammation within the encephalon, while inflammation of the dura mater and of the substance of the brain follows only on mechanical injuries. We state these opinions doubtfully, and rather with the intent that our readers may know what is current among German pathologists, than for their adoption and approval. If the new views could be entertained which describe the arachnoid as a membranous plexus of organic nerves, the pathology of the serous inflammations we have discussed must have a complete and minute reviewal. Possibly, indeed, there may be a clue obtained in this way to those inappreciable changes in the brain which give rise to delirium and other sensorial phenomena.

We have now to state in conclusion, that, after a careful perusal and analysis of Dr. Neisser's volume, we find it to be a valuable contribution to the natural history of disease. We cannot, indeed, recommend a more efficient plan than that which he adopts for the elucidation of obscure points in, and the general advancement of, pathology. The estimate of the symptoms, and their relations from the results of experience and the data of physiological science must be peculiarly rich in results. There is one great defect in Dr. Neisser's method, and that is, no attention is given to the etiological relations of the diseases he describes. We find no chemical analysis of the urine, no investigation into the condition of the blood, no consideration of the causal antecedents. The whole treatment is unmitigated empiricism.

## ART. IX.

*On Disorders of the Cerebral Circulation, and on the Connexion between Affections of the Brain and Diseases of the Heart.* By GEORGE BURROWS, M. D., late Fellow of Caius College, Cambridge, Fellow of the Royal College of Physicians, London, Physician and Lecturer on the Principles and Practice of Medicine at St. Bartholomew's Hospital.—London, 1846. 8vo, pp. 220.

THE sensations experienced by all persons on placing the head in a dependent position, or on otherwise favoring congestion of the cerebral vascular system, are so determinate, that it has been almost universally received as a fixed principle, that the amount of blood within the cranium may be increased. And yet it cannot be denied, that an opposite opinion has been broached and advocated, for it seems to be a general law in metaphysics, that every principle, whether true or false, shall have its antagonistic principle. In the present work, Dr. Burrows aims at a twofold object: he proposes to show, firstly, that this opposite opinion is erroneous; and, secondly, that changes in the circulation within the cranium have a much wider pathological scope than they are generally supposed to possess.

The doctrine that the quantity of blood circulating within the cranium is, under all circumstances, a fixed quantity, is an offset from the iatro-mathematical school of Borelli, and was promulgated by Dr. A. Monro at Edinburgh during the latter half of the last century. It was founded upon the incompressibility of the substance of the brain, and upon the spheroidal form of the skull. The idea might probably be traced from Monro to Boerhaave, or Pitcairn, or to some of their disciples. When, however, the last of the iatro-mathematical school had disappeared, we find Dr. Kellie taking up and defending this one of its doctrines which Monro had promulgated, and enlisting the late Dr. Abercrombie amongst his disciples. The name of Dr. Abercrombie was a tower of strength, and gave renewed vigor to the iatro-mathematical views. Dr. Clutterbuck adopted them in his article on Cerebral Apoplexy, in the 'Cyclopædia of Practical Medicine,' and even Dr. Watson, in his very recently delivered Lectures, maintained the invariableness of the quantity of blood in the brain, adducing the argument which Dr. Pitcairn, or even Borelli, would probably have advanced. "This depends," remarks Dr. Watson, "upon the mechanical structure of the cranium, and is capable of explanation upon the known principle of hydraulics." It is but justice to Dr. Watson, however, to observe, that in the second edition of his Lectures, he acknowledges a change of opinion on this point in consequence of Dr. Burrows's demonstration to the contrary.

The above history of this heresy, sketchy and slight as it is, shows that erroneous ideas reappear like weeds, and are as apt to perpetuate themselves in the soil of science. Dr. Burrows was impressed with the idea, that the opinions promulgated by Dr. Kellie and his successors were erroneous, and that they obscured the pathology of a most important organ; he therefore repeated the experiments of Dr. Kellie, and in the present work we have the results of his inquiries. These results, as well

as the pathological views of Dr. Burrows, we now propose to place before our readers.

Dr. Kellie inferred from his experiments—1, that a state of bloodlessness is not discovered in the brains of animals which have died by hemorrhage, but, on the contrary, very commonly a state of venous cerebral congestion; 2, that the quantity of blood in the cerebral vessels is not affected by gravitation or posture of the head; 3, that congestion of the cerebral vessels is not found in those instances where it might be most expected, as in persons who die by hanging, strangulation, suffocation, &c.; 4, that, if there be repletion or depletion of one set of vessels (arteries or veins) in the cranium, there will be an opposite condition of the other set of vessels.

In examining these experiments analytically, Dr. Burrows clearly shows, that they do not bear out Dr. Kellie in the inferences he deduced from them, and that, in fact, Dr. Kellie did himself at first draw the inference that the brain of animals might be depleted by bleeding, and their vessels drained of a very sensible proportion of the blood contained in them. It was in a subsequent communication to the Medico-Chirurgical Society of Edinburgh that he contradicted himself. It appears that his experiments stand alone, and have not been repeated: Dr. Burrows's researches are therefore the more interesting. We subjoin the first *in extenso*:

"On the 11th of January, 1843, I killed two well-grown rabbits. The one (A, plate 1), by opening the jugular vein and carotid artery on one side of the throat; the other (B, plate 2) was strangled. Each animal died violently convulsed. A ligature was drawn tightly round the throat of the rabbit (A) immediately it expired, to prevent any further escape of blood from the vessels of the head. The rabbits were allowed to remain twenty-four hours on a table, resting on their sides.

"While the blood was flowing from the rabbit (A), the conjunctiva was observed to become pallid and the eyeballs to shrink within the sockets. Upon the examination of the head of this rabbit, the integuments and muscles appeared blanched and exsanguined. Upon removing the upper portions of the cranium, the membranes of the brain were found pallid, and scarcely the trace of a blood-vessel was to be detected on the surface of the brain. The longitudinal and lateral sinuses were nearly empty of blood, and their course was not denoted by any colour of blood. Upon making sections of the brain, the interior appeared equally exsanguined.

"Soon after, the cord was drawn tight round the throat of the rabbit (B), the conjunctival vessels became congested, the eyeballs turgid, prominent and even projecting beyond the margin of their sockets. The integuments and muscles of the head were found full of blood. Upon opening the cranium, the superficial vessels of the membranes, as well as the sinuses, were full of dark liquid blood. The whole substance of this brain and its membranes appeared of a dark reddish hue, as if stained by extravasated blood. The contrast between the two brains in point of vascularity, both on the surface and the interior, was most striking. In the one scarcely the trace of a blood-vessel was to be seen; in the other every vessel was turgid with blood." (p. 13.)

Dr. Burrows has appended coloured drawings of the post-mortem appearances in the brains of the two rabbits, and they are certainly in striking contrast. He observes, however, that the brains of sheep slaughtered by butchers are much less depleted than the brains of rabbits killed in the mode he describes. This difference is owing, he thinks, to a difference in

the mode of death. Sheep, he observes, die partly from hemorrhage, and partly from division of the pneumogastric nerves, and of the cervical position of the cord.

Another contradiction to the iatro-mathematical doctrine regarding the cerebral circulation is contained in a tabular synopsis of the principal pathological appearances met with in 72 insane patients of Bethlem Hospital, examined by Mr. Lawrence, and published by Dr. Webster in the twenty-first volume of the '*Medico-Chirurgical Transactions*.' In 53 of the 72 cases examined, the vessels of the brain were found congested; in one only were they exsanguine—"unusually empty"—and in this case death was immediately consequent upon the hemorrhage induced by the rupture of a femoral aneurism. Dr. Burrows, therefore, observes:

"Hence it is not a fallacy, as some suppose, to assert that bleeding diminishes the actual quantity of blood in the cerebral vessels. By abstraction of blood we not only diminish the momentum of blood in the cerebral arteries, and the quantity supplied to the brain in a given time, but we actually diminish the quantity of blood in those vessels. Whether the vacated space is replaced by serum, or resiliency of the cerebral substance under diminished pressure, is another question, into which I do not now enter." (p. 15.)

Can posture influence the cerebral circulation? We suspect each of our readers need only stoop to the ground to answer the question experimentally; yet the iatro-mathematical theory prevented the due reception of the results of daily experience, and apparently led Dr. Kellie into a wrong appreciation of some experiments he instituted on rabbits. As Dr. Burrows simply repeated these experiments of Dr. Kellie, we will subjoin his account:

"On the 28th of December, 1842, two full-grown rabbits were killed by prussic acid, and while their hearts were still pulsating, the one (C, plate 3) was suspended by the ears, the other (D, plate 4) by the hind legs. They were left suspended for twenty-four hours, and, before they were taken down for examination, a tight ligature was placed round the throat of each rabbit, to prevent, as effectually as was possible, any further flow of blood to or from the head, after they were removed from their respective positions.

"In the rabbit (C) the whole of the external parts of the head, the ears, the eyeballs, &c., were pallid and flaccid; the muscles of the scalp and bones of the cranium were also remarkably exsanguined. Upon opening the cranium the membranes and substance of the brain were pallid, the sinuses and other vessels were exsanguined; anæmic beyond my expectation. #

"In the rabbit (D) the external parts of the head, ears, eyeballs, &c. were turgid, livid, and congested; the muscles and bones of the cranium were of a dark hue, and gorged with blood, which at some parts appeared extravasated. Upon opening the cranium, the membranes and vessels were dark, and turgid with liquid blood; the superficial veins were prominent, the longitudinal and lateral sinuses were gorged with dark blood, and there was staining of the tissues, if not extravasation of blood into the membranes. The substance of the brain was uniformly dark, and congested to a remarkable extent.

"Dr. Kellie asserts, but I think his experiments do not support him, that the contrast in the appearances within the heads of the two animals was but trifling. In my analogous experiments the contrast was most striking. In the one was to be seen a most complete state of anæmia of the internal as well as external parts of the cranium; in the other a most intense hyperæmia, or congestion, of the same parts; and these opposite conditions of the vascularity of the brain induced solely by posture, and the consequent gravitation of the blood.



"If the cranium were the perfect sphere, as taught by Monro, and as subsequently entertained by Abercrombie, and other distinguished writers on the pathology of the brain, these effects on its circulation (which I have now described) ought not to have resulted from the force of gravity on the blood in the cerebral vessels." (pp. 17-18.)

Four coloured plates illustrate this account; and certainly the contrast is as striking as in the experiments described previously.

These researches of Dr. Burrows ought, we think, to be considered conclusive on the question, so far as experiments of the kind can be considered conclusive.

It appears to us, however, that the original theory has had much more importance attached to it than it deserves. In the first place the cranial contents cannot be considered as free from atmospheric pressure any more than the heart, liver, &c. The cranium is *not* a perfect sphere, while the sinuses of the dura mater, situate within it, communicate with the exterior of the skull by several large veins. Then, if it be granted that the skull is a perfect sphere, and the amount of fluids must at all times be equal, the necessary equalization is attained by the pressure of the columns of blood forced in by the heart, which columns, dividing and sub-dividing until, at length, lost in the capillaries, exercise in the meanwhile a compressing power upon the contractile tunics of the vessels, and when this is overcome, upon the surrounding nervous tissue. This complicated structure of vessels and nervous fibrils cannot, we think, be properly compared with a homogeneous fluid, nor can the laws of hydraulics be applied to it. In short, we think that the cranium cannot be rightly likened to a beer-barrel, nor its contents to beer; nor do we think it necessarily follows that, because the brain is incompressible, its functions may not be destroyed by compression; indeed, we believe this is acknowledged by the spheroidal theorists. The pressure, they allow, may be so great as to rupture the vessels, whether that pressure be induced by retardation of the blood in the venous circulation, or by increased energy of the arterial circulation. That the quantity of *fluids* within the cranium generally cannot be sensibly increased, is probable nevertheless; but this is not, in truth, the prominent and fundamental proposition of the iatro-mathematicians. It was, really, that the quantity of *blood* could not be sensibly increased; the other proposition was only secondary to this. The one had a great therapeutic importance, because it bore on the propriety of bleeding and other points in the treatment of apoplexy, and was, therefore, adopted exclusively by those writers whose opinions Dr. Burrows combats; the other, having no practical value, was not noticed. But, although we believe the iatro-mathematical theory never received the full consent of the profession, or was generally acted upon in the treatment of congestive affections of the head, yet it threw an air of doubt and obscurity about an important point in physiology, which has materially tended to render the progress of cerebral pathology slow and imperfect. By the removal of a great error, Dr. Burrows has done as good service to the medical commonwealth as if he had discovered a great truth. We here, however, subjoin an important caution given by him:

"It may now be affirmed that the encephalon is not exempt from this law in physics—the gravitation of the fluids to the lowest parts of the corpse. The dis-

covery of the operation of this force on the blood within the cranium after death suggests a precaution very essential to be followed, when it is desired to ascertain the precise amount of congestion of the cerebral vessels at the time of death. In such cases a ligature should be placed round the throat of the corpse, and drawn sufficiently tight to compress the cervical vessels, and arrest all flow of blood through them. This precaution will be most required in the examination of bodies where, from the kind of death, the blood may be suspected to remain fluid in the heart and great blood-vessels. The depending or elevated position of the head, during the examination of the body, will not then induce deceptive appearances, which mislead us in our conclusions as to the previous amount of congestion in the cerebral vessels." (p. 20.)

This caution is rendered the more necessary, because the non-congested state of the brain which has been observed in persons who have been hung, or strangled, has been adduced (vide Dr. Kellie's paper) in proof of the iatro-mathematical proposition. But Dr. Burrows shows that the brain may or may not be congested according to circumstances, and these circumstances consist in the mode in which the strangulation is effected, and in the anatomy of the cerebral vessels. He thus accounts for the occasional absence of cerebral congestion in those who have suffered death by hanging :

"When criminals are hung by the executioner, the knot of the rope is usually adjusted on one side of the neck; and it is found, after death, beneath the ear, resting on the mastoid process. It has been often observed, in the dissection of such criminals, that the cheek and integuments on this same side of the head are not near so livid and congested as on the other side. The pressure of the rope has not completely obstructed the return of blood through the external jugular vein on the one side, although it has effectually stopped the current on the other. In such cases it is probable that the deep-seated internal jugular vein on the one side has only been partially compressed, and has permitted to a certain extent the return of blood from the internal parts of the cranium.

"But there is another still more efficient cause of this occasional absence of congestion of the cerebral vessels after death by hanging: it is the subsidence of the *fluid* blood after death, while the body is yet suspended, through the cervical vessels which are not completely obliterated by the pressure of the cord. And it should be recollected that there are some channels which are scarcely, if at all, affected by the compression of the rope. These other channels are the vertebral sinuses and spinal plexus of veins, so ably delineated by M. Breschet." (pp. 25-6.)

The sinuses of the cranium may also be drained otherwise than through the vertebral sinuses. In examining the bodies of those who have died by strangulation, the great vessels of the neck are usually cut across to get at the thoracic viscera; and then, when the head is elevated to open the skull, the blood gravitates, and flows from the cut ends, and the blood-vessels (previously congested) are rendered comparatively empty. Dr. Burrows further observes, in proof of this explanation, that in other instances where life has been destroyed by obstruction of the respiration, congestion is usually present.

The cerebral arteries and veins were supposed by Dr. Kellie and his disciples to stand in such relation to each other that, although the quantity of blood in the one might be diminished, it would be proportionately increased in the other. In states of anæmia, for example, a diminished quantity of blood might pass into the cerebral arteries, but a proportionate increase would take place in the cerebral veins and sinuses; and so the

whole volume of blood within the skull would remain the same. Now there is no proof that such a relative condition of the two classes of vessels exists. Dr. Burrows very ingeniously shows, too, that the extravascular serum contained in the encephalon plays an important part in equalizing the pressure and the contents of the cranium. This point is, however, discussed in the next section. We need only observe, further, that to strengthen his case Dr. Burrows adduces the recorded cases of pathologists to prove that the brain is liable to be congested, loaded with blood, &c. ; so that the quantity of the latter is a variable quantity.

*On vascular pressure within the cranium.* After objecting to some theoretical views regarding the incompressibility of the brain, Dr. Burrows proceeds to observe that the force which is impressed on the cerebral substance, through the momentum of the blood in the cranial vessels, is derived partly from the left ventricle of the heart, and partly from the reflux of the venous blood during expiration. When the brain is exposed in the living animal, the dura mater is found to rise and fall a little alternately with every systole and diastole ; and experiments show that this alternate rise and fall is really (as it appears to be) dependent upon cardiac action. As to the venous reflux, we quote Dr. Burrows's lucid statement :

"If the dura mater be exposed to view, and observed during the period of expiration, when the free return of venous blood from the brain is impeded, and a larger quantity of arterial blood is distributed with increased force towards that and other organs, the surface of the membrane is seen to rise ; the brain itself swells and becomes turgid, but again subsides with the succeeding inspiration. All physiologists who have considered this subject agree in regarding these latter respiratory movements of the brain as partly, if not wholly, attributable to the reflux of the blood in the veins during expiration. Ecker, indeed, attributes these movements of the brain in great part to the ascent of the cerebro-spinal fluid during expiration. The last-mentioned writer has detailed numerous experiments, which show that if ligatures be placed upon the two carotid arteries of an animal, these cerebral movements accompanying respiration are not suspended ; but if the external jugular veins in dogs (and these are the principal cerebral veins in these animals) be tied, these respiratory movements are much weakened, but not altogether destroyed ; and if the cervical vessels, both arterial and venous, be divided, all movements of the vein are immediately lessened ; and when the loss of blood becomes excessive, they cease altogether, and there follows a remarkable shrinking of the organ. But the effects of this reflux of the blood in the veins upon the cerebral substance are still more manifest after those accidents where, with loss of a portion of the bones of the cranium, there is also a laceration of the dura mater. In such cases the distending forces of the vessels acting on the cerebral substance are so strongly exhibited, that not only is the alternate rising and sinking of the exposed surface observed to correspond with each expiration and inspiration, but portions of the substance of the brain are actually protruded through the opening in the dura mater and bones of the cranium. Hernia of the convolutions of the brain is thus effected by a force from within, just as hernia of the convolutions of the intestines is produced by analogous forces, when the walls of the abdomen are accidentally perforated." (pp. 46-7.)

Such being the causes of cerebral pressure in continual action, it remains to ascertain what provisions have been made to equalise this pressure, so liable to vary greatly in amount, and acting on so delicate an organization as the brain, confined in its unyielding bony case. This is to be found in the cephalo-rachidian fluid. We have already, in our

Fourteenth Volume, given a *précis* of Majendie's views respecting this fluid. From experiments instituted by Ecker, we learn that the spinal theca rises and falls like the brain, concurrently with expiration and inspiration. If the theca be carefully divided, so as to leave the arachnoid membrane intact, the latter appears at the opening like a fine bladder containing fluid. This fluid is the cephalo-rachidian, and is manifestly subject to alternate increase and diminution of pressure, concurrently with the respiratory acts. It is engaged in a perpetual alternation of ascent and descent, and presses against the solid parietes with a force equal to the internal momentum. Dr. Burrows thus states his views of its functions :

"This extra-vascular serum appears to me to be supplemental to the other contents of the cranium; it is removeable by pressure or absorption; at one time giving place to an increased quantity of blood in the cranium, at another, making up for a deficiency of blood in the vessels of the head. This extra-vascular serum not merely acts as supplemental to the varying quantity of blood, but also to the variable quantity of nervous matter in the brain. Its quantity is in the inverse proportion to the quantity of this nervous matter. Thus, in hypertrophy of the brain, there is a most remarkable deficiency of serum within the cranium; the brain, its ventricles and membranes, are so devoid of this fluid, that they are almost dry; on the contrary, in atrophy of the organ, the ventricles and membranes are distended with fluid.

"This extra-vascular fluid may probably perform another office: perhaps through this cerebro-spinal fluid a more equable pressure is diffused over the whole mass of the brain and cord; and, for the reception of this regulation of pressure, may be the contrivance of those cavities called ventricles, which dip into the central parts of the brain. The sac of the arachnoid may subserve the same purpose." (p. 56.)

This theory is plausible, and will certainly serve to explain some obscure points in cerebral pathology, but we apprehend its plausibility will prevent a ready adoption of it by pathologists, without corroboration by further experiments and observations. Essentially, it considers the spinal column and cranium as one cavity, and consequently reference must be had for the future to the functional condition of the spinal cord in all cases of cerebral compression. The amount of the cephalo-rachidian fluid must exercise, if this theory be correct, a most important influence on the functions of the cerebro-spinal axis. The means and agencies by which that amount may be diminished and increased, will also have an important bearing on the pathology and therapeutics of the brain and spinal cord. Dr. Burrows details a case of chronic inflammation of the investing membranes of the brain, with copious serous effusion, and which latter, in his opinion, by varying in site and amount, was the cause of a peculiar variation of the symptoms. The patient presented "a remarkable alternation between excitement and calm, between returning power and paralysis. On one day she had no command over the sphincters, and the lower limbs appeared almost powerless and insensible, but exhibiting slight involuntary movements when the feet were pricked. On other days she called for the bed-pan, and the legs were immediately drawn away with an expression of pain upon a slight amount of irritation." We think this case extremely inconclusive, since other causes might have been in operation, as, for example, recurring alternations of vascular action and atony.

It is certain, indeed, that the pathological influence of this fluid must always be difficult to appreciate, until its physiological relations and behaviour are better understood. There are some anomalous cases of paraplegia which may ultimately be elucidated by this theory; we allude to those examples in which there is no perceptible disease, either of the investments or structure of the spinal cord, and in which, upon some vivid emotion occurring, the paraplegia temporarily disappears. Now it *may* so happen (we put the case confessedly as a remote hypothesis), that in such examples there is a deficiency of pressure upon the dorso-spinal cord, because of a diminished quantity of the cerebro-spinal fluid, and that when in consequence of an emotion, the brain receives a greater influx of blood, the pressure then becomes sufficient for normal action;—that, in fact, the dorso-lumbar cord in such cases is permanently in a condition like that of the brain in syncope. The following are Dr. Burrows's general deductions:

“The foregoing inquiry will elucidate many interesting phenomena observed in states of vascular congestion or depletion within the cranium. When arterial or venous congestion of the brain is suddenly induced, the first effect will be an increased pressure on all the contents of the cranium; this pressure will cause the expulsion of a portion of this extra-vascular serum into the spinal canal. On the other hand, when abstraction of blood from the cranium is effected, there is a tendency to shrinking of the encephalon, but the vacated space is immediately occupied by a certain quantity of this serum. But when the power of the heart is inordinately increased for any length of time by stimulants, general plethora, or hypertrophy of the left ventricle, a train of symptoms is remarked similar to those produced by varying degrees of mathematical pressure artificially exerted on the brain.

“These effects of increased determination of blood to the brain may be witnessed even where there is no structural lesion of that organ, although the effects are probably partly obviated by the anatomical peculiarities just described; but they will be most strikingly displayed where there is pre-existing structural disease in the cranium.

“When obstruction to the return of blood from the brain takes place, so that the blood becomes almost stagnant in the sinuses, that part of the force of the left ventricle (which, in the normal state of the cerebral circulation, is expended partly in propelling the blood onward through the capillaries towards the right auricle, and partly in distending the vessels through the cranium) is, under such circumstances, expended upon the interior surface of the cerebral blood-vessels. This pressure is partly sustained by the resistance of the vascular tissues, and the remainder is borne by the surrounding cerebral substance. Whatever this force may be, it becomes a source of increased pressure upon the cerebral substance; and the more so, according to the pre-existing morbid states of the encephalon and its vessels.

“When the circulation is thus excited or obstructed, an obvious state of congestion of the integuments of the head and face is produced, and from the experiments I have detailed, it may, I think, be inferred that a simultaneous congestion of the internal vessels of the cranium is formed.

“In previously healthy conditions of the cranium, when it contains nothing but the brain and normal quantity of serous fluid, the cerebral substance may readily accommodate itself to a temporary increase of blood in its vessels (arterial or venous, or in both), and to the consequent pressure, by expulsion of a certain amount of serum; but when the cranium contains abnormal and unremoveable substances, then the brain cannot bear these accessions of vascular fulness and consequent pressure.



"In those pathological states of the encephalon where there is an increase in the quantity of solid matter within the cranium, and a diminution of the quantity of extra-vascular serum, as in hypertrophy of the brain, tumours and cysts in that organ, and in large extravasations of blood on the surface, every cause which is capable of exciting the heart's action produces a notably increased disturbance of the functions of the brain. The variable character of the symptoms of cerebral disturbance in these permanent lesions within the cranium, are thus probably accounted for by the variable vascular pressure. Andral has offered a nearly similar explanation of the occasional recurrence and intermittent character of cerebral symptoms, although their supposed cause, organic disease in the cranium, is permanent. It seems to me probable that many permanent structural lesions within the cranium do not affect the functions of the brain by pressure, except when there is some cause in operation capable of inducing vascular congestion, or when the lesion is of a mechanical nature, or is gradually increasing." (pp. 58-60.)

Dr. Burrows thinks it necessary to combat some views lately published, in which syncope is represented as being allied to apoplexy, and dependent on venous congestion. He maintains that it results from insufficient vascular pressure, and not from the inadequate supply of blood to the brain. In syncope arising from strong moral emotion, the quantity and quality of the blood, he argues, are unaltered previously to the attack, and the suspension of the cerebral functions arises solely from the diminished energy of the heart. We apprehend that these views of Dr. Burrows will not meet with more general acceptance than those he combats. It may be justly argued, we think, that if the heart be so enfeebled as not to be able to propel the column of blood with the force necessary to cerebral action, it will propel a column of diminished size, or in other words, in syncope, the heart's action being enfeebled, the contractility of the arteries will react upon the column of blood, and by rendering it smaller, diminish the supply of blood to the brain, as is generally supposed. Now the importance of a due supply of blood is, we think, sufficiently manifest from the great vascularity of the gray substance. Dr. Burrows argues, that in cases of hypertrophy of the brain, the substance of the organ is extremely anæmic, for the hypertrophy, by filling the skull, completely prevents the blood entering the capillaries. Dr. Burrows cannot wish his readers to understand that the *quality* of the blood has not quite as much influence on the functions of the brain as the quantity. In cases of anæmia from hemorrhage or defective sanguification, it does not appear probable that the amount of the fluid circulating through the vascular system is diminished, nor is it improbable that the increased rapidity of the heart's action in a great degree counterbalances the diminished force of the systole. Besides, we have examples of cardiac affection, unaccompanied by an anæmic condition of the blood (meaning by anæmic, a deficiency of the red globules), in which the systole is slow and feeble, and yet there are few of the characteristic symptoms of anæmia. There can be no doubt that the horizontal position increases the vascular pressure on the brain, when the heart's action is enfeebled, but it also increases the quantity of blood passed through the brain in a given time. We would ask also, if we grant (as we willingly do) that a certain amount of vascular pressure is favorable to the functional activity of the brain, how is it that this acts? Is it not by favoring the circulation through the numerous capillaries, and thus increasing the assimilating activity of the cerebral



substance? And, if so, may not the proper condition of the matter to be assimilated (the blood) have as important an influence on the organ as the proper condition of the vessels that convey it, or the organs that assimilate it? We think when Dr. Burrows inquires how it is that diminished vascular pressure interrupts cerebral function, he will be induced to modify his views in some degree.

*Cerebral disorders from ligature of the carotids.* It has long been known that compression or ligature of the carotid arteries has an important influence upon the condition of the brain. A female was operated upon by Mr. Key for aneurism of the innominate.

"In about an hour and a half after the operation she appeared asleep, the respiration being natural, with the exception of a snore. This noise gradually became fainter, and she expired in about two hours more; just four hours after the operation. Upon examining the body it was found that the opening of the other (left) carotid from the arch of the aorta was nearly closed; it was scarcely one tenth its natural size. The substance of the brain was healthy; its vessels sound, and containing the ordinary quantity of blood. There was a little serous effusion between the membranes."

Now, Dr. Burrows, (a little influenced by his *idolum specûs*, we think) observes that "there appears nothing to account for this woman's death but the sudden diminution of the momentum of the blood in the cerebral arteries, occasioned by the closure of the only pervious carotid." But surely a defective *supply* of blood to the brain, in this case, so much below the ordinary quantity sent to it, may be allowed to go for something, and that the want of blood, as well as the want of pressure, was a cause of death. In cases in which one carotid has been tied, the cerebral disorder has been characterized by hemiplegia of the side opposite to the obstructed artery, with disorganization of the hemisphere of the same side. These results may, in part at least, be very fairly attributed to defective nutrition of the hemisphere implicated; but Dr. Burrows, faithful to his theory, says that the cause of the commencing hemiplegia is the pressure induced on it by the vascular distension of the opposite hemisphere. There is no proof, however, that one hemisphere can thus compress the other; but, on the contrary, it is probable that the falx cerebri will effectually prevent such a result, just as the tentorium protects the cerebellum from compression by the cerebrum.

Considering that compression and ligature of the carotids have been found useful in epilepsy, mania, and other chief-diseases, Dr. Burrows thinks that the ligature may be resorted to in hopeless cases of the kind alluded to.

"Although the ligature of the common carotid is attended with risk to life in some cases (perhaps in the proportion of one death in four operations), still experience proves that where proper precautions have been taken the operation is not so dangerous as many suppose. Therefore, in violent and hopeless cases of epilepsy and some kindred maladies, which are characterised by extreme cerebral congestion, it appears to me that, other remedies failing, this operation may be fairly resorted to. I am aware of the responsibility of advocating a remedy attended with risk to life; but are not all our best remedies most violent poisons in the hands of the unskilful? But this truth does not forbid their use by the more expert. So may this powerful method of influencing the cerebral circulation be justifiable in aggravated cases of the class referred to, and where the pre-

cept of Celsus, '*salus est anceps remedium experiri quàm nullum*,' may be fairly put into practice." (pp. 78-9.)

We would only observe that there is another good precept to be remembered, and that is "Saltem non nocere."

*Causes of coma.* The progress of neurology, during late years, is amply marked by the clearer comprehension of the phenomena of cerebral diseases. What is the condition of the brain that produces coma? was a question not easily answered twenty years ago; and the answer involved the hypothesis of an obstruction to the flow of the "nervous fluid," or a confession of ignorance. The difficulty of explaining this condition was increased by the circumstance that often no appreciable change in the constitution, or vascular system of the brain, could be detected after death from profound apoplexy. Dr. Abercrombie has particularly noticed examples in which not even congestion of the brain was observed. Dr. Burrows's explanation is, that the active depletion practised before death, in two of the cases mentioned, had emptied the cerebral vessels, and that the apoplexy was immediately dependent on sudden vascular pressure. Now although bloodletting may relieve this, the respiration has already become involuntary, slow, and stertorous, and the blood decarbonised; so that the coma is kept up by the circulation of the latter through the brain, and the patient actually dies of slow suffocation. It is probable that in deaths from acute laryngitis, when the respiration has been restored by tracheotomy, and in deaths after resuscitation from hanging, drowning, and suffocation from irrespirable gases, there is the same fatal condition of the brain.

Is the coma in the so-called serous apoplexy dependent on pressure from serous effusion, or on vascular pressure from congestion? Dr. Burrows argues in favour of the latter. He thinks the diagnostic symptoms between serous and sanguineous apoplexy are more fanciful than real; that, in fact, the serous effusion is itself a sequela of vascular congestion, and that the serous apoplexy is no other than simple apoplexy, that is to say, cerebral congestion terminating in effusion.

From our review of Dr. Neisser's work (see the preceding article), it will, we think, be clearly manifest that serous effusion is one of the modes in which the subacute form of arachnitis terminates. In such a form of arachnitis there is doubtless vascular congestion; but we cannot conclude with Dr. Burrows that that congestion is the cause of the coma and other signs of a fatal termination. We have no evidence of "vascular pressure" during the earlier stages of the disease, when the vascular action is the greatest. On the contrary, there is an increased activity of the cerebral functions. It is only when the serum accumulates within the cranium, and especially at the base of the brain, that the comatose symptoms come on (vide p. 405); for it must be remembered that the seat of the effusion has an important influence on the development of symptoms of pressure.

Dr. Burrows applies similar views to extravasation, and advances that the effused blood is not the cause of the apoplectic coma. We will quote his opinions on this point:

"I am of opinion that apoplectic coma is rarely dependent upon the extravasation of blood, although the concomitant paralysis undoubtedly is. Upon the

examination of the brains of apoplectic patients we sometimes find large extravasations of blood, which from peculiar appearances in the clot, we feel assured have existed there for many days, or even weeks, and yet during the greater part of that period there has been no coma. Upon other occasions we discover small extravasations of blood within the brain, which, from their appearance, we can decide have only been effused a short time prior to death; and, nevertheless, there has been a well-marked coma in these cases. Hence, if pressure be regarded as the physical cause of apoplectic coma, and that pressure is supposed to be occasioned by the extravasated blood, then we must account for the paradox of a small extravasation producing a coma which terminates fatally, and a large effusion of blood having no such effect." (p. 92.)

It appears to us that Dr. Burrows is wanting in his usual lucidity and precision here. There appears somewhat of special pleading in the argument. The observation we have just made respecting the importance of referring to the exact locality of the extra-vascular causes of pressure applies with peculiar force to extravasation of blood. In the two cases related by Dr. Burrows in proof of his views, the influence of a hemorrhagic site is very clearly shown. We have already observed in the article referred to that the effects of pressure from extra-vascular causes are manifest in proportion as the locality of the compressing cause is near the base of the brain. Now in the case of Mrs. G. (detailed by Dr. Burrows) the first extravasation of blood was "a large clot, probably an ounce, effused into the anterior half of each anterior lobe;" and the symptoms when this occurred are thus described:

"In the forenoon of Sept. 12th she had complained of vertigo and drowsiness while engaged in her domestic affairs. She did not fall or lose her consciousness, but contrived to get up stairs to her bed-room. She was assisted to her bed, where she remained perfectly sensible for some hours, but stupor increased; so that, towards evening, she could not be roused at all. Her family now became alarmed, and sent for medical assistance. She was found lying on her back, perfectly insensible; her face pale, and expression placid; hands cold; breathing natural; pulse 120, small, and feeble; pupils dilated, and insensible to the influence of light."

On the following day she was restless and delirious; so much so, indeed, that she could hardly be kept in bed—a state of things quite unlike apoplectic coma. The second extravasation, occurring about forty-eight hours before death, was beneath the cerebral arachnoid over the left hemisphere, and at the base of the brain; all the ventricles, although not distended, contained clots of soft and uniformly black blood. There was also a considerable quantity of blood effused into the tissue of the spinal pia mater, especially over the medulla portion. This blood, Dr. Burrows thinks, was probably derived from the ventricles of the brain, it having followed the course of the cephalo-rachidian fluid. The symptoms accompanying this extravasation are thus described: "Her countenance was haggard; her eyes fixed and vacant; she was roused with difficulty; her articulation imperfect, and she answered incoherently. She gradually became comatose, and sank on the third day after her admission." Here we have a state of things very different to the preceding, with a corresponding difference in the seat of the extravasation. Of course this solitary case would prove nothing; but as it is corroborated by numerous similar cases, we must fairly presume that the difference in the seat of the effusion was connected with the difference in the symptoms.

The next case is equally instructive. A female had a series of fits. The last and fatal fit was profoundly apoplectic, with coma, stertor, and death in twelve hours. On examining the corpse there was a clot of recently effused, dark-coloured blood, extending nearly across the tuber annulare. The penultimate fit (six weeks previously to the fatal attack) was complicated with hemiplegia of the right (?) side. The corresponding post-mortem appearance was a diffused yellow staining in the substance of the right hemisphere, becoming deeper downwards, and a large and somewhat hard clot of blood external to the lateral ventricle, a softening or breaking down of the corresponding corpus striatum, and part of the optic thalamus.

The extravasation may also influence the development of coma, not simply by its immediate relation to important structures, but by its relation to the cephalo-rachidian-fluid. The pressure of extravasation spread over the hemispheres will be expended upon the whole depth of the brain before it can act upon the base, and, in particular, the cephalo-rachidian fluid in the ventricles will be interposed like a water-cushion between the two surfaces. Should the extravasation be gradual, a corresponding absorption of this fluid will tend to equalize the pressure, and the symptoms may be comparatively trifling; for it is well known how extensively the hemispheres may be injured without serious consequences. But if the blood be extravasated at the base, it is in immediate and direct contact with the great trunk conductors to and from the seat of perception and intellect, and, by compressing them, or otherwise interrupting their functions, will interrupt the functions of the superincumbent mass.

*The connexion between apoplexy and diseases of the heart.* Having arrived at sounder physiological views, Dr. Burrows next proceeds to apply them to pathology. The state of the circulation through the brain must necessarily be influenced by all disorders of the heart—of that central force-pump by which the circulation throughout the system is maintained. Portal was the first writer who devoted any considerable attention to the connexion between apoplexy and cardiac diseases. Dr. Kellie's experiments and inferences probably prevented Portal's doctrines taking that root in the professional mind which might have been expected; and the omission of Dr. Abercrombie to note specially the action of a diseased heart on the cerebral circulation, in his investigations into the pathology of apoplectic affections, tended to keep them still further in the background. But if error multiplies itself, truth is never lost; and Portal's views began at last to modify the views of English pathologists. Of late years we think the doctrines as to the influence of cardiac disease on the vascular system within the cranium have been much less undecided than Dr. Burrows seems to suppose. Dr. Kellie's doctrines never had an extensive influence in *practice*; and so soon as Bright, Craigie, Brichteau, Bouillaud, Andral, Hope, &c., took up its pathology, the influence of cardiac disease in predisposing at least to apoplexy was clearly established. "It is found, for instance," Dr. Craigie observes, "that in most cases of apoplectic seizure, either the left ventricle of the heart is affected with hypertrophy or dilatation, or the mitral valve is ossified, and its aperture contracted; or the aortic valves are more or less diseased; or even the whole of the arch and part of the descending aorta may be ossified, roughened internally,

and incapable of transmitting the blood properly.”\* Nevertheless, it is interesting to find that Dr. Burrows presents us with more precise data, founded upon tabulation by the numerical method. In the subjoined table he gives an analysis of 132 cases of apoplexy and sudden hemiplegia, derived from trustworthy sources, arranged with reference to the coexistence of cardiac disease :

Authors.	Cases.	Diseased Heart.	Per Cent.
Andral . . . . .	25	15	60
Clendinning . . . . .	28	15	53·5
Hope . . . . .	39	27	69·4
Burrows . . . . .	34	23	67·6
Guillemin . . . . .	6	4	66·6
Total . . . . .	132	84	63·6

“The inference from the foregoing calculation is, that in any given number of cases of apoplexy and sudden hemiplegia, no less than three fifths will present unequivocal signs of cardiac disease: either hypertrophy, dilatation, valvular disease, or some combination of these lesions. This proportion proves that the frequency is much greater than is commonly supposed, even by those who admit the occasional influence of cardiac disease in the production of apoplexy and hemiplegia.” (p. 117.)

The order in which the pathological phenomena arise in cases of this kind is very neatly and lucidly stated by Dr. Burrows. The constantly augmented force with which the blood is projected from the hypertrophied left ventricle gradually dilates the cerebral arteries, and diminishes their contractile power. The surrounding cerebral substance thus becomes more liable to pressure from the column of blood sent through them, and on any sudden increase of this pressure coma may take place, or the tunic of an artery yield, and hemorrhage occur. Should there be an atheromatous condition of the arterial tunics, the latter is the more likely to take place. Valvular diseases of the left side of the heart act on the brain by exciting congestion, first of the pulmonary vessels, then of the right cavities of the heart, and, next, of the venous system within the cranium. While the blood is thus retained on the one side, if there be also hypertrophy of the left ventricle, it is propelled with increased force on the other ; and thus the brain is included between two opposing forces, and both classes of vessels distended and dilated.

“The relative frequency of these several cardiac lesions in cases of apoplexy and sudden hemiplegia may be estimated from the following analysis of twenty-five cases recorded by Andral, and thirty-four cases taken from my own case-books :

	No. of Cases.	Heart diseased.	Hypertrophy with valvular disease.	Hypertrophy (simple).	Valvular disease.
Andral . . . . .	25	15	9	4	2
Burrows . . . . .	34	23†	10	6	6
Total . . . . .	59	38	19	10	8

(p. 123.)

*Influence of age on apoplexy.* In considering cardiac disease as a cause

\* Elements of the Practice of Physic, 1840, vol. ii, p. 885. See also the Edin. Med. and Surg. Journ., vol. xix.

† In one case there was simple dilatation of the cavities.

of apoplexy and hemiplegia, we have followed our author hitherto through his researches exclusively devoted to a consideration of the force-pump. But the condition of the conducting tubes merit, we think, an equally decided notice. On this point Dr. Burrows's work is defective, as he only incidentally alludes to the condition of the cerebral arteries in cases of apoplexy. It has occurred to every practitioner to observe that persons suffer from extensive disease of the heart, and yet very rarely experience an apoplectic attack, or any important symptoms which can be referred to the brain; and, upon further consideration, they will probably remember that almost all the cases of prolonged cardiac disease, thus free from cerebral complication, have occurred in young persons. We know of no numerical data which show the number of apoplexies in persons under twenty years of age; but it is remarkable that they are so few as to be thought unworthy of notice by Dr. Burrows and other numerical writers. In an elaborate table which Dr. Burrows has drawn up of the ages at which 215 cases of apoplexy and hemiplegia occurred, the cases being related from his own records, or from those of trustworthy observers, there is not one entered under the age of twenty. It is a circumstance worthy notice, too, that the numbers at each age are progressively greater as the age advances. We subjoin the very valuable table which Dr. Burrows has made; in which the proportion to the population living at each age is indicated—a most important estimate:

Age.	No. of Cases.	Population of this age.	Proportion of cases in 1000 persons.
20 to 30	16	3,000	5.3
30 to 40	30	2,500	12.0
40 to 50	40	1,800	22.2
50 to 60	41	1,300	31.5
60 to 70	54	1,000	54.0
70 to 80	30	500	60.0
80 and upwards	4	200	
Total	215	10,300	

“It must be remembered that the figures in the second column of the preceding table do not represent the *actual* numbers of cases of apoplexy and hemiplegia occurring at successive ages in any given population (ex. gr. 20,000), but only the *relative proportion* of cases in each successive decade, and this is compared with the numbers living of the same age. The population is assumed to be 20,000, of whom about one half will have attained the age of twenty years; and the numbers living in the successive decennial periods will be *nearly* those assigned in the third column of the table.” (p. 131.)

Dr. Burrows, adopting some views of Dr. Clendenning as to the increase in size and force of the heart, with increasing age, attributes this greater frequency of apoplectic affections at the more advanced ages to the increased force and size. Bizot found that the walls of the left ventricle, and the septum, increase in thickness as age advances; but he found also, that a gradual change in the arterial tunics was equally constant. Baillie, Stevens, &c., have remarked how constantly they are affected in old age by atheromatous degeneration, and, indeed, considered it almost a normal state. Bizot found opacity of the membranous portion of the semilunar valves in 80 per cent. of females aged from 60 to 89 years; and in 92 per cent. of men of the same age. The first traces of this change are usually observed in



men about the age of 49. Considering, then, the constancy of atheromatous degeneration in persons aged about 50, we should feel inclined to think that the great predisposing cause of apoplexy at ages above that is this brittle condition of the arteries, whereby they are rendered unable to resist—not the column of blood impelled ordinarily by the hypertrophied left ventricle, but the accidental propulsion, or retention, of a larger quantity of blood than usual. In short, that the adaptive power existing in the resiliency of the arteries of youth and manhood is impaired in proportion as age advances; and that, consequently, in cases of apoplexy, we should look as carefully to the condition of the conducting tubes—the cerebral vessels, as to that of the central force-pump—the heart. Whether, however, the predisposing cause be in the one part of the circulating system, or in the other, or whether it be in both, the quotation from Goldsmith, by which Dr. Burrows illustrates the most suitable treatment, is not less apt:

“To husband out life’s taper at the close,  
And keep the frame from wasting by *repose*.”

*Treatment of Apoplexy.* The practical deductions as to treatment, as given by Dr. Burrows, seem to us to be eminently sound and valuable, so far as they go. In the first place, the posture of the patient should be attended to; he should be so placed that the blood may gravitate from the head. Secondly, however valuable the indication by the pulse may be as to the propriety of bleeding, that operation should not be decided upon until the condition of the heart has been ascertained. Without this knowledge the pulse cannot be taken as a safe guide.

“But suppose a careful examination of the apoplectic or hemiplegic patient’s heart discloses the existence of valvular disease, to the extent of obstructing the circulation through its cavities, here the pulse will be a most deceptive guide as to the propriety or impropriety of abstraction of blood. If the mitral valve be principally implicated, and allow of regurgitation from the left ventricle, the small and irregular pulse so commonly observed with that lesion would probably dissuade from that free abstraction of blood which the cerebral symptoms might require. If, in another case of apoplexy or hemiplegia, the aortic valves be found diseased to the extent of not only obstructing the onward current of blood, but also of allowing regurgitation into the ventricles, during its diastole, there will probably be associated with this lesion considerable hypertrophy of the left ventricle. Here will be observed a full and vibrating or thrilling pulse, but a pulse of increased action without real power, and hence a deceptive pulse; and one which, if it be regarded without reference to the structural changes of the heart, would invite to a more copious abstraction of blood than was called for by the general symptoms. In each of these last-mentioned cases, greater relief to the symptoms will be obtained by a free local abstraction of blood from the vicinity of the heart (either by cupping from beneath the left mamma, or between the left scapula and spine,) than by a much larger depletion by venesection.” (p. 142.)

When the inflammatory stage supervenes, the symptoms

“May generally be controlled in a most striking manner by small local depletion from the temple or mastoid process on the side opposite to the paralysis; by the application of cold to the head, and by the administration of purgatives, by restricted diet, and by extreme quiet in the sick room. In addition to these remedies I have found, when the heat of head is diminished, that a blister ap-

plied near the occiput affords great relief to the oppressive headache. If the patient be not very advanced in years, or extremely exhausted by depletion, great benefit will be derived at this stage of apoplexy from the administration of small doses of mercury. One grain of calomel may be given every six hours, leaving the mineral to act as a purgative, or to slightly affect the gums, but not allowing the mercury to produce ptyalism, which is generally very distressing to the hemiplegic patient, whose powers of mastication and deglutition are already impaired by his disease." (p. 146.)

The sequelæ of an apoplectic fit require great attention. The pains in the palsied extremity, dependent most probably on centric irritation, are only relieved by time. The changes in the brain gradually advance, and the affected parts at length put on a more healthy action. It is during the convalescent stage that the greatest circumspection is required, as patients are apt to resume their mental and bodily activity at a very early stage. If this be permitted, Dr. Burrows observes—

"One of three accidents may happen. First, if there be too early excitement of the cerebral circulation after the fit, and before the fluid parts of the extravasated blood are absorbed, and while the cerebral substance around is still soft from infiltration, and before a cyst is formed around the clot, a fresh extravasation will take place, and produce irreparable mischief.

"A second accident likely to ensue from too early exertion after apoplexy, followed or not by palsy, is a renewal of the head symptoms, and subsequent disorganization of the cerebral substance.

"A third serious consequence of a too early return to mental occupation, or the anxieties of business, is the supervention of inflammation of the brain and its membranes." (p. 149.)

So far as the circulatory system is involved, in the treatment of apoplexy, this view will, we think, be acknowledged to be extremely judicious. The treatment, however, of this class of affections involves more important and recondite points than the dynamics of a diseased heart. The circulation through the brain is liable to be impeded by numerous other agents: Diseases of the air-passages, lungs, and liver are amongst those which act like cardiac disease. Then there is that whole class of affections in which there is morbid change in the circulating fluid, as in disease of the kidneys; and another numerous class may be found in diseases of organs which exercise an incident excitor action on the brain. Gastric disorders belong to this category. Nor can arthritic affections, so frequently seen in men of a sanguineous temperament, and so constantly connected with atheromatous changes in the arterial tissues, be overlooked.

*The connexion between cardiac diseases and functional disorders of the brain.* It is a necessary conclusion, from the foregoing experiments and deductions, that diseases of the heart must have important etiological relations to functional diseases of the brain. There result from the disturbance of the cerebral circulation, vertiginous affections, cephalæa, nervous irritability; and even insanity, may (with a suitable concurrence of circumstances) be immediately dependent on cardiac disease. Dr. Burrows first relates five instructive cases, to show the influence of the latter in inducing epistaxis. In one of these the hemorrhage appeared to be periodic, and the whole warn us, that it should not be regarded as an isolated or unimportant symptom. Dr. Burrows even states his opinion that it may "often be considered as strictly pathognomonic of an obstructed

circulation through the heart, as hemoptysis is symptomatic of tuberculated lungs, or intestinal hemorrhage of an indurated liver." Probably that congestion of the Schneiderian membrane, which excites sternutatio—one of the apoplectic molimina—is sometimes dependent on the same causes as the epistaxis.

*Headache.* This is often a prominent symptom in hypertrophy of the heart, and its origin is often mistaken.

"These persons generally have pallid faces, and peculiarly hard, incompressible pulses; they are troubled with severe and obstinate headache, vertigo, some impediment in the speech, muscular tremors, or imperfect palsy. The histories of these patients almost invariably agree in one particular—that they have been addicted to spirit-drinking; they are also liable to be attacked with profuse hemorrhages, and ultimately become affected with general dropsy. My attention was first directed to these patients by Dr. Latham, and numerous instances have I since seen; and the more closely I have studied them, the more frequently have I found such persons labouring under hypertrophy of the heart, without evidence of great valvular obstruction, but perhaps combined with albuminous urine." (p. 176.)

*Nervous irritability* is another affection consequent on functional disturbance of the brain, the origin of which may be traced to cardiac disease.

"Middle-aged persons, of both sexes, in the upper ranks of society, apply occasionally for medical assistance, and who are suffering from uneasy sensations in the head, lowness of spirits, feelings of debility, occasional faintness, disposition to sigh, urgent desire for fresh air about them, irritability of temper, incapability of steady occupation, disturbed rest at nights. Such persons are often supposed to be dyspeptic, hysterical, nervous, or on the verge of insanity. Neither they themselves, nor their ordinary medical attendant, have suspected the existence of any disease of the heart. Auscultation in these cases has several times revealed to me the physical signs of valvular disease in the heart, or of serious changes in the aorta, and then, upon more close inquiry, other symptoms indicative of interruption to the course of the blood through the heart are confessed to.

"The history of such individuals often informs us that they have suffered from rheumatic fever many years before, or they have had some previous severe inflammatory affection of the chest, most commonly pleurisy; or they remember, under the influence of mental excitement or bodily exertion, many years ago, having felt some peculiar sensation in the cardiac region, or that they suddenly fainted. The subsequent progress of such develops many more unequivocal symptoms of confirmed cardiac disease." (pp. 178-9.)

Dr. Burrows thinks if auscultation were generally employed among insane patients that numbers would be found to be suffering from cardiac lesions. Dr. Burrows rides his hobby pretty vigorously here. May not disease of the brain induce disease of the heart? We must not suppose the brain to be a passive organ; only acted upon, and never reacting. Cardiac diseases are certainly more frequently fatal amongst the insane than amongst the general population. In an interesting table given by Dr. Thurnam, in his "Statistics of Insanity," the deaths from diseases of the heart in the Retreat, near York, are 6.402 per cent. of the total deaths; while in England and Wales, according to the returns of the Registrar-General, they were only 1.075 per cent.

*The connexion between acute cardiac disease and cerebro-spinal affections.* Dr. Burrows gives a short literary history of the progress of our

knowledge as to the connexion between acute diseases of the heart and spasmodic and maniacal affections. Six of these anomalous and deceptive cases have come under his own observation; so that he is led to believe they are of more frequent occurrence than is generally supposed. These and other examples from authors are detailed, the whole numbering sixteen cases of pericarditis, complicated with disease of the brain and spinal cord. Eleven of these were fatal; and Dr. Burrows observes, in not one of them could a trace of disease be discovered in the brain or its membranes, and a morbid change in the spinal cord in only two. In seven of the sixteen there was no rheumatic affection. In four of the cases treated successfully the diagnosis of cardiac disease was satisfactorily established; in the fifth it was suspected. In two of the eleven fatal cases cardiac disease was detected during life; in one it was suspected; and in the remaining eight was only discovered on examination of the corpse.

Various explanations have been attempted of the pathological connexion between functional disorder of the cerebro-spinal axis and cardiac lesions. Dr. Burrows adopts the explanation of Dr. Bright (which also closely coincides with that of Bouillaud); namely, that the inflammatory affection in these cases implicates the phrenic nerve, and that it is the more immediate means of communicating the irritation to the brain and spinal cord. Dr. Hope considered that the pneumogastric nerves might be the channel, while Dr. Watson is of opinion that the cerebral symptoms depended upon a disturbance of the cerebral circulation.

We are inclined to think, on a careful perusal of Dr. Burrows's cases, that they are too heterogeneous to be tabulated, or to form a basis for numerical deductions. The rheumatic cases should have been arranged in one division; for there is no analogy between case 1, headed "Active Articular Rheumatism, complicated with Carditis and Pericarditis, presenting the ordinary symptoms of an Inflammatory Affection of the Brain," occurring in one of the boys at Christ's Hospital, or case 13, of a young man labouring under general dropsy, and dying of apoplexy, and case 16, of a middle-aged man suffering from asthma and an extraordinary curvature of the spine. A careful analysis of such cases would, we think, show that our estimate of their value is just. In case 2 (related by Dr. Burrows) the head was not examined. In case 3, by Dr. Abercrombie, chorea and hysterical (?) delirium came on subsequently to the inflammatory attack, and were probably the result of excessive depletion; in this case, also, it does not appear that the head was examined. In cases 9 and 10, the supervention of dementia and insanity was a sufficient proof of the morbid change in the brain or membranes.

We think that the cerebral symptoms in cases of the kind described by Dr. Burrows do not arise from one or two causes only. When it can be distinctly shown that the inflammatory action in the pericardium has implicated the phrenic nerve, the theory of Bright and Bouillaud, adopted by Dr. Burrows, will suffice. But Dr. Burrows should not, we think, have omitted to point out distinctly the cases in which this nerve was implicated. In six of the cases we might *suppose* such a complication, in the remainder there is not even ground for the supposition. On the other hand, it must be remembered how many cases of pericarditis occur without this cerebro-spinal complication.

Another cause operating in the rheumatic forms of this class of cases, is a metastasis of the morbid action to the membranes of the brain and spinal cord. Dr. Burrows, without denying its occasional occurrence, objects, that in not one of the eleven cases could a trace of disease be discovered in the brain and its membranes. Dr. Burrows has himself explained how excessive congestion may exist before death, and yet be undiscovered after death. But we apprehend that it is precisely in rheumatic metastases we might reasonably expect such a result, without allowing for the effects of depletion. We see rapid metastasis from one joint to another, inducing apparently violent inflammatory action and impeding the functions of the articulation, and yet leaving no trace of its previous presence after it has disappeared. Just such a state of things may occur to the serous membrane of the brain. Further, changes may be thus induced in the delicate structure of the brain, which, inappreciable to the naked eye, would be rendered obvious by the microscope, and yet no mark of abnormal vascular action remain.

In chronic cases, the complication of renal disease with pericarditis is not unfrequent; and, as one of the principal secondary results of structural disease of the kidneys is to disorder the functions of the brain, as well as of the serous membranes generally, we think attention to those organs in cases of the kind under discussion is essentially requisite, if we would attain a perfect knowledge of their pathology. Now, we do not find that Dr. Burrows has directed his attention at all to this important point. Lastly, we think vascular pressure from the diseased heart sufficient under a suitable combination of circumstances to develop the complicating symptoms.

*Treatment.* Although we have differed in opinion with Dr. Burrows as to the pathology, we fully coincide with him in enforcing the necessity of an early examination of the heart by means of auscultation in all obscure and intractable affections of the brain and spinal cord. Dr. Burrows's plan of treatment is, to deplete repeatedly from the region of the heart by cupping-glasses applied between the left scapula and the spine, or by the application of leeches near the left mamma. Venesection may be indicated in robust persons, and he is not of opinion that free depletion from the arm favors a metastasis to the heart in rheumatic inflammations. As regards counter-irritants.

"There is no remedy," Dr. Burrows observes, "upon which, after local depletion, I place more reliance, in the treatment of pericarditis, than a blister, and, in the more severe cases of pericarditis, characterized by hurried respiration, anxious countenance, great dyspnea, short cough, frequent small pulse, with the physical signs of pericardial effusion, the amendment consequent on the application of a large blister to the sternum is so rapid and striking, that it can hardly be realized by those who are not in the habit of contemplating such cases.

"In all cases of pericarditis, excepting in patients affected with tubercular disease of the lungs, or in the very anæmic and cachectic, I employ mercurial preparations as freely as in acute inflammations of other parts."

He administers

"A few large doses of calomel, varying from five to ten grains, combined with a grain of opium, and then continue half the dose of the same combination at moderate intervals, until the urgent symptoms abate, or the mouth becomes distinctly affected by the mercury."

Dr. Burrows would have written a more valuable monograph, had he cultivated a more extensive acquaintance with recent authors, both British and foreign. It is rather late in the day to quote a writer like Dr. Cooke. It is clear, too, that Dr. Burrows is warmly devoted to his *idolum specûs*; so manifest, indeed, that no harm we are sure will result from the great prominence he gives to the etiological relations of cardiac disease in affections of the brain. The work, however, taken as a whole, is excellent; it has the characteristics of an English production in an eminent degree; it is lucid, precise, practical; and will, we think, establish Dr. Burrows's reputation.

## ART. X.

1. *Examen Clinique de l'Hydrotherapie.* Par H. E. SCHEDEL, Docteur en Médecine.—Paris, 1845.
2. *The Dangers of the Water Cure, and its Efficacy examined.* By JAMES WILSON, M.D., and JAMES M. GULLY, M.D.—London, 1843.
3. *The Cold Water Cure: its Uses and Misuse examined.* By HERBERT MAYO, M.D., F.R.S., formerly Surgeon at Middlesex Hospital.—London, 1845.
4. *A Medical Visit to Graeffenberg.* By Sir CHARLES SCUDAMORE, M.D., F.R.S.—London, 1843.
5. *Hydropathy.* By EDWARD JOHNSON, M.D.—London, 1843.
6. *Graeffenberg; or, a True Report of the Water Cure, with an Account of its Antiquity.* By ROBERT HAY GRAHAM, M.D.—London, 1845.
7. *Life at the Water Cure, or a Month at Malvern.* By R. J. LANE.—London, 1846.
8. *Confessions of a Water Patient.* By Sir E. B. LYTTON, Bart.—1845.

IN consequence of the modern water-cure having been originated by a non-medical and uneducated man, and having been subsequently, for the most part, adopted and professed by lay practitioners, or by medical men of somewhat equivocal reputation,—and yet more, from the system being held out as a *panacea* or cure for all diseases, with an exclusive scorn of medicinal aid,—the medical profession, as a body, have naturally enough, and not inexcusably, treated it with much contempt, not to say aversion, and have shown a pretty general determination not to admit it into the catalogue of therapeutic means. Exercising a natural influence on the public, medical men have succeeded in communicating to a large portion of the intelligent classes the feelings entertained by themselves. Thus hydropathy has become a tabooed subject, being either entirely excluded from medical journals and medical books, or only admitted into them for the purpose of being ridiculed or utterly denounced. Indeed, it is regarded almost as a violation of professional etiquette to mention this subject in the language of toleration, much more to speak of it with approbation. Accordingly, we think it not unlikely that some of our brethren, and those even of the most estimable, may regard our present article as a departure from what is medically proper, and will pronounce us almost worthy to have the severe sentence of “water-doctor” passed against us. We have, however, been too long accustomed to speak our opinions openly and



boldly, when we believed them to be just, whether they were in accordance with the current notions or not, to be deterred, on the present occasion, by any apprehended risk of offending mere professional conventionalism. Whatever we conscientiously believe to be true in medical science, especially if, at the same time, calculated to promote the great end and aim of all professors of the healing art—the increase of the means of lessening the sufferings of mankind—that we shall freely and fearlessly promulgate, careless of personal consequences.

Our purpose, in this article, being carefully and calmly to investigate the real merits of the system now so widely established under the name of hydropathy, we hold ourselves absolved from mixing up this investigation with any considerations whatever respecting the merits or demerits, the objects or motives, of those who practise it. We regret to think that there is, and has been from the beginning, not a little quackery and mystification mixed up with really effective practice, in hydropathic proceedings, and that not a few of the conductors of the water-establishments have been, and are, very ill-qualified to indicate, much less to direct and conduct, any therapeutic processes capable of modifying, in an important degree, the vital conditions and functions of the human body. If it shall appear, however, as we believe it will, on further examination, that the external application of cold water is capable of being beneficially applied, in the cure of diseases, in modes of greater efficacy, and to a much greater extent, than has been hitherto practised by medical men, there remains only one course for the members of the profession to pursue, viz. *to adopt the improvements*—if such they are—regardless of their origin, or their past or present relations. When the religious reformer proposed to adapt profane airs to church psalmody, saying that he saw no good reason why the devil should have all the good tunes to himself, he is generally supposed to have acted as wisely as he thought shrewdly and spoke quaintly. In like manner, we see no good reason why the doctors of the orthodox or legitimate school, should refuse to accept good things, even at the hand of the hydropathists. They have done like things before now, as the pharmacopœia, in more pages than one, can testify; and we have not heard that there has been any great reason for regretting that they did so. For our own parts, we avow ourselves of such a catholic spirit, and so lowly-minded withal, as to be ready to grasp any proffered good in the way of **HEALING**, whosoever may be the offerers, and wheresoever they may have found it. Not merely hydropathy, but even mesmerism, yea, stark-naked and rampant quackery itself, may, in this sense, be a welcome knocker at the gate of physic. It is not the demerits of the donor or the birthplace of the gift, that, in such a case, we are bound to look to—but simply whether it is qualified to aid us in our glorious and divine mission of soothing the pains of our fellow-men. If it is so qualified, the baseness of its source will be lost in the glory of its use; and, if aught of its original impurity still attaches to its application in our hands, the fault will be in us, not in it. A saint may sing the devil's tunes without contamination; a hero may wield the weapon he has wrested from a robber or a murderer; the medicament or the formula of the most arrant quack may be hallowed in the prescription of the true physician.

It is in this spirit we enter upon an investigation of the claims of

Hydropathy, as propounded and practised by Preissnitz and his disciples. And we invite our readers to follow us in a like temper, convinced that they will be benefited by an examination of the subject, whether they adopt our views or not. Some of our views we are sure they must adopt—particularly this: that cold water, applied in the manner of the hydropathists, is a powerful modifier of the condition of the human body, both in health and disease, and, when weighed in the therapeutic balance with other remedies, merits, at least, a fair trial in legitimate practice.

It will be an after consideration in what manner, or under what circumstances, this trial can best be made; and, supposing the result of the trial to be satisfactory, it will be a yet further consideration, and one of great importance, how the remedy shall best be applied in the ordinary practice of medicine. We ourselves believe that distinct bathing establishments will still be found best for giving full effect to the hydropathic system, although we believe, also, that many parts of it may be adopted in ordinary practice at the patients' own homes; and the whole of it certainly be conducted at the water-establishments under the authority and general direction of the ordinary medical attendants. If hydropathy is, as we believe, a therapeutic agent of great power and value, it would be worse than absurd to exclude it from legitimate medicine; but, if it is to be adopted by the profession, it can only be adopted in a strictly professional manner. If distinct establishments are found to be requisite for its complete and successful exhibition, the members of the medical profession can, of course, sanction and patronise those only which are conducted by legally qualified and competent practitioners. And they cannot be expected to show any countenance, even to those which, although under the superintendence of legally qualified persons, are conducted on empirical or absurdly exclusive principles. A hydropathic establishment should be simply a great bathing establishment, or water hospital, and should contain the means for using water in all its medicinal forms, hot as well as cold, in the form of vapour as well as liquid, medicated as well as pure. In such an hospital, although drugs would, doubtless be but in slight requisition, it would be contrary to all rational proceedings to exclude their use entirely. The very fact of a case being sent to such an hospital, presupposes the previous failure of drugs, or, at least, presumes their unsuitableness in that particular instance; and they would, for the most part, be dispensed with in the *commencement* of the treatment, at least: but no unprejudiced and competent observer can assert that drugs should be entirely banished from the treatment of any case at all times. The same scientific judgment and the same practical skill that prescribed the water-treatment as best calculated to fulfil the indications present at any one time, could alone determine whether, at any other time, medicaments might be proper, either as auxiliaries or substitutes. Nothing but the blindest dogmatism or the wildest empiricism could maintain that, because the water-treatment is found useful, all other means must be useless; or, reversely, that, because drugs are often found beneficial, that therefore all other kinds of treatment, hydropathy included, must be injurious. The absolute exclusionist, be he water-doctor or drug-doctor, is equally unreasonable and equally unjustifiable.

In the composition of the following article we have derived our mate-

rials mainly from the published writings of hydropathists, but also, partly from personal observation of the practice of hydropathy itself, and from the reports of patients who had been the subjects of it. We have been careful to select as our authorities the best informed and most impartial of the writers on the subject of the water-cure, and we have used our best endeavours to appropriate what alone seemed trustworthy. It is so extremely difficult for a writer, on any one side of a question that has become the subject of active controversy, to avoid partiality in relating events and drawing inferences, that we make no apology to our authors for having on many occasions refused their evidence and rejected their conclusions. Many things, however, we have admitted on the authority of the writers alone, when they did not seem to be contradicted by other facts, and were in accordance with the general principles of physiology and therapeutics. We have so far admitted the validity of the maxim—*cuiuslibet in sua arte credendum*; and, so qualified, we think the propriety of the admission will not be gainsaid. But we have gone further than this. We have accepted at the hands of our hydropathic authors more than one alleged fact and explanation, even although their validity seems to us questionable. And we have done this, because the statements are of a kind justly to challenge attention, and to demand thorough investigation.

On the whole, then, we wish the reader to be prepared to find in the following article, not simply an exposition of the doctrines of hydropathy, as they appear to ourselves well established, but such also as they are laid down by the best authorities of the water-school; one of our objects in writing it being, not merely to endeavour to ascertain what we consider as truth, for the benefit of our readers, but likewise to incite them to make inquiry and examination for themselves, in order that agencies, of such obvious potency on the human frame, may no longer be permitted free scope if evil, or no longer be debarred from ordinary medical practice, if good.

The internal and external use of water, in the treatment of disease, has been frequently discussed by physicians in all ages, from Hippocrates downwards. Their opinions will be found cited in detail by the systematic writers on the subject of baths, and, among others, by Sir John Floyer and Lanzani. To them we refer such as are sufficiently curious to wish for an exact acquaintance with the subject, in its historical relations. For our present purpose, and to render the history of the medical use of water clear to the less minute student, we will group it under a few convenient heads.

1. According to Lanzani,\* the true method of using cold water consists almost entirely in its internal administration, in very large doses, in certain stages of certain fevers. His work is most elaborate in every sense; learned, methodical, and comprehensive. It is divided into two books: the first devoted to an explanation of the causes, symptoms, complications, and nature of fever; the second, showing that copious imbibition of cold water is the best means of combating the symptoms, on scientific grounds, and consequently the best remedy for fever. This is obviously an argument somewhat theoretical, but it is supported by a chapter of

\* *Vero Metodo di servirsi dell' Acqua Fredda nelle Febri ed in altri Mali, sia interni come esterni.* Di Nicolo Lanzani, Medico Napoletano. 2da edizione. In Napoli, 1723.

cases, and backed by the opinions of a host of learned doctors, the author's predecessors. The actual value of the work is considerably diminished by its scientific character, because many of the doctrines held in its day have now become obsolete, and tend to encumber and obscure, rather than strengthen and enlighten, the practical facts by which they are accompanied. But the same remark applies to the early advocates of other remedies. Lanzani appears to have had no knowledge of the external use of water, nor of its application to the treatment of chronic diseases. He used it in combination with drugs.

Lanzani may be regarded as the representative of a considerable number of writers and practitioners, both in Italy and elsewhere, among whom water has been employed (internally) as the most effectual febrifuge.

2. About the year 1700, Sir John Floyer and Dr. Baynard employed water very freely as an external application, in the ordinary manner of cold bathing, preceding it by a course of physic, and accompanying it generally by copious water-drinking.\* Their practice appears to have been chiefly in chronic diseases, such as rheumatism, gout, paralysis, indigestion, general debility, and various nervous affections, in the whole of which a large amount of success is said to have been attained. The baths, at which their cases were treated, were frequently designated by some saint's name. Probably a remnant of superstitious reverence for the saint not only assisted to attract patients to the well, but infused into them a faith in the remedy, which materially promoted their recovery. The practice pursued was simply cold plunging, guarded by certain rules and cautions to prevent accidents.

Sir John Floyer supports his views by the citation of numberless learned authorities, from the Bible to Dr. Mead. He seems to have attached rather an excessive importance to grave precedents, which causes his portion of the conjoint work to savour more of the library than the bedside. At any rate he mingles together practical facts and the opinions of writers, in such intricate relations, that it is not always easy to discover on which he relies most confidently for the maintenance of his tenets. Dr. Baynard, on the other hand, deals more in cases, of which he presents a very abundant collection. His mode of reasoning is particularly pointed and sagacious. No one can leave the perusal of his work without a strong conviction of his being an honest, shrewd, enterprising, and diligent contributor to medical literature.

These writers mention the occasional practice of persons bathing in their shirts, and wearing them throughout the remainder of the day without drying; they also give an instance or two of cases cured or relieved by the application of a wet towel. The former practice is alluded to as an instance of rashness on the part of patients, and the latter is so rarely mentioned, that in neither can they fairly be said to have anticipated Preissnitz in the systematic employment of the wet sheet or wet compress—although both were actually employed by them. They also speak in very favorable terms of a course of cold preceded by a month's warm bathing, but not

\* *Psychrolousia*; or, the History of Cold Bathing, both ancient and modern. By Sir John Floyer, of Lichfield, Knt., and Dr. Edward Baynard, Fellow of the College of Physicians, London. 2d edition. London, 1706.

in the modern hydropathic method of the cold following immediately upon the warm, or upon sweating, which is a practice they carefully deprecate. They seem to have had but a slight acquaintance with the use of cold bathing in fever, or acute diseases, though instances of such practice are given.

The following passage from Dr. Baynard, though not strictly a part of our present subject, is a curiosity, and affords a good sample of his peculiar manner. When the period of its publication is considered, it must be regarded, in some of its parts, as a remarkable case of the forestalling of exact experiment by speculative reasoning. Baynard adduces the remarks it contains in support of his hydropathic views; but we need not stay to examine them in that respect. We transcribe portions of the passage:

"I conceive life to be an actual flame; as much flame as any culinary flame is, but fed with its peculiar and proper pabulum, made out of the blood and spirits for that purpose; and my reasons are these, viz.: 1. Life is as extinguishable as any flame is, by excluding the air, &c. For hold your handkerchief close to the mouth and nose of any animal that has lungs, and life is put out, the creature is dead in a moment; there is no skin broke, nor bone broke; no wound, nor bruise; there is your whole man, but dead he is. 2d. No flame will burn without aerial niter, or a *quid aerium*, whatever it be; some will have it a mixed gas of niter and sulphur, but whatever it be, 'tis a *causa sine quâ non*, something without which no flame will burn; and that the lungs serve to this use, and are air-strainers, is very clear to me, by that experiment of the candle and two puppy dogs, put into a great oven, and stopped close up with a glass door to see through; and in a little time, when they had sucked in some, and the candle wasted the rest of the niter, the dogs died, and the candle went out with them at the same instant.

"All ustion, as the *quid inflammabile* wastes, leaves by incineration alkalious and caustical salts, either fixed or volatile, which, from their figure or imbibed fire, become of a pungent corrosive nature, and fix upon the membranes, being nervous and most exquisite of sense and perception, which by irritation cause a light inflammation, which inflammation is called thirst; which salts are melted and washed off by drinking, the grosser by stool with the solid excrements, but those of most solid and subtle particles creep with the chyle into the blood, and have no way out but by the urine. Hence water is the best menstruum to dissolve salts; and that which is most simple and elementary is the best water, as least impregnated; such waters wash off and dissolve their points and angles, by which they prick, sheath, and envelope them in their own pores, and with themselves run off by urine; but if so forced by heat and motion as to disturb them in their passage, the current of urine is checked, and the salts leave their hold of the water, shoot their vortex, and from the channels get into the habit of the body, which, if not dissolved, melted, and thrown off by sweat, they inflame and cause fevers, &c.; nor will they cease their action and inquietude until totally dissolved, or forced back into their common passages, and the salts precipitated and run down by urine. For I look upon the pores and sweat-vents as so many back-doors and sally-ports, by which nature drives out the enemy crept into her garrison. This truth is demonstrated in all fevers, where the caustical salts are not washed off, but remain behind on the glands and membranes, forsaken of their dissolving menstruum, the water, &c., which that ingenious chemist, Mr. George Moutt, by chemical analysis made appear in six quarts of febrile urine, which I sent him, and he found but the thirtieth part of those salts usually found in a sound man's urine, so that of necessity they must remain behind and be left (like so many French dragoons) to quarter on the blood and spirits at discretion. The history of which is printed in the 'Philosophical Transactions' for some years since.

"Now, that which we call an insensible perspiration is nothing else but the



smoke made from this vital flame, and the pores are the spiramenta through which it passes, and when these are stopped, this smoke is returned and the flame becomes reverberatory, which is sometimes necessary to force an obstruction, for the body has its registers and vent-holes, as well as other furnaces. But to proceed, these salts sometimes crystallize, so that the common menstrua will not touch them, no more than a file will steel or hardened iron, and then it is a true diabetes (and here the physician is at his wit's end, and that no far journey.) then hey! for lime water, quince wine, and other restringents, which, if it were possible, would rather make a coalescence, and tie the knot the harder. No; the cure lies in solution by melting down the salts, which must be done by open, raw, and unimpregnated menstrua, such as the Bristol waters are, as most simple, having least contents in them." (pp. 47 et seq.)

3. About the beginning of the present century, Dr. Currie's practice in fever is well known to have consisted principally of cold affusion, or immersion, in the early stages of the disease, and in certain acute affections of the nervous system. His work is so well known that it is unnecessary to enter into any detail as to its contents.\* He seems to have known but little of the application of cold water to the treatment of chronic diseases, as represented by Floyer and Baynard, nor to have employed the copious libations described by Lanzani. He cannot be said to have forestalled Preissnitz in any other respect, than in the prompt and energetic use of cold water in the suppression of acute febrile and nervous affections. He brings a large amount of scientific argument and practical experience to bear out his views. He has also placed in a clear light some points of practice on which important errors previously prevailed, such as the safety of cold applications when the body is heated beyond the natural degree, and the relative value and safety of cold or tepid water, of immersion, affusion, and ablution. On these points his work is of great practical value. We may have occasion to revert to some of them hereafter.

4. The prevalent opinions of medical men in this country, on the general subject of the external use of water, previously to the Preissnitzian era, may be considered to be represented in the article BATHING, in the 'Cyclopædia of Practical Medicine,' published within the last twenty years. The article in question places bathing in a very subordinate position among means available for the actual cure of disease. In its cold form it is recommended as a valuable tonic, used with many restrictions, in nervous debility and other analogous states; and, in its warm form, its use is almost limited to the allaying of irritation in certain disorders, the more formidable symptoms of which are to be encountered by other remedies. Other articles in the same work have done justice to Dr. Currie's views. Beyond this, the medical profession have hitherto done little or nothing with bathing as an instrument of cure. We shall hereafter find reason for believing that a vast superfluity of caution has existed in the employment of this remedy, and that some of the supposed cautions have really increased, instead of diminishing, the danger, as well as destroyed the efficiency of its application.

The author of the article in the Cyclopædia describes cold bathing as partially or absolutely contraindicated in the following conditions: par-

\* Medical Reports of the Effects of Water, cold and warm, as a Remedy in Fever and other Diseases. By James Currie, M. D., F. R. S., Fellow of the Royal College of Physicians, Edinburgh. 2 vols. 2d edition. London, 1805.



tially, in infancy and old age ; pregnancy ; indurations, obstructions, or chronic inflammations of internal parts ; acute inflammations of the same ; chronic inflammations of mucous membrane : absolutely, during menstruation ; in great plethora, or tendency to active hemorrhage, or congestions in important viscera ; affections of the heart ; loaded state of the bowels ; great general debility—though then often advantageous after warm water or vapour bath.

5. The ancient Romans were accustomed to produce perspiration by surrounding the person with heated aqueous vapour, and, while freely perspiring, to plunge into cold water. Interesting remains of baths for this purpose, evidently of Roman architecture, and containing fine specimens of mosaic pavement, may be seen in several parts of England ; as, for example, on the margin of the Cranham woods, in the village of Whitcombe, about six miles from Cheltenham ; at Bignor in Sussex, &c. It is also well known to have been a frequent practice of the Roman youth to plunge into the Tiber, when heated by exercise in the Campus Martius.

The modern Russians, also, as is well known, excite perspiration in a similar manner, and then roll themselves in snow. A somewhat similar practice has prevailed among the North American Indians. The following description of their process was given by the celebrated Quaker, William Penn, to Dr. Baynard.

“I once saw an instance of it, with divers more in company. For being upon a discovery of the back part of the country, I called upon an Indian of note, whose name was Tenoughan, the captain general of the clans of Indians of those parts. I found him ill of a fever, his head and limbs much affected with pain, and at the same time his wife preparing a bagnio for him. The bagnio resembled a large oven, into which he crept, by a door on the one side, while she put several red-hot stones in at a small door on the other side thereof, and then fastened the doors as closely from the air as she could. Now, while he was sweating in this bagnio, his wife (for they disdain no service) was, with an axe, cutting her husband a passage into the river (being the winter of '83, the great frost, and the ice very thick), in order to the immersing himself, after he should come out of his bath. In less than half an hour he was in so great a sweat that, when he came out, he was as wet as if he had come out of a river, and the reek or steam of his body so thick, that it was hard to discern any body's face that stood near him. In this condition, stark naked, he ran into the river, which was about twenty paces, and ducked himself twice or thrice therein, and so returned (passing only through his bagnio to mitigate the immediate stroke of the cold) to his own house, perhaps twenty paces further, and wrapping himself in his woollen mantle, lay down at length near a long (but gentle) fire, in the middle of his wigwam, or house, turning himself several times, till he was dry, and then he rose and fell to getting us our dinner, seeming to be as easy, and well in health, as at any other time.”—(Baynard, pp. 103-4.)

The extraordinary revivifying effect of the cold plunge bath, after the system had been excited by artificial heat, is testified by various evidence of the most unquestionable kind. Numerous travellers have spoken of this very enthusiastically ; among others, Stevens, the American (in his ‘Incidents of Travel’), who took a Russian bath, after a most fatiguing journey, and came out of it, he says, quite a new man. We have had similar information from more than one private source. This practice, however, similar as it is to that of Preissnitz, has never, until his time,

been extensively, if at all, employed in Europe as a means of curing disease.

6. In the foregoing synopsis are contained the principal forms in which cold bathing and water drinking have been used in the treatment of disease, before these means were so vigorously adopted by Preissnitz. It will be obvious that, from none of the writers mentioned, could he have learned his bold and comprehensive practice. In his method are combined those of Lanzani, Floyer, and Currie, accompanied by novel and powerful processes, to which those writers were entire strangers. The douche, the wet sheet, the sweating blanket, the cold plunging after sweating, the wet compress, the sitting bath (*sitz-bath*), must be allowed to be, in a great measure, peculiar to the Graeffenberg peasant and his disciples. From the same source have proceeded some important precepts on the subject of diet and regimen. Preissnitz, moreover, is distinguished from all the authorities quoted, by his entire abandonment of drugs.

Vincent Preissnitz was originally a small farmer, residing at Graeffenberg, near the town of Freiwaldeau, in Silesia. He is about fifty years of age. The following is a description of him by Sir Charles Scudamore :

“ Of Preissnitz himself I shall say a few words, and describe my impressions on first seeing him. His countenance is full of self-possession; rather agreeable; mild, but firm in expression; with an eye of sense, and a pleasing smile. The smallpox, and the loss of some front teeth from an accident, impair his good looks. His manners are sufficiently well-bred. On closer acquaintance, you discover that he is quick in perception; is reflective; prompt, however, in decision; simple and clear. He inspires his patients with the most entire confidence, and he exacts implicit obedience.” (*A Medical Visit to Graeffenberg*, pp. 2-3.)

Other travellers give a similar description of Preissnitz. They all agree in stating that he is a most arbitrary and tyrannical despot, issuing laws as irrevocable as those of the Medes and Persians, commanding obedience with a haughtiness that might well excite admiration and envy even in an autocrat, and exciting as much fear in his patients as is found in the subjects of the Grand Turk himself. He is also represented as remarkably cool and collected in emergencies, ever ready with his remedy on occasions of danger, and possessed of an imperturbable self-reliance. These traits suffice to prove that he is a man of original and powerful mind, exactly adapted to carry out a novel and startling practice. While his firm and decided manner is calculated to secure the confidence of his patients, his coolness and self-reliance enable him easily to bear the responsibility by which such confidence is attended.

His practice originated in a succession of trifling accidents, by which he was led to employ bathing in a neighbouring spring for the relief of disease. It is not necessary to give them in detail. Success in these first attempts procured him a local renown, and he became the village doctor. From villagers his fame soon spread to patients of a higher rank, and Graeffenberg gradually became the resort of the hipped and the halt from all the surrounding district. By these his praises were sung louder and louder, until all the world began to furnish him patients by the hundred. He now possesses an enormous establishment, capable of containing several hundreds of patients, which is almost constantly crowded with

ladies and gentlemen of every degree, and from every nation; while his disciples and followers, as is well known, have spread themselves throughout the world, and maintain, in every country, numerous and flourishing establishments formed on the original model of Graeffenberg.

His treatment, although apparently constructed of such simple elements, is capable of being varied almost *ad infinitum*, according to the peculiarities of the case or the fancy of the prescriber, and of being rendered so powerful, as often to excite in the patients and spectators apprehensions of danger—and sometimes, no doubt, to produce it in reality. It is scarcely too much to say that he has modified the application of water, and some very few other means, in a manner so ingenious as to render them no imperfect *nominal* substitute, at least, for most of the drugs in the pharmacopœia. He has his stimulant, his sedative, his tonic, his reducing agent, his purgative, his astringent, his diuretic, his styptic, his febrifuge, his diaphoretic, his alterative, his counter-irritant. Combined with these are peculiar regulations as to diet, dress, and regimen. The following is his general mode of proceeding.

In his first interview with the patient, after hearing sufficient to give him a rude insight into the locality and general features of the malady, Preissnitz proceeds to investigate its suitableness to his method of cure. He does this by sprinkling the surface of the body with cold water, or witnessing the taking of a cold bath, and then watching the development of reaction. If this appears in a certain amount of activity, he pronounces the case appropriate for his treatment; if not, he advises the abandonment of all hydropathic intentions. This is a mode of ascertaining the power of the constitution quite original, and it cannot be said to be unscientific. The power of resisting the external application of cold is a most essential conservative property of the animal system, and the degree to which it exists must be regarded as, in some respects, a criterion of the amount of *vis medicatrix* possessed by the patient. We see no very decisive reason for pronouncing it a more fallacious guide than the orthodox custom of feeling the pulse. The only objection to it that occurs to us is that it may not be always free from hazard.

This point being satisfactorily determined, the patient is straightway admitted into the mysteries of the cure. In the first place, he finds himself restricted to a peculiar diet. Every stimulant is absolutely prohibited, from brandy and claret, to mustard and pepper; so, also, are most of the luxuries imported from foreign parts, such as tea, coffee, and every kind of spice. The meals consist of three—breakfast at eight, dinner at one, and supper at seven or eight o'clock. For breakfast, cold milk is the beverage, and bread and butter its only substantial companions. At dinner, there is no other restriction than those above named. Supper is a repetition of breakfast, with the occasional addition of preserved fruit or potatoes. Throughout the day, no warm beverage whatever is permitted, and much of the food is brought to table considerably cooled. As some compensation for these manifold deprivations, the patient is allowed to gratify his appetite with every reasonable variety, and a free abundance, of substantial and nutritious food. He finds it a maxim that generous diet will promote his recovery, the treatment being responsible for preventing surfeit. He no longer finds an embargo laid on fruit and vegetables, and is not ex-

pected to dine seven days in the week off dry bread and mutton chops. So that, on the whole, there is perhaps about an equal amount of indulgence and restriction, as respects diet, to a patient coming to the Graeffenberg rules from those of some fashionable physician in London.

In the next place, the majority of patients are directed to enter upon a course of water-drinking, the quantity of water varying from five or six to thirty or forty tumblers in the twenty-four hours. A large portion of this is taken before breakfast, the rest at suitable periods after meals, so as not to interfere with digestion, with frequently a glass or two the last thing at night. Exercise is generally advised at the time of water-drinking, except when this accompanies some other process of treatment incompatible with it.

A third rule insisted on is, that every patient shall take a large amount of exercise during the day. This is, to some degree, indispensable after the cold baths, as a means of procuring the necessary reaction. Walking in the open air is the mode generally selected, when possible. In case of bad weather, or lameness, other plans are contrived; such as gymnastics, sawing or chopping wood, &c. As a general rule, every patient is required to take a long walk before breakfast. It is a *vexata quæstio*, we believe, among hydropathists, as among doctors, whether the patients should rest or walk immediately after a meal; but the water-doctors generally incline to advise very gentle exercise at such times; and, we believe, properly. The well-known experiments on greyhounds, and such other convincing facts, are counterbalanced, to say the least, by the habits of the working man, who proceeds to his labour as soon as he has swallowed his dinner, and rarely suffers from so doing.

After these preliminaries, and the case being pronounced suitable for the treatment, the next morning witnesses the patient's initiation into more active proceedings. At any early hour of the morning, varying according to the time required for the operation about to be undergone, a bath attendant enters with the formidable machinery for the administration of a rubbing with a wet sheet, a packing in the dry blanket, or a packing in the wet sheet. The first of these processes consists of throwing a wet sheet over the whole person, and applying upon it active friction of a few minutes' duration. A glow is thus excited. The patient then dresses, takes his water, and sets forth upon his morning's walk. The second of the above three operations requires the patient to be enveloped in several blankets, with perhaps the superincumbence of a large feather pillow, until free perspiration is excited, which generally requires a period of about three hours. When the perspiration has continued the prescribed time (from fifteen minutes to an hour, or more), the patient is subjected to some kind of cold bath, either by the wet sheet, as just described, by pouring water over the person from pails or watering-pots, or by taking a plunge bath. This being followed by friction and water-drinking, the morning's proceedings are concluded by exercise. Packing in the wet sheet is similar to the foregoing, with the addition, next the skin, of a sheet wrung out of cold water. It is generally of shorter duration, as forty-five minutes or an hour, the object being to excite a glow, instead of perspiration. It is followed by cold bathing, as just described. During the packing, in both instances, some glasses of cold water are imbibed through a tube.

At other parts of the day, other portions of the treatment are applied, such as the sitz-bath, the douche, the shower-bath, head-bath, foot-bath, &c.

The sitz-bath is a tub of cold water, in which the patient sits for a period varying from a few minutes to an hour, or even longer, using constant friction to the abdominal region. The other baths mentioned in this paragraph need no description. These, as well as the former processes, are sometimes repeated during the day. In certain cases the day's proceedings commence with some of them, in place of those previously mentioned. A rubbing with the wet sheet is frequently employed before getting into bed at night.

In fever, from whatever source, the patient is enveloped in a succession of wet sheets, renewed as often as they become warm, for a period varying with the intensity of the case—say, from 30 minutes to 5 or 6 hours. In other similar cases, cold immersion or affusion is employed with the same view—viz. to reduce the morbid heat of the system.

The *umschlage*, or compress, is an essential and seldom-omitted part of the treatment. It is a cloth, well wetted with cold water, applied to the surface nearest to the supposed seat of the disease, securely covered with a dry cloth, and changed as often as it becomes dry during the day. It is sometimes covered with a layer of oiled silk, which, by impeding evaporation, prevents the inconvenience of frequent change. This compress speedily becomes warm, and remains so until dry. It is termed a heating or stimulating bandage. In cases of superficial inflammation it is more frequently changed, so as to be kept cold, whereby its effect is just the reverse, being then a local antiphlogistic.

In some establishments the sweating has been effected by other means, than the simple envelopments of Preissnitz, as by the vapour-bath, or a chamber highly heated by a stove. We have heard of a temperature of 180°, and even that of 198° Fahrenheit, being employed for this purpose. The blankets used by Preissnitz are very bad conductors of caloric, therefore they cause the heat given off by the body to be accumulated around its surface, by the lengthened influence of which the sudorific action is effected. This process differs in no other manner than in degree and rapidity of effect from exposing the same surface to heat of any other origin. The animal heat, when once evolved, becomes a quality of the surrounding atmosphere. Being kept in contact with the body by blankets, it constitutes an artificial elevation of temperature, and nothing more. Therefore, in cases where active sweating is required, we can suppose no disadvantage to result from using other kinds of artificial heat, and can easily imagine advantages in a higher temperature than that attainable from animal heat alone. But we would limit this remark to dry heat. Aqueous vapour, by a well-known law, impedes evaporation, and would therefore restrict the full completion of the sudorific process. For this reason it is used to prevent plants from parting with their moisture in hot-houses. For the same reason it should *not* be used when the intention is to promote the removal of moisture, or to promote perspiration.

A point uniformly insisted on by Preissnitz is, that his patients should abstain from wearing flannel next the skin. When we consider how generally the use of this article of clothing has been advised by physicians



and adopted by invalids, especially in this country, we can easily conceive that strong prejudices will exist in the minds of patients against relinquishing it. Yet it appears to be almost universally discarded by hydropathists, and, as far as we have learnt, without any mischievous consequences.

Another maxim of Preissnitz is, that his patients are never to take any kind of drug. It should be remarked that, not being licensed to practice medicine, it would be illegal for him to administer drugs. So that it does not follow, from his disuse of them, that he himself would be opposed to their use *in all cases*, much less that their use is in any way *inconsistent* with his practice. His medical disciples, not being similarly restricted, so far as we can learn, usually employ drugs occasionally, though sparingly.

How are we now to proceed, in order to arrive at a just appreciation of the value of the means thus briefly enumerated? The more usual course would be to enter upon an examination of the practical results, as published by hydropathic writers. But, in the present inquiry, this plan would scarcely answer; for the means employed are so strange, so much at variance with those by which disease is commonly treated, and not a few of the reporters are so little entitled to claim credit for even a capacity to report medical results truly, that the greater part of our readers would disbelieve the alleged facts, rather than admit the principles they would carry with them. It will be more proper, therefore, to omit matters of evidence for the present, and to see if we can find in hydropathic practice any conformity with the principles on which we should estimate the merits of any other new remedy.

If a new vegetable were imported, or a previously unknown chemical substance discovered, and we were called upon to use it as a medicine, we should first inquire whether it possessed any of those qualities which are regarded as constituting medicinal virtues. We might assume that we are sufficiently acquainted with the characters of most diseases, to pronounce what description of influence would have a counteracting effect upon them. It would then remain to inquire, whether the qualities possessed by the article in question were of a kind to lead us to expect any description of such influence from their operation. If they were not, we should be indisposed to try the remedy until well assured, from abundant and unquestionable practical evidence, of its curative powers. If they were, we should be inclined to give it a trial, even if the proofs of its remedial properties were not unexceptionable. For instance, if the article under consideration merely possessed a nauseous taste, a specific colour, or a powerful odour, it would offer little inducement for an experiment of its medical powers, because those qualities are not known to possess any intrinsic influence over any diseased condition. But, if it were a purgative or a sedative, no one could hesitate to recognize it as *a priori* entitled to a trial by physicians; because experience has taught us that, by the means of purging or tranquillizing, certain diseases or morbid symptoms may be cured or relieved. And, since it is the case with many of our present remedies, that with the property we wish to employ is combined another we would gladly avoid (purgatives being debilitating, sedatives narcotic, &c.), and with their amount of usefulness is thus associated



a certain tendency to mischief,—if the new remedy presented to us appeared to possess the essential quality, and to want the mischievous power of that otherwise used for the same purpose, we should be still more desirous of availing ourselves of it in practice.

If we apply these remarks to Hydropathy, as practised by Preissnitz, the first inquiry ought to be, does it furnish the physician with instruments which he, as a skilful workman, can undertake to employ? Does it contain, among its various machinery, any really therapeutic means, any powers capable of carrying out the indications which we regard as palpable in many diseases? Can it evacuate, can it brace, can it tranquillise? We cannot entertain the idea that the professors of Hydropathy have hit upon any grand secret concerning the origin or nature of diseases, or the philosophy of their removal. Such a supposition, were it a necessary article of faith in the hydropathic creed, would render us the most obstinate of sceptics. But, if the practitioners of this new school profess merely to have introduced more efficient, or less dangerous, means of fulfilling the purposes which all physicians have in view in treating disease, we are willing to give them a patient and impartial hearing. Or, if they profess nothing of the kind, and reject such an idea with contempt,—if, nevertheless, their system appear *to us* of the nature we are indicating, we can still entertain it with the hope of discovering something of good in it.

Let us now inquire, then, on physiological and pathological grounds, supported by some personal experience, what appear to be the effects, or among the effects, of a course of water treatment according to the Preissnitzian system.

1. In the first place, we remark the careful withdrawal of all stimulants from internal parts. In this hydropathy is at once distinguished from ordinary practice. The refinement of civilized life, and the complicated affairs of society, prevent the human frame from being treated entirely as a machine. The body is compelled to undergo an usage not always suitable to its welfare, in consequence of its having to minister to the mind. The exhaustion of the latter, from exertion and excitement, is restored by artificial stimuli applied to the former. These are generally directed to parts ill adapted for their reception. Thus, the stomach, constructed to digest simple food, and to admit fluid at the impulse of thirst, becomes the vehicle of conveying to the nervous system alcohol in its various forms, and other similar fluids. These are unnatural to the stomach itself, though grateful to the nerves. Consequently, the mucous lining of the alimentary canal may suffer in the attainment of an object required only by the nervous system. This is, possibly, the very origin of a proportion of those manifold chronic ailments known under the terms dyspepsia, hypochondriasis, bilious affections, &c., and is unquestionably an aggravating cause in many. To the treatment of these affections the physician brings his purgatives, his carminatives, his anodynes, his stomachics. But it is to the surface of the same unfortunate membrane that they are all applied; and it frequently results, that, when they relieve temporary suffering, they often leave the general health worse than they found it. From this predicament hydropathy professes to be entirely exempt, by abstaining from artificial interference with internal mucous membranes.

2. In the next place, the Hydropathists adopt a system of diet, such as other practitioners seldom venture to prescribe. If a person, suffering from constipation, or any of its long train of attendant ills, applies to an ordinary physician, he is probably told scrupulously to avoid fruit, pastry, and all vegetables, except, perhaps, a favorite one, or, it may be, two. He is also cautioned against the use of veal, pork, even beef, and new bread. We have known such a patient ordered to live for months—we might say, years—constantly on mutton, and bread never less than five days old. This case is neither singular nor infrequent. What is the consequence of this? The patient is compelled to take aperient pills and draughts every day, or every other day; to stimulate the digestive organs (rendered torpid by the use of so monotonous a regimen) by occasional glasses of sherry or porter; and, to compensate the deficient nutrition obtained from so barren a source, by indulgence in strong tea and coffee. Such a patient goes to a hydropathic establishment, and is straightway ushered into a *salle-à-manger*, in which he finds all the variety of food customary at a foreign *table-d'hôte* dinner, and is told to obey the dictates of his appetite. He does so, timidly at first, and apprehensive of direful consequences, but he finds, to his astonishment, that he can take the forbidden luxuries of broccoli, turnips, veal, game, puddings, and fruit, with as much impunity as the never-varied mutton and dry bread, to which he was previously restricted. This is an occurrence so frequently experienced, and so universally attested by hydropathists and their patients, that we cannot refuse to admit it as a point attained by *their system*—therein being comprehended the water *and all its accessories and concomitants*.

3. A third important principle of hydropathic treatment is, that almost all its measures are applied to the surface. It is one of the most formidable difficulties with which the ordinary physician has to contend, that nearly all his remedies reach the point to which they are directed, through one channel. If the brain requires to be placed under the influence of a sedative or a stimulant, if the muscular system demands invigorating by tonics, if the functions of organic life need correction by alteratives, the physician has no means of attaining his object, except by inundating the stomach and bowels with foreign, and frequently to them pernicious, substances. In being thus made the medical doorway to all parts of the system, and so compelled to admit every description of therapeutical applicant, the organ of digestion is contorted to a purpose for which it was never intended. The consequence is, that it has to be consulted before we enter upon the treatment of any case, and it often forbids our availing ourselves of remedies, or plans of action, which are plainly, perhaps urgently, indicated by the condition of other organs, or of the system at large. Thus, to take the three cases above mentioned: how often do we find that one stomach will neither bear ether nor opium; another is injured by steel; and others are intolerant of mercury. The two latter remedies are peculiarly illustrative of these remarks. Iron is employed to raise the tone of the general system, but it occasions constipation by its action on the alimentary canal; therefore, in order to counteract this portion of its effect, it can only be used in conjunction with aloe, or some other purgative, the tendency of which, as respects the system at

large, may be exactly the reverse of that of the steel. With mercury the case is just the opposite. We wish to introduce it into the system, but it is purgative as well as alterative and antiphlogistic, and the former quality often renders very difficult our attaining the benefit of the two latter. The physician, then, is frequently placed in the dilemma, either to injure the stomach in an attempt to relieve other parts, or to leave the latter to their fate, because they can only be rescued at the peril of the former. His only mode of escape from this predicament is, to employ a legion of *adjuvantia*, *dirigentia*, and *corrigentia*, in the multiplicity and confusion of which it is by no means easy to make out so clear a balance of power as shall enable him clearly to foresee which kind of action, in the *mêlée*, will get the uppermost; and, unless he be well skilled in chemistry, he may unconsciously prescribe a dose so scrupulously guarded as to be neutralized and altogether impotent.

Of course we do not conclude that hydropathy has discovered a remedy for this difficulty; but its own plan of proceeding is not similarly embarrassed, because it deals with outward instead of inward parts. Whether it can produce an efficient substitute for steel, mercury, opium, and other remedies, to which we are alluding, is altogether another question, and one which its professors must bestir themselves to solve, by the careful record and honest publication of their successful and unsuccessful cases.

4. Fourthly, Hydropathy employs a system of most energetic general and local counter-irritation. It has been held by some medical philosophers, that two kinds of morbid action cannot co-exist in the same individual. According to this theory, if we can set up an artificial, but harmless, disease by treatment, its development will be attended by the departure of any other disorder that previously existed. Thus is supposed to be explained the operation of mercury in curing various diseases, the disorder arising from its own action being easily disposed of afterwards. We attach no value to this dogma as a dogma, but it serves to embody a large number of well-known facts, and may be as properly appropriated by hydropathists as by other practitioners. By the diligent employment of hydropathic machinery, due regard being had to the constitutional vigour, a condition is often excited, termed by hydropathists *the crisis*. This sometimes consists in the appearance of various cutaneous eruptions; sometimes it is characterized by a series of boils, more or less severe; in other cases its leading feature is disturbance of the function of some internal organ, creating diarrhea, abnormal urinary discharges, vomiting, &c. In general this effect is trifling, and seldom proceeds to such a degree as to excite alarm, or to give cause for special interference; so that the measures which have led to its appearance are in most cases continued, and in some even increased, until it has run through its course and subsided. This is not always the case: sometimes it proceeds to a more serious length, and requires careful management to prevent mischief; the boils, in particular, are frequently very troublesome;—even death has, in a certain proportion of instances, ensued, either as an immediate or remote consequence of the so-called crisis.

Whatever the crisis may be—or whether what is so called be a crisis in reality—there is no disputing that it results from the operation of a powerful system of counter-irritation—or of irritation at least. It is to

this that we now wish to direct attention, because we suspect that in it is contained the true explanation of the good effects of the water-cure in many chronic cases.

5. A fifth physiological feature of the water-cure is the number of *coolings* to which the body is subjected during the day. The generation of caloric in the animal system has been traced to its real source. It results from the *burning up* of waste matter, which, by accumulation, would become injurious. The oxygen of the atmosphere, admitted into the lungs by inspiration, traverses the various blood-vessels of the body, and, in the minute capillaries, unites with carbonized substances. The union produces the carbonic acid emitted from the lungs in expiration, and is attended with the development of what is called animal heat. It is obvious that lowering the temperature of the body, within certain limits, by awakening an uncomfortable sensation in the nerves, would induce increased activity in this calorific process, in order to maintain or restore the average degree of warmth. This increased activity could only be supported by an additional consumption of carbonized matter. If the carbonized matter were already there, and if its existence constituted the disease, or an important part of it, as is probably sometimes the case, a perfect cure would result from its removal. But supposing there is no such matter present, what then would be the consequence of stimulating this decarbonizing operation? The consequence would certainly be, that the constituents of the tissues themselves would be consumed, in order to supply the pabulum required by the oxygen. This would as certainly excite an effort at restoration, by which the digestive organs would aim to renew to the tissues the amount abstracted by the oxygen. In other words, the appetite would be increased.

Hence it is that more food is required in cold climates than in warm—in winter than in summer. The greater consumption necessary to maintain equal temperature in cold weather, can only be met by increased supply. What, in a vague and general manner, arises from the ordinary progress of the seasons, may be rendered methodical and profitable, by the careful interference of art.

It has been urged that the effect here considered would equally result from exposure to cold air, as to cold water. In the words of Mr. Herbert Mayo, "This is not only entertaining, but satisfactory as far as it goes; and admits very well of being popularly and loosely brought forward in favour of cold bathing: but unluckily it is as much or more in favour of our living in Nova Zembla, as of our resorting to Graeffenberg." (p. 5.)

The same intelligent writer proceeds to notice other modes of exposure to cold, which are found to produce evil instead of good, which are, indeed, familiar as the frequent causes of serious disease, and against which we are of old cautioned:

"Nudus ara, sere nudus,—habebis frigora, febrim."

It is singular enough that this very argument, now employed to discountenance the use of cold bathing, is the very strongest theoretical argument in its favour, as was long ago pointed out by that very sarcastic writer, Dr. Baynard, in the following anecdote:

"Here a demi-brained doctor, of more note than nous, asked, in the amazed

agony of his half-understanding, how 'twas possible that an external application should affect the bowels and cure the pain within. Why, doctor, quoth an old woman standing by, by the same reason that, being wet-shod, or catching cold from without, should give you the gripes and pain within." (p. 119.)

If a rude exposure of the surface to cold and wet is capable of producing internal disease, there is no doubt that a close relation exists between those agents and the morbid conditions of internal parts. Therefore, if they could, by skilful management, be so applied as to excite an opposite effect from that to which their bad consequences are due, they would then become equally powerful means of removing disease. This is the very thing that Preissnitz and his disciples profess to have done—and to do.

Let us, then, consider a little further the consequence of repeated applications of cold, supposing, for the sake of argument, it is used with due reference to the constitutional powers, so as to create an increased activity of the vital functions. It appears to us that this is exactly the thing needed in the treatment of a great many cases of chronic ailments. It is easy enough to construct methodical catalogues of organic lesions and their symptoms, and to assign, on paper, a "local habitation and a name" for every malady that is to require our treatment. But the truth is that, practically speaking, there are a vast number of cases in which the symptoms may be said to constitute the only disease that can be detected, and in which they point rather to a general torpidity or derangement of all, or almost all, the vital functions, than to special change or disturbance in any particular organ. Many cases known as indigestion, gout, rheumatism, liver complaints, or nervous affections, come under this description. In a large portion of such cases, and their like, we could conceive the practice of Preissnitz to be peculiarly beneficial, if it consisted in nothing more than the frequent application, and skilful adaptation, of cold water. It was mainly by this means that the cures described by Floyer and Baynard were effected, simple cold bathing having been almost their only instrument.

6. Another physiological feature of hydropathic treatment consists in its creating a large amount of stimulation in the system. This stimulation is of a peculiar kind, and very different from that produced by alcoholic fluids or pharmaceutical stimulants. The difference is in its not awakening abnormal activity, to be succeeded by abnormal depression, in the nerves and organs of circulation, as is done by the stimulants just mentioned. The fall of a heavy douche, the sudden plunging into a cold-bath with speedy exit, active friction in a shallow bath, are means of stimulating the system in the manner here intended. The effect, we are told, is manifested in the altered look of the patient after taking the bath, in his freshened cheek, his brightened eye, his elastic step, his cheerful tone. But it is *not* manifested in a quickened pulse, or a heated imagination, nor followed by exhausted energy or lowered spirits. This is the description given by hydropathists (whose practice we are not teaching but describing)—and which we have ourselves heard given by patients. It is also said that drinking, in rapid succession, several glasses of perfectly cold water has a decidedly stimulating influence on the system. If these descriptions be correct of hydropathic stimulants, that they are powerful as well as innocuous, exciting and not exhausting, they constitute a valuable

instrument in the treatment of disease, and deserve the more careful attention of physicians.

We happen to have been acquainted with a case of a lady who was at a hydropathic institution for the treatment of very aggravated chronic rheumatism. Her general powers were much shaken, and she had been unable to walk at all for a period of about four years, before undergoing this system of treatment. After several weeks of sweating and cold plunging, locomotion began gradually to return. The first indication of this was, that she could walk a few steps *immediately after leaving the cold bath*. For a considerable time this continued to be the only occasion of her being able to walk during the day, though she afterwards made considerably further progress. We mention this case because we can guarantee its truth, and it always appeared to us a striking and instructive instance of the stimulating property of a cold bath.

7. A still more important and less questionable quality of the water cure is its power of lowering the system to any extent, without any of the debilitating means otherwise used for that purpose. In a general inflammatory or febrile condition of the body, a lengthened immersion in cold water, or envelopment in a succession of cold wet sheets, would reduce the temperature and force of circulation to the most extreme degree. These means are, to the functions of life, what an extinguisher is to a flame. Their reducing power can be gradually applied up to the point of actual extinction. Anywhere short of that, withdraw the means, and the flame, whether of oil or of life, gradually resumes its previous brilliancy.

In the treatment of febrile diseases an important indication is to reduce the morbidly increased activity of some of the organic functions, most distinctly manifested in the circulation and the temperature. For this purpose the great instrument heretofore most in use is blood-letting, as being our only certain and expeditious method of reducing the frequency, force, or fulness of the pulse. So that, in order to suppress febrile action, we hazarded occasioning a more or less lingering debility. The *post hoc*, whether *propter hoc* or not, is too frequently a protracted convalescence, during which the patient is in constant danger of relapse. The mortality that occurs during convalescence after fever, from recurrence of the original disease, from some of its numerous sequelæ, or from the accidental inroad of some other disorder, is so considerable as to render this a period of great anxiety to the patient and the physician. It is a question deserving of cautious and dispassionate investigation, whether any portion of the liability to these mishaps is attributable to the bleeding, purging, salivating, and low diet, employed in removing the fever.

In some of the cases of fever described by Currie we cannot fail to be struck by the rapidity and completeness of the cures effected by cold affusion or immersion, when used sufficiently early. The disease appears to have been suddenly checked or destroyed. In the course of a few hours, or a day or two, a patient threatened with, or labouring under, a dangerous fever, was restored to perfect health. No period of debility ensued, no organs were found to have been seriously or permanently injured. The result of his well-known treatment, by cold bathing, of the fever which appeared in the 30th regiment is thus described :



"These means were successful in arresting the epidemic; after the 13th of June no person was attacked by it. It extended to fifty-eight persons in all, of which thirty-two went through the regular course of the fever, *and in twenty-six the disease seemed to be cut short by the cold affusion.* Of the thirty-two already mentioned two died. Both of these were men whose constitutions were weakened by the climate of the West Indies; *both of them had been bled in the early stages of the fever*; and one of them being in the twelfth, the other in the fourteenth day of the disease, when I first visited them, neither of them was subjected to the cold affusion." (Vol. i. p. 13.)

Again:

"In cases in which the affusion was not employed till the third day of the fever, I have seen several instances of the same complete solution of the disease. I have even seen this take place when the remedy had been deferred till the fourth day; but this is not common." (Ibid. p. 23.)

In contemplating these facts, we are driven seriously to ask, not only is not the debility consecutive to fever partly occasioned by the remedies employed in its treatment, but are not its attendant local and organic lesions in great measure produced by the febrile paroxysm itself? And could they not be avoided, by boldly applying a remedy by which this febrile condition would be more speedily subdued? The real nature of fever is, unfortunately, beyond the reach of our present knowledge. We only recognize the disease in its causes, its symptoms, its complications. In them we perceive much to lead us to answer the above questions in the affirmative. It is peculiarly a general disease. Its local characters usually appear subsequently to its general development, and wear much more the aspect of consequences than of causes. Almost any of the local complications of synochus, or typhus, may appear in exanthematous fevers where they cannot be causes.

It appears to us to be a most important subject of inquiry whether a very serious fallacy does not pervade the medical profession at present as to the best manner of employing cold water in fever. Dr. Currie says:

"When the affusion of water, cold or tepid, is not employed in fever, benefit may be derived, as has already been mentioned, though in an inferior degree, by sponging or wetting the body with cold or warm vinegar or water. This application is, however, to be regulated, like the others, by the actual state of the patient's heat and of his sensations. *According to my experience, it is not only less effectual, but in many cases less safe; for the system will often bear a sudden, a general, and a stimulating application of cold, when it shrinks from its slow and successive application.*" (Vol. i, p. 73.)

"It is evident De Haen was not regulated, in his use of external ablution with cold water, by rules similar to those which I have ventured to lay down from several years' experience. Instead of pouring water over the naked body, *he applied sponges soaked in cold water to every part of the surface in succession for some time together, in my judgment the least efficacious, as well as the most hazardous, manner of using the remedy.*" (Ibid. p. 84, note.)

This is a remark which we suspect to be of very great importance, and to contain the real secret of much of the difference, as to the treatment of fever, between hydropathists and the regular faculty. Modern physicians have professed to regard Dr. Currie as a very high authority on this point, and his work is constantly quoted as the most enlightened guide for the use of water in fever; but the above opinion and precept have been, of

late years, entirely disregarded, and the converse has been made the rule of practice. In the article on Bathing, in the Cyclopædia, formerly referred to, the author says :

"The only cases in which refrigeration is required as a remedy are those in which the animal temperature is elevated above the natural standard ; and this happens only in *febrile diseases*. To ensure refrigeration, the water should be applied *at first only a little below the temperature of the skin*, its heat being insensibly and gradually reduced, *but never below that of tepid, or, at most cool*. The *gentlest mode of applying it is the best, as with a soft sponge* ; and the process should be persevered in, without interruption, until the desired effect is produced." (Art. Bathing, Cyclopædia of Practical Medicine.)

We believe this mode of applying water in the treatment of febrile diseases to be that which has for many years generally prevailed, not from ignorance of the precepts and practice of Dr. Currie, but from a general belief that fever once formed could not be extinguished by the cold affusion as recommended by him. The hydropathists have renewed his system in its full boldness. It is, therefore, a question of the first interest, on which side does reason preponderate ?

On carefully examining the cases of fever reported at length by Currie and Lanzani, it will be seen that their cures were effected by what may be termed a process of reaction. The immediate consequence, in most cases, of the copious libation "*ultra satietatem*" of the one, and the affusion, or immersion, of the other, were perspiration and sleep. These constituted the reaction. When exacerbation of the fever ensued, and required a repetition of the remedy, it occurred several hours after the cold application, when the period of reaction had long passed over ; and evidently proceeded, not from the consequences of the cold treatment, but from the non-removal of the diseased action. The cold appears to have acted in a most decidedly *medical* manner, with a palpable and immediate succession of consequences altogether different from what the gradual coolness of the sponge and tepid water can be expected to produce. If these cases are correctly stated, as they appear to be, it is preposterous to confound the febrile paroxysm with reaction from a cold bath, or to expect any portion of the beneficial effect of cold immersion in fever, from tepid or cool sponging. The two kinds of treatment are in no measure similar.

But it may be supposed there is a danger in the sudden and active employment of the cold bath in fever. We suspect that this is entirely imaginary. Dr. Currie was certainly very bold in its administration, and had extensive experience of its effects. In the second edition of his work he says :

"I have thus related all the instances which have occurred to me since the last edition of this volume (a period of five years of extensive and attentive observation), in which the affusion of water on the surface of the body, cold or tepid, proved either less beneficial in its effects in fever than I had formerly represented it, or entirely unsuccessful. I would add, if any such had occurred, the instances in which this remedy had appeared to be injurious. But experience has suggested to me no instance of the kind, and extensive as my employment of the affusion has been, *I have never heard that it has suggested, even to the fears or prejudices of others, a single occasion of imputing injury to the remedy.*" (Vol. ii, p. 25.)

This statement, which does not appear to have been assailed, goes far towards proving the innocence, as his numerous cases do the curative powers, of reaction in the treatment of fever. We certainly cannot quarrel with hydropathists for seeking to revive, in its real character, a method supported by so high an authority.

8. It is scarcely necessary to remark that a judicious system of cold bathing is a valuable tonic. This has been always known; but it has not been so widely recognized in practice as in doctrine. It has been thought necessary that cases for cold bathing should be carefully selected; that they should consist only of such patients as have unimpaired constitutions; that certain diseases were absolute contraindications against the use of this remedy; that it is a treatment requiring unquestionable vigour in the patient, and skill in the physician to employ it without injury. It is scarcely too much to say it has been regarded as a treatment rather for the strong than the weak, and as tending rather to reduce than augment the powers of the system—and yet it is called a tonic. This is an illogical paradox not quite solitary in medical literature. The cold bath seems to be professionally employed to strengthen the body, as temptation is to strengthen virtue, by furnishing an enemy to struggle against. Thus it is considered more as a test than as a source of strength.

The hydropathists have discarded this excessive precaution, and boldly used their remedy as a tonic, wherever a tonic is required. They have administered it to the young and the old, the weak, the bilious, the gouty, the scrofulous, the dyspeptic, and the paralytic. Neither mucous membranes nor mesenteric glands, infantile weakness, nor senile decrepitude have stood in their way. To almost all cases, all ages, and all constitutions, their method has been applied. Unless it can be shown that this all but universal administration of the system has produced serious evils, we are actually driven to admit that it is in the same proportion safe. And we are bound to admit—though we have known some instances where the practice has been seriously injurious, and have heard of others of a similar kind—that the proportion of bad consequences has not appeared to us greater than in the ordinary modes of treating similar diseases. The practice of the hydropathists is so open, and their disciples so numerous, that the innocence of their proceedings may be said to be established by the absence of evidence to the contrary. We cannot enter any circle of society without encountering some follower of this method, ready to narrate a series of *psuchrolousian* miracles, prepared to defend, and zealous to applaud the Preissnitzian practice; but few or none come forward with satisfactory evidence of anything like general mischief having resulted from its general practice. Judgment must, therefore, be entered by default against its opponents, and hydropathy is entitled to the verdict of harmlessness, since cause has never been shown to the contrary.

But not only have hydropathists despised the discrimination usually employed in the selection of cases for cold bathing, they have manifested an equal apparent boldness in the manner of using it. In place of the spongings and the dribblings to which ordinary practitioners commonly deem it prudent to limit the use of this remedy, they employ active plunging and powerful douches. Perhaps it is to this that they owe some portion of the impunity with which they appear to have applied it so gene-

rally. They assert that the more violent practice is really the more safe, and that the danger to be apprehended is in proportion to the supposed mildness of the process, sponging being less safe than total immersion, and a shower-bath more dangerous than a douche. And assuredly theory, in this respect, goes with them to some extent. In the plunge there is a sudden shock, which awakens nervous energy, and leads to speedy and effectual reaction; whereas, in sponging, the whole surface is exposed to a gradual and powerful cooling, without the protection of stimulus. In the former, the whole frame is at once covered with water, and shielded from the reducing evaporation which would attend the latter. Moreover, the plunge can be more speedily gone through, and followed up more immediately by exercise. The same distinction may be made between the hydropathic douche and the orthodox shower-bath. The force of the latter falls almost exclusively on the head and shoulders, as it merely trickles down the rest of the frame. How different is this from the powerful impulse of the douche upon all the muscular parts!

9. Another conspicuous item in the catalogue of hydropathic machinery is the *sweating process*. On this subject hydropathists are, in some measure, divided. It is said that Preissnitz has considerably modified his views respecting its efficacy and its safety. In the earlier period of his practice he seems to have employed it in nearly all cases. More recently he is said to have discarded it, as a general remedy, in favour of packing in the wet sheet, though still largely applying it in cases to which his matured experience has taught him to regard it as especially beneficial. We wish here to direct attention to it merely in a physiological and pathological point of view, and need not, therefore, enter into the question as to the relative value of the past and present practice of Preissnitz.

The skin is a part through which nature has arranged that a large amount of matter should be removed from the body during health, and a still larger amount, of different character, in the process of recovery from many diseases. It is well known that a deficient cutaneous excretion is incompatible with perfect health. Perhaps there is scarcely any disease in which the function of the skin is not, to some extent, deranged. To what extent, physicians have not bestowed sufficient pains to learn; nor have they been accustomed to give much attention to this part, in the practical investigation of diseases. Still less has it acquired an important position in the list of parts to which medical treatment is directed. Therefore, we possess little information, in medical writers, as to the amount or frequency of cutaneous disturbance in general disease, as to the effect of therapeutic means in correcting such disturbance, or as to the value of the correction in the cure of disease.

According to Preissnitzian writers, in almost all cases of indigestion, gout, rheumatism, nervous affections, indeed of chronic disorders in general, the action of the skin is either deficient or depraved, the part itself being found dry, hard, rough, thick, pale, relaxed, or in some other manner unnatural. They farther tell us that a course of perspiration, or of the wet sheet, followed by cold bathing, corrects these signs of disorder, and reduces the part to its normal condition; and that the beneficial influence of the remedy is speedily manifested in the improvement of the case in other respects. But it might be expected that such a course would, at

least, reduce the general strength, and require more vigour of constitution than many such patients possess. And yet, if we may believe the hydropathists, or even their patients, a course of active hydropathic sweating is found to strengthen, instead of weakening, the system. There is a gain, instead of a loss, of weight under its operation. Whether this be attributable to the subsequent cold bathing, to the water drinking, or to the peculiar regimen, may be a matter of question; but the fact would seem to be too notorious to be contradicted. We are told that it is no infrequent occurrence at hydropathic establishments for the liquid perspiration to be streaming on the floor, having penetrated through the material on which the patient is reclining, as well as the blanket in which he is wrapped! The blanket also, when removed from the person, is dripping with liquid in all directions, as if itself just removed from the bath! On these occasions several pounds of matter must be removed from the body. The patient, dripping and steaming, next hastens into the plunge bath, stays there his appointed time, undergoes the prescribed friction, drinks his water, and finds himself actually invigorated by the strange process he has undergone!

Nay, more: it is placed beyond doubt, by experience, that this proceeding may be repeated daily, or even twice a day, for *many months*, without producing any deleterious effect upon the general health? Many cases have occurred in which it has been ascertained that it has been attended with an increase of weight, and that of no slight amount. We know the particulars of one case, in which a gouty gentleman gained seven pounds in a fortnight of such treatment; and of another, in which there was a gain of eight pounds in ten days. We are also acquainted with the case of a lady who was unable to walk at any other period of the day, except immediately after the sweating process—a sure proof that it did not occasion debility.

The safety of the immediate succession of cold bathing upon copious sweating has been called in question; but the practice of so many hydropathists as there are around us amply establishes this point. On scientific grounds the question was completely set at rest by Dr. Currie.

An effective and innocuous means of increasing the excretion from the skin being thus found, which appears to combine with its own peculiar action the indirect effect of a tonic, have we not reason to regard it as a promising instrument of cure, in many disordered states of the system? We fully believe that we have. We know the utility of augmenting the secretion of the mucous membranes, the liver, the kidneys; we recognize this in our constant practice. It is by this means that we combat a large proportion of chronic as well as acute maladies. Why should the skin alone be neglected? Physiology teaches us that it is the vehicle for conveying out of the system a large amount of matter, as well solid as liquid; and practical experience exhibits it as the channel through which the *materies morbi* in many instances, and the burthen of plethora generally, find their exit. These facts indicate it as a legitimate locality for the same artificial measures which are found serviceable on other secreting organs.

It may be objected to what we are now urging, that profuse perspiration itself characterizes many diseases, of which it is one of the most formid-



able symptoms. How can sweating cure acute rheumatism, it may be asked, of which it is almost a constant feature? But the same remark applies to other medical phenomena. Excessive purging and increased action of the kidneys are dangerous, frequently mortal, symptoms. But does that prevent our employing them as remedies? Do we not, in spite of our frequent experience of their injurious effects, apply them almost constantly to the cure of disease? Are there ten cases out of ten thousand in which some kind of purgatives are not administered? Nay, is not dysentery itself treated by purgative calomel? Let us extend the same tolerance to sweating. It is contrary to all the instruction of experience to confound the consequences of a phenomenon violently excited by morbid causes, with those it induces when seasonably created, and carefully managed, by skilful treatment.

In many of these cases, the benefit does not appear to result so much from stimulating the function of any particular organ, as from removing a certain portion of matter from the system at large. There is no reason to suppose that exciting the liver, the colon, the duodenum, or the kidneys, for instance, has any special influence over a morbid condition of the brain. We find that drugs which act upon any of these organs, frequently relieve such conditions, and they may often be selected indiscriminately, the one answering much the same purpose as the others. A common antibilious pill, retailed for a penny by a druggist, or a patented nostrum of Cockle or Morrison, will generally do as well as the most elaborate prescription. The particular adaptation seems to depend more on constitutional idiosyncrasy than on any fixed relation of the part diseased with the part treated. The whole of those remedies appear to act in such cases, either by a general principle of counter-irritation, or by removing a quantum of fluid, or of excretory matter, from the circulation, either of which object might be attained as speedily, as certainly, as extensively, and as safely, by the skin as by any other part.

10. But the power of the water-cure over excretions is not limited to the skin. It professes to be both a purgative and a diuretic. That it is diuretic, in a certain sense, needs no proof. It is no new discovery that, in proportion to the quantity of fluid imbibed by the mouth will be the quantity emitted by the kidneys. This, though verbally, is not medically a diuretic action. It may consist simply in the mechanical discharge of the fluid imbibed, with no augmentation of the proper functions of the kidneys, as respects the previous condition of the blood-vessels. But it is not perhaps unphilosophical to give hydropathists the benefit of supposing that water-drinking may do indirectly what it does not appear to do directly: by its diluting power, may it not destroy the influence of any mischievous constituent of the blood, the excess of dilution being immediately repaired by the removal of the water through the kidneys, in company with the deleterious matter dissolved in it? This view might be admitted, if it could be shown, practically, that drinking water has the same effect on disease as taking diuretics.

The purgative action of hydropathy is less equivocal. It frequently happens, in cases of constipation, that, after a few days' or weeks' use of its appliances, the patient is attacked with diarrhea. This is sometimes



troublesome, but we believe seldom dangerous. On its subsiding, the bowels are said to have generally acquired a regular and healthy action, which is thenceforward maintained by persevering in the drinking, bathing, &c. In other cases, a regular action of the bowels comes on in a gradual manner, without the occurrence of diarrhea, the treatment appearing to influence the bowels through its action on the system at large. In others, and every one has seen examples of this, the mere drinking of a few glasses of water before breakfast is represented as a purgative that may be relied on. In these the daily dose is regulated according to circumstances, being increased when signs of torpidity are observed. We are ourselves acquainted with some persons who regulate this function as accurately by water-drinking as they formerly did by medicinal aperients. There are cases, again, in which the sitz-bath, or other external applications of cold water, produce a purgative effect.

It may be asked, is not this effect too uniform for the purpose of the practical physician? Does it not often result from the mere percolation of water through the mucous lining of the intestinal canal? Is it not, therefore, a mere pouring out of what has been swallowed? Is it not clearly inadequate to excite the particular action of the liver, the pancreas, the lower or upper portion of the intestinal tube? Is it not necessary that we should be able to act on these parts separately, for the effectual cure of disease? These questions, important as they appear, may with equal justice be asked as to the practical proceedings of our profession in general. It is true that, in theory, many nice distinctions are laid down respecting the peculiar operation, as to locality or otherwise, of different cathartics. But are these distinctions generally observed in practice? Did not Abernethy's page 72 contain the curative maxim for all cases? and were not his prescriptions almost always identical? Has not every respectable family doctor his "my pills," carefully prepared from the same ingredients for every difficulty in the bowels? Is not the black draught as universal a purgative as Preissnitz would make cold water? Are not all our moneyed dyspeptics and hypochondriac nabobs sent in a body to mineral springs, because they are purgative, without any preliminary investigation as to their action on the duodenum or the colon, the liver or the pancreas, or as to the expediency of such action in the individual case in question?

11. We observe, also, in the history of hydropathic practice, the development of a peculiar sedative or tranquillising influence. It is well illustrated in the following passage from Mr. Mayo's preface:

"Through repeated attacks of a sort of rheumatism my constitution appeared completely broken down. Already crippled in my limbs, *preserving what power of exertion I still retained only through the use of opium*, and my indisposition still increasing, I looked forward to being before long worn out with suffering—as to death, as a release. I could not bear the fatigue of a land journey, or I should have gone at once to Graeffenberg; but Coblenz and Boppard might be reached from London by water—so I went to Marienburg in June, 1842. On arriving there I was placed on a routine system of sweating and bathing. The immediate effect on my health was strikingly beneficial, *and in a week I was able to relinquish the use of opium*. The rheumatism did not, however, give way proportionably to my general improvement. The pains of the joints were, indeed heightened." (p. 1.)

This was a painfully severe case, one in which every conceivable remedy had been previously tried, not excepting repeated change of air, the Bath-waters, &c.; yet nothing had succeeded in relieving the system from the necessity of constantly using opium. A "routine system of sweating and bathing" was applied, and in a week the patient was able to relinquish his doses of opium, notwithstanding that the rheumatism did not give way; indeed, the pains in the joints increased. How is this to be explained? only by supposing that, independent of any curative influence over the actual disease, the water-cure exercised some sort of sedative action on the system at large. Similar instances are said to be familiar at hydropathic establishments. If these accounts may be depended on, hydropathy would appear to contain, in its armamentarium even an anodyne, and one of great power. Every practitioner knows the difficulty presented in the treatment of chronic cases, by morbid irritability, and painful nervous sensations, which are not only intolerable to the patient himself, but most prejudicial to his recovery; and which can only be relieved, from time to time, by repeated and gradually augmented doses of a drug, whose own effects are almost as pernicious as the symptoms it is used to palliate. This is one instance of a predicament in which the physician is not infrequently placed, when he has most gravely to consider whether there is most mischief in the disease to be combated, or in the only remedy by which it can be encountered. If "a routine system of sweating and bathing" affords a means of extrication from the present instance of this difficulty, this is a strong reason why it should not continue to be obstinately excluded from the well-fenced pale of the medical profession.

12. In addition to the effects already considered, and which have occupied as much as can be spared of our space, the water-cure pretends to the possession of other important powers. Thus, it is said to be a *stomachic*, since it almost invariably increases the appetite. It is a local *calefacient*, in the application of the wet cloth covered by the dry one. It is a *derivative*, cold friction at one part, by exciting increased action there, producing corresponding diminution elsewhere. It is a local as well as general *counter-irritant*, the compress frequently acting, if not like a blister, at least, like a mustard poultice. It is essentially *alterative* in the continued removal of old matter by sweating, and its renewal as shown in the maintenance of the same weight.

13. Lastly, our subject brings us to make a few remarks on *medical habits* in reference to chronic cases. In such cases, we have only commenced the treatment, when we have removed the immediate symptoms; the real difficulty consists in preventing their recurrence. Accordingly, the patient quits his physician with ample instructions for his future guidance, and with most impressive warnings as to perseverance in their observance. What are these instructions, and to what habits do they lead? Let us take a case of "biliousness" or chronic dyspepsy, and briefly trace the history of its "legitimate" treatment, according to the heroic school of London.

In addition to constipation, the patient, we shall suppose, is affected with acidity, deficient or depraved appetite, foul tongue, oppression after meals, susceptibility to cold, debility, headache, despondency, irritability of temper,

inconstancy of purpose, hopelessness of relief, with divers local grievances. A few brisk cathartic doses, combined with mutton diet, and a gentle stimulant, empty the bowels, and carry off most of the attendant ills. By continuing this plan for a short time the patient is, what is medically termed, cured; but, for future protection, he is furnished with a prescription—say—of aloes, colocynth, and calomel, or some such compound, to take *pro re nata*; another of senna and salts, to take less frequently, as more urgent symptoms require; a third of calumba, gentian, or cinchona, to take at noon with a glass of sherry. He is told to live on boiled mutton, rice, and dry bread, avoiding fruit and vegetables.

What future, as respects health, has such a person before him? As long as he lives he will be a martyr to the disease, probably in an increasing degree; he must abandon all hope of the action of the bowels ever resuming its normal state; his general strength will gradually diminish; his nervous system will become more and more irritable; his whole comfort and enjoyment will be sacrificed in order to empty the alimentary canal; he will become one of the most pitiable of all sufferers, a person “living by rule;” his health will be supported, as one of our witty doctors remarks, like a shuttlecock between two battledoors, by the alternate impulse of senna and sherry, of calomel and coffee, of jalap and gentian. As long as these instruments are so directed, that their respective influences succeed each other in compensating proportion, all seems, for the time, smooth; but let either overdo or underdo the mark, and every thing breaks down. The game must then be commenced anew, to be continued as long as feather and cork resist the tendency which it has to knock them to pieces.

This is scarcely a caricatured picture of the discipline to which dyspeptic patients are often forced to submit. Everybody’s experience must furnish abundant proof that the illustration is too close to nature. It is in the latter stages of these affections, when the patients have long been under the influence of therapeutic means, that Preissnitz pronounces them “drug-diseases.” If, by this term, he means that drugs constitute the whole disease, then he is no doubt wrong; yet, in one point of view, he is right. The original complaint for which the drugs were administered might, very probably, have been one requiring some artificial remedy, and which would have induced more serious consequences, had not some such remedy been employed. But it is quite possible that a persevering use of such remedies may create a train of symptoms, in addition to those which existed before, and induce such a host of wants as may constitute a prominent feature of the case, by the time it is submitted to the curative process of such a practitioner as Preissnitz; therefore his term, drug-disease, may not be altogether inapplicable.

But what is often the result of placing the cases, now under consideration, in a hydropathic establishment? Precisely such as might be expected from the abandonment of a pernicious custom, and the adoption, at the same time, of a more natural mode of life with healthier and hardier habits; and with the immense additional mental stimulus of cheerfulness, of faith and hope in the new system, and of unbounded confidence in the

new doctor. It is accordingly the general report that, in a large proportion of such cases, the patients are enabled immediately to discontinue the use of purgative medicines; they can bear a mixed animal and vegetable diet, in the ordinary proportion; a regular action of the bowels is shortly acquired, and no further stimulant or pharmaceutical tonic is necessary. When they quit the establishment, formal and complex means being no longer required, we are assured that they are able, for a time, at least, to maintain the ground gained, simply by common-sense diet, drinking a few glasses of water in the morning, taking a daily cold-bath, and persevering in their habitual exercise. The country rings with such accounts as these; if they are correct, undoubtedly, the patients are in a fair way of recovering their lost health and strength, and are pursuing, subsequently to systematic treatment, a much more rational and scientific course of medical habits, than that enjoined to the dyspeptic disciple of medical orthodoxy.

The questions, with which we set out, may now be hypothetically answered: they were, "Does hydropathy furnish the physician with instruments which he, as a skilful workman, can undertake to employ? Does it contain, among its various machinery, any really therapeutic means, any properties capable of carrying out the indications which we regard as palpable in many diseases?" These questions, we think, may be allowed to have been answered in the affirmative, if we may depend on the results of our own limited experience; they must be allowed to be so answered, and unequivocally, if we may admit as perfectly trustworthy, the accounts published by the hydropathists themselves, and by those who have subjected themselves to the treatment. On another occasion we may, perhaps, endeavour to sift this evidence in a more rigid manner, in order to ascertain, with certainty, what in it is true, what false, what doubtful, and what inapplicable. But in any inquiry we may institute, we must continue to examine the water-cure relatively to other modes of practice: this is the only method of arriving at an estimate of its actual value to the practical physician. The imperfections which it shares in common with ordinary treatment, and which are inseparable from all human performances, may be left entirely out of sight; to dwell on them, would be uselessly to encumber the question, like inserting a crowd of corresponding items upon both sides of an equation. The philosopher's duty is to remove such superfluities, in order that the real problem may appear in a just and intelligible form.

In conclusion, we will venture to place on record the following, as among the more important impressions which have remained on our mind after a careful examination of the whole subject:

1. We should be glad to see Dr. Currie's practice revived (for the sake of experiment, at least,) in all its boldness, for the suppression of the general febrile paroxysm. On carefully looking over the evidence published by Dr. Currie and his contemporaries, it is impossible to deny that they attained a larger amount of success in treating fever by water than other practitioners have done by other means. We have already pointed

out how their practice has been misunderstood by modern writers. But, while we regard this practice as well adapted for treating general fever, we find no proof that it is competent to meet the dangerous local complications with which fever is so often accompanied. These complications may reasonably be expected less frequently, when the early treatment of fever is rendered more efficacious. But, when they do occur, we find nothing in hydropathic writers to show that lancets, leeches, blisters, &c., can be dispensed with.

2. In a large proportion of cases of gout and rheumatism the water-cure seems to be extremely efficacious. After the evidence in its favor accessible to every body, we think medical men can hardly be justified in omitting—in a certain proportion of cases, at least—a full trial of it. No evidence exists of any special risk from the water-practice in such cases.

3. In that very large class of cases of complex disease, usually known under the name of chronic dyspepsia, in which other modes of treatment have failed or been only partially successful, the practice of Preissnitz is well deserving of trial.

4. In many chronic nervous affections and general debility we should anticipate great benefits from this system.

5. In chronic diarrhea, dysentery, and hemorrhoids, the sitz-bath appears to be frequently an effectual remedy.

6. We find nothing to forbid a cautious use of drugs in combination with hydropathic measures. On the contrary, we are convinced that a judicious combination of the two is the best means of obtaining the full benefit of each. The water-cure contains no substitute for the lancet, active purging, and many other means necessary for the relief of sudden and dangerous local maladies. The banishment of drugs from his practice was necessary, and perhaps natural, on the part of Preissnitz: the like proceeding on the part of qualified medical men superintending water-establishments in this country, evinces ignorance or charlatanry, or both.

7. With careful and discreet management, in the hands of a properly qualified medical practitioner, the water-cure is very rarely attended with danger.

8. Many of the principal advantages of hydropathy may be obtained in a private residence, with the assistance of ordinary moveable baths. Therefore, it can easily be brought under the direction of the regular medical practitioner.

9. In many cases, however, it is evident that what may be termed the mere *accessories* of the water-cure, are of extreme importance in bringing about a favorable result; and these accessories are frequently not available—or available in a very inferior degree—in ordinary practice. Among the more important of these accessories we may mention the following as having relation to most of the chronic cases treated in hydropathic establishments: 1, relief from mental labors of an exhausting or irritating kind, from the anxieties and responsibilities of business, from domestic irritations of various kinds, from mental inaction or ennui, &c.; 2, change of locality, air, scene, society, diet, &c.; 3, the fresh mental sti-

mulus involved in the almost constant occupation of the patient's time, in the performance of the numerous and various dabbings, paddlings, sweatings, washings, drinkings, rubbings, &c., imposed by the water treatment; 4, the frequent and regular bodily exercise taken in the open air, or within doors; 5, the powerful mental stimulus supplied by the confidence generally reposed by the patients in the means employed, and by the consequent hope, alacrity, cheerfulness, &c.; 6, the total abandonment of vinous and other stimulants, and of drugs,—all of which have, in a large proportion of cases, been tried and found, not only useless, but probably, productive of disadvantage.

10. A certain and not inconsiderable portion of the benefits derived from hydropathic establishments, are, however, attainable without them, by other means, as by travelling, &c. &c. For example, we suspect that many of the most striking results witnessed in such establishments, as in the case of Sir Edward Bulwer Lytton or Mr. Lane, would have probably been obtained, if the patients had chosen to hire themselves, and had worked as agricultural laborers, in a dry, healthy district, and had lived on agricultural fare, sufficiently nutritious in quantity and kind, for a sufficient length of time.

11. Notwithstanding the success of the founder of hydropathy, its practice by non-professional persons can neither be fully advantageous nor safe. At the same time, it is true that very little experience is necessary to enable an educated medical man to acquire sufficient insight into it for purposes of practice. Many of the best hydropathic physicians have, in the first instance, devoted very few weeks to studying the subject in Germany.

12. Many advantages would result from the subject being taken up by the medical profession. The evils and dangers of quackery would at once be removed from it. Its real merits would soon be known. The tonic portion of its measures might then be employed in conjunction with special remedies of more activity, which, no doubt, would often prove exceedingly beneficial.

13. The benefits ascribed to hydropathy, but arising indirectly from the abandonment of drugs, vinous and other stimulants, &c., may certainly be obtained without sending patients to Graeffenberg.

14. Finally, it must always be remembered that the distinction between quacks and respectable practitioners is one, not so much of remedies used, as of skill and honesty in using them. Therefore, let our orthodox brethren be especially anxious to establish and to widen, as far as possible, this distinction between themselves and all spurious pretenders. "*Artem medicam denique videmus, si à naturali philosophia destituatur, empiricorum praxi haud multum præstare. Medicina in philosophia non fundata, res infirma est.*"



## ART. XI.

*Redogörelse för Sjukvården a Kongl. Seraphimer Lazarettets Afdelning för Invertes Sjuke, under Loppet af Ar 1842.*

*Medical Report of the Internal Diseases treated in the Royal Seraphim Hospital in Stockholm, during the Year 1842. By Dr. MAGNUS HUSS. —Stockholm, 1844. 8vo, pp. 228.*

WE had imagined, on reading the title of this book, that it would be mere hospital report, full of long details of cases, but with little attempt at generalization, and, consequently, merely valuable as an addition to medical statistics. From the charge—often well-founded—of deficient knowledge of contemporary medical literature and science, we knew our author would be perfectly free; but we were not prepared to find, under the modest name of an Annual Report, such elaborate disquisitions on the origin and nature of some of the most obscure diseases;—and these written, too, with a candour and honesty of purpose, which, in our own insular pride, we rarely expect to meet with beyond the British seas.

Before examining this Report in detail, we may premise that it is only a portion or a single part of an elaborate series, several volumes of which are already before the Swedish public, though they are as yet unnoticed in our British Journals. Unfortunately these preceding volumes are not as yet in our possession, and this is the more to be regretted, as several maladies and classes of diseases which, to judge from the number of cases briefly alluded to in this volume, are of very frequent occurrence in the Swedish capital, obtain in this Report only the mere notice of their number, while, for details of their peculiarities, and for the author's opinions of their nature and treatment, we are referred to the preceding volumes.

In this Report of 1842, the various diseases that affect the human frame are considered successively under the following heads:

1. Diseases of the nervous system.
2.       “       respiratory organs.
3.       “       organs of circulation.
4.       “       lymphatic system.
5.       “       digestive organs.
6.       “       liver and of the spleen.
7.       “       urinary and genital organs.
8.       “       organs of locomotion.
9.       “       skin.
10. Mental disorders.
11. Cases of poisoning.

In the clinical wards of the Swedish hospital the most important diseases are necessarily selected for the instruction of the pupils, but those also of less intensity but of more frequent occurrence, are admitted to familiarize the student with the aspect of maladies most common in his future practice. During the year 1842 seventeen students were admitted as clerks in the wards (*tjenstgörande*), and to each of these clerks were allotted, in the space of nine months, 35 cases of disease, of all of which

they were obliged to keep a full and complete journal for the inspection of the clinical professor. Besides the clinical observations delivered by the professor at the bed-side, a course of special pathology is continued throughout the session in lectures which are given twice in the week; and we can estimate the comprehensive nature of this course from the fact, that the nine months of the past session were exclusively occupied by the consideration of diseases of the intestinal canal, so that two or even three years must elapse, ere all the maladies of the human frame are completely described. The mortality in the hospital, during 1842, was  $11\frac{1}{2}$  per cent.; the greatest number of patients died in the earlier months of that year; while December presented the lowest rate of deaths.

No epidemic of any consequence appeared in the capital during this year, excepting a gastric nervous fever among the police or gendarmerie, which increased the usual average of that complaint by about 60 cases.

The first division, viz. that of diseases of the nervous system, occupies a very large portion of this volume; but we do not regret to find more than one hundred pages devoted to these interesting but most obscure maladies. Our author commences with inflammation of the brain and spinal cord, and of their envelopes. The first case, which is related at great length, is one of fatal inflammation of the spinal meninges. The patient, a sailor, was able to perform his usual duties for fourteen days after the accident, before any serious symptoms supervened. No disease was discovered on dissection of the brain after death, but there was slight delirium during life, with great irritability of temper. Convulsions occurred, both in the upper and in the lower limbs, and it was necessary to use the catheter, as the bladder did not act. From the appearances recorded on the post-mortem examination, it would seem that the dura mater was the original seat of the inflammation, and that pus had exuded from thence into the neighbouring muscles on the back; though, during life, the latter phenomenon could not be distinguished from a simple abscess, the result of supposed external injuries. Dr. Huss ascribes the delirium and irritability of temper in this patient, to sympathetic irritation of the cerebral system. Death ensued in the fourth week after the accident.

The second case is not in itself remarkable, being one of simple *meningitis cerebialis*, but it was accompanied with a general emphysema of the whole trunk, which came on on the twenty-second day of the disease: no cause for this appearance could be discovered on dissection. Dr. Huss aptly compares this emphysema to that which occurs in typhoid fever of a malignant character, where the intestines become enormously distended with flatus. The cause of this symptom may, he thinks, in both cases, be traced to diminished energy of the inflamed brain, whereby the blood becomes deteriorated, and tends to separate into its constituent parts. Our author, however, evidently does not mean to infer that, in all cases of typhoid fever with great distention of the bowels, there is necessarily inflammation of the brain or of its envelopes.

In the third case of this Report (p. 9) we have an illustration of the great advantages of full doses of calomel after venesection in acute cerebral meningitis. The treatment here pursued seems to have been most energetic and sensible; it is one of those cases, where no doubt can be raised, as to the powers of medical science in arresting disease.

It is justly remarked by Dr. Huss, in reference to this case, that we must not be deterred by the apparent debility or defective nervous energy, from pursuing a steady antiphlogistic course of treatment. "It is this horror of debility," says he, "which too often paralyses the efforts of the physician, and allows the inflammation to proceed unchecked to a fatal termination." The great value of free evacuation of the bowels in cerebral disease, appears to be better known to Dr. Huss than to many of his continental brethren. "Should calomel," he says, "fail of purging, I assist its operation by purgative enemata, and by the free use of croton oil, or of turpentine." Our author has seen little benefit from the cold douche applied to the head in cerebral inflammation, but he acknowledges its marked efficacy in the delirium of typhus fever. At the same time he admits that there is great difficulty in the diagnosis between the two affections, as the blood often presents the same disintegrated condition in cerebral inflammation, as in the most malignant typhus. Blisters to the shaven scalp he unequivocally condemns; in all cases in which they have been tried, he has found them to do harm by increasing rather than diminishing the symptoms; though in the latter stages, when inflammation has subsided and effusion still remains, they may occasionally be useful. "When," he continues, "the symptoms of inflammation and congestion have subsided, and are succeeded by those indicating debility of the brain and of the nervous system, I find it best to wait a day or two, till there is an obvious indication for the use of stimulants and sedatives; I then employ camphor and arnica to fulfil the former intention; and opium seems to be best adapted for the latter. I have more than once observed, that opium in doses of one or two grains when muttering delirium succeeds to violent nervous excitement, has quickly produced refreshing sleep." English practitioners will fully appreciate the truth of this observation.

Two cases of *hydrocephalus internus*, the next disease here mentioned, are not of remarkable interest, except that the first was found to be accompanied by a large cavity in the cerebellum containing fluid, and totally distinct from that found in the ventricles. He compares this appearance to the sacculated dropsies of the ovary, and believes these cysts to be formed by a defect in development of the cells of the cerebral substance. Dr. Huss has never observed any connexion between disease of the cerebellum, and the excitement or depression of the sexual appetite.

*Apoplexy.* Of 17 cases, 12 occurred in males, and 5 in females; and nine of these patients were between their thirtieth and fortieth year, which is surely a greater proportion than that met with at the same period of life in this country. Of these 17 cases, 8 proved fatal; in the preceding year only 4 died out of 15.

Our author has seldom found blisters or irritant applications to the scalp of any use after apoplexy, and the possible benefit they may confer is, he thinks, more than counterbalanced by the annoyance they occasion to the patient.

Dr. Huss does not believe that, when the power of speech is entirely lost in apoplexy, the effusion must necessarily have taken place in the anterior lobes of the brain; he has seen speech wanting when the posterior lobes alone were the seat of the maladies. In case 8 the apoplectic effusion occurred in the posterior lobes of the left side, and the power of

speech was entirely absent during the six months that the patient survived the attack ; but we must observe that the left hypoglossal nerve was found to be only half the size of that upon the opposite side.

Our author is of opinion that, the larger the clot and effusion of blood in apoplexy, the greater will be the loss of motor power, but he acknowledges that the same does not hold good in regard to the seat of the lesion ; but, he adds, it is well known that the case is different in apoplexy occurring at the base of the brain, where a very small effusion causes great loss of nervous power.

At page 30 of the Report we find an interesting case exceedingly well detailed, which, during life, was supposed to be apoplectic effusion into the pons varolii. There was paralysis of both sides of the trunk, with partial loss of feeling, but the intelligence remained perfect. After death an effusion of blood was found in the left crus cerebri, and another of like character in the corpus striatum of that side. Dr. Huss owns that he dares not venture on any explanation of the double paralysis in this case.

Before concluding this portion of our subject we may observe that our author throws out a suggestion, that an intimate connexion will, one day or other, be discovered between lesion of the corpora striata and optic thalami, and the functions of the urinary and genital organs.

Under the title of, *Organic lesions of the brain* (p. 33), we find a case of supposed softening of that organ, with paralysis of the right side, spontaneous movements of the whole of the extremities, and loss of feeling in the fingers and toes. Two grains of opium divided into four doses were given daily, with ten grains of the "sal cornu cervi," by which combination our author believes that the narcotic properties of the opium are diminished, leaving its sedative qualities untouched. Dr. Huss has found opium very useful in cases of suspected softening of the brain.

"I have constantly observed," he says, "that when in this complaint the most prominent symptoms were cramps of the limbs, or tremors, or both of these symptoms together, opium was the most efficacious remedy, though I cannot venture to assert that it effected a cure." (p. 37.)

*Osteoma frontalis*. In speaking of this disease and of tumours in general, pressing upon the surface of the brain, Dr. Huss observes, that it is difficult to decide *after death* what portion of the cerebral system has particularly and secondarily suffered from such pressure, though it is easy to determine what convolutions have been immediately compressed. The case of frontal osteoma here described, closely resembles those cancerous tumours described by Walther and Cruveilhier, and so beautifully figured by the latter. The pulsations of the brain could be very distinctly heard on applying the stethoscope over the tumour. How far the congested and enlarged vessels in the tumour itself may have contributed to the sound of cerebral pulsation, is a question which we leave to the consideration of our readers. It may be doubted, perhaps, in the present instance, whether we are justified in identifying the tumour described by Dr. Huss with those of a decidedly cancerous character ; for its external portion diminished, in a very marked degree, under the prolonged use of mercury and iodine ; while, at the same time, the mental powers which were before much weakened, regained their original force. The mode of

treatment was, by first blistering the tumour and then applying the unguent. hydrarg. iodid. The tumour did not entirely disappear, but Dr. Huss suspects, from the complete return of the mental faculties, that the internal portion of it, which produced all the evil symptoms, was that which was soonest and most completely removed.

**SPINAL IRRITATION.** To the consideration of this disorder Dr. Huss devotes a very considerable portion of this volume. It is indeed a malady concerning which much has been written lately, but we have seldom or never met with a more complete history of its varied symptoms than that contained in the pages of this Report. Our author complains of the numerous, varied, and complex definitions attempted of this disease; his own, however, will probably admit of dispute. He believes, "that that same condition of irritation which, physiologically speaking and under physiological influences, is transplanted from impressions made on the surface-nerves of the body to the spinal cord, here appears as a disease arising primitively from the spinal cord itself, and which is thence propagated to the external surface, or extremities of the nerves." Physiological irritation, therefore, is transmitted from without to the nervous centres, pathological irritation from the centres to the extremities of the nerves.

Dr. Huss asks, if this be not an old disease under a new name? or at least he acknowledges, that were many of the symptoms to be taken separately they have undoubtedly been before described; but the honour of uniting them all as one malady is due only to the present day. The subject of spinal irritation has been most ably discussed by English writers; indeed we may justly assume to ourselves much of the light that has been recently thrown on this obscure disease. The labours of the Messrs. Griffin, of Limerick, in this regard, are pleasingly acknowledged by Dr. Huss, who seems also to be well acquainted with the investigations of Dr. Marshall Hall, of Dr. Copland, and of Mr. Travers, as to the connexion of reflex action with the phenomena of the disease in question. Nor have our German brethren been slow to pursue a subject so congenial to their laborious and speculative minds. Our author has, we think, steered a middle course, a thoroughly practical one, between the mazes of German speculation and the hasty conclusions too often arrived at by the clever writers of France. We do not think we say too much when we state his views and practice here will be found as consonant to English feelings and to English medicine, as is the construction of the beautiful language of Sweden to that of our mother country.

Dr. Huss objects to the generally received definition of spinal irritation, viz. that it is an alteration in the *functions* of the spinal cord, independent of any organic change in that part; for he observes, that this merely expresses the condition and not the original cause of the malady; pathological anatomy, he acknowledges, has taught us but little regarding its origin, and that little merely of a negative character, viz. that the same phenomena which are developed in organic disease of the spinal cord, may also appear during life, when no lesion or change of structure can be discovered on dissection.

The symptoms of spinal irritation are classed by our author under the following heads:

1. Pain of various parts of the vertebral column, existing either idiopathically or developed by pressure. 2. Cramps, either of a tonic or of a clonic nature, in those parts subjected to the influence of the spinal cord. 3. Loss of power in the same portions of the body, varying from simple stiffness and weakness to complete paralysis. 4. Altered sensibility, either by excess or by great diminution of sensation.

Of all these phenomena the varieties seem to be almost endless; and not less remarkable is the constant variation of symptoms in the same individual. We will, however, briefly consider each of these divisions as here made out more in detail.

1. Dr. Huss believes that the pain developed by pressure on the vertebræ is of more importance, in a diagnostic point of view, than that experienced by the patient without such manipulation; but he differs from some of our recent writers, when he asserts that it is only the feeling of pain developed by *firm* pressure that is of importance. With the Dr. Griffin he agrees that pain *above* the vertebræ, subjected to pressure, is rarely observed. It is almost always felt in those organs or parts of the frame that correspond in position to the compressed vertebræ; or, if not there, it occurs in more distant parts lying *below* the seat of pressure. But Dr. Huss does not maintain that tenderness on pressure of the vertebræ must necessarily exist in spinal irritation. On the contrary, it is occasionally absent; and its value as a diagnostic sign is considerably diminished by its occurring, with a similar train of symptoms, in organic disease of the spine itself. As to the immediate cause and nature of this symptom, Dr. Huss rejects alike the theory of its being occasioned by pressure on the spinal cord through the medium of the vertebræ, which is anatomically impossible, as also the doctrine of Stilling and of Hirsch, who believe the seat of the pain to be in the muscles, skin, and ligaments interposed between the vertebræ and the hand of the compressor. Our author finds no other means of explaining this symptom than by supposing that there exists in such cases a neuralgia of the vertebræ themselves. This theory may perhaps suffice in the present state of our knowledge to account for such intense pain being felt on firm pressure of the vertebræ; nor do we think it inadequate to explain those cases where the slightest touch occasions agonizing torture, and even convulsions. The theory of a neuralgic condition of the sensitive surface-nerves of the back must be also allowed to carry with it a considerable semblance of truth.

2. "Phenomena resembling cramp," &c. From the simplest attacks of cramp these symptoms may pass into severe convulsive movements of the extremities, closely resembling those of chorea; which disease our author considers to be spinal irritation, in its most complete condition, with clonic convulsive movements. At other times the symptoms are rather those of tetanus; and this latter malady is, according to Dr. Huss, the type of spinal irritation in its tonic and most acute form.

3. "Diminished power of motion." General paralysis of the motor powers sometimes occurs under this form.

4. "Altered sensibility," &c. This may be diminished, or much increased. In exalted sensibility, accompanying spinal irritation, the cutaneous nerves are often so affected in very limited portions of the surface;



sometimes this does not exceed the space covered by a shilling. Dr. Huss looks upon spinal neuralgia as another variety of this form. On the other hand, sensation may be totally absent from some portions of the cutaneous surface, even to impressions of the most painful nature.

Of the difficulties attending the diagnosis of spinal irritation our author speaks most feelingly; and he acknowledges that in many cases of obscure disease affecting the spinal column, it is necessary to suspend your opinion for weeks, nay, sometimes even for months, to enable you to decide on the nature of the malady, from the effects, or from the negative results, of the remedies employed.

Dr. Huss adds but little to our previous information respecting the causes of this malady, but ascribes its prevalence among the females of the higher classes in Sweden to that false system of female education, which neglects the due exercising of the body, while it overstrains the mental powers. Whatever, then, diminishes the bodily strength, as overstudy, excitement, or long and debilitating disorders, will predispose to this disease; especially in nervous and irritable constitutions. Of immediate exciting causes our author reckons the following: (a) Contusions, strains of the spinal column, from lifting of heavy weights; (b) intense mental affections, fear, sorrow, &c.; (c) lesions and irritations of the extremities of nerves springing from the spinal column, both of those distributed on the cutaneous surface, and those which terminate in internal organs, as also the presence of worms in the intestinal canal; (d) the most important and most frequent cause of spinal irritation is a disordered state of the blood itself (giving rise to the neuræmia of Dr. Laycock). As an example of this, Dr. Huss refers to the frequent occurrence of spinal irritation in chlorotic females, and to the phenomena produced by poisoning with strychnia, or *secale cornutum*, or by the introduction of lead into the system.

As regards the termination of this malady, it may persist for years without any advance towards a cure, the only change being the constant variation of the symptoms; but it may also eventually produce organic disease. Thus constant palpitation of the heart, from spinal irritation, may eventually cause the dilatation of that organ, or more frequently hypertrophy, with or without dilatation. With our English writers, Dr. Huss agrees that spinal irritation of itself is rarely fatal.

A long paragraph is devoted by our author to a review of the theories of spinal irritation. All that have hitherto been broached he rejects as doubtful and unsatisfactory, but modestly enough he does not attempt to replace them by a theory of his own. Perhaps he inclines a little towards the theory of Hirsch, whose opinions in regard to the minute anatomy of the nervous system are somewhat analogous to those of Dr. Stark, of Edinburgh. Hirsch believes the nerves to be tubes containing a fluid which, when collected in excess in any part of the nervous system, gives rise to pain, and to the other symptoms of spinal irritation; thus assimilating these phenomena closely to congestion of the blood-vessels. But, as Dr. Huss very justly adds, "until the existence of this nerve-fluid, or ether, or essence, can be distinctly proved, these doctrines may amuse but cannot instruct the scientific pathologist."

*Treatment.* External remedies should be applied in general to that

part of the spine where pain is felt on pressure. Venesection, whether local or general, should be cautiously employed in debilitated persons, where the disease, as in chlorosis, appears to be connected with deterioration of the blood; and Dr. Huss remarks that venesection alone is seldom able to effect a cure, but at the most acts only as a palliative of the disease. Of the class of derivatives, Dr. Huss employs most of those generally used in English practice, recommending always that we should begin with the milder forms, while in protracted cases he has employed moxas and the actual cautery with success. "Should the patient seem worse after using the milder derivatives, I always abstain from having recourse to those of a severer kind."

Much alleviation of the pain is often obtained by simple manipulation or shampooing of the affected parts; and our author has also seen great benefit to result from covering the spine with wool or cotton dipped in turpentine. This is kept applied to the back by means of a sheet or towel, and its moisture is preserved by continual irrigation with fresh turpentine.

This plan is, however, not without danger in highly irritable constitutions. Mercurial ointment, pushed to salivation, has proved occasionally useful in our author's hands, though no inflammatory symptoms were present. Though no disciple of Preissnitz, Dr. Huss states that he has seen favorable results from the cold douche, and from cloths steeped in cold water, and applied to the spine; but he acknowledges that in the majority of instances these remedies have proved injurious.

Electricity and electro-galvanism are contraindicated, he thinks, in all cases where plethora exists, but may be employed with advantage in chronic and debilitated cases, though the advantage resulting from their use is so slowly obtained, as to render their application most tedious to the patient. In cases of a neuralgic character, magneto-electricity combined with acupuncture caused a most marked improvement in the symptoms.

1. *General external remedies.* "Bathing," &c. The warm bath is occasionally useful, but is much more efficacious when 3j of caustic potash is diffused in the water. Perhaps, however, as our author suggests, a smaller quantity of the alkali than that above stated should be first tried, and the baths should be repeated at first every third, and afterwards every second, day. The mud-baths of Germany (*soolenbäder*) are often really valuable in cases of spinal irritation; and Dr. Huss deplors the little attention paid in general to the baths and mineral springs of his own country, which, he believes, would be found to be as efficacious as many of the celebrated German spas, if his countrymen would but be convinced that the means of cure were to be found at their own doors.

*Clothing.* Much harm is done, according to our author, by the habit of wearing excessive quantities of warm clothing. Thus he relates an instance of a lady, who suffered from spinal irritation, with constant chilliness of the back and abdomen, and who wore, besides two thick belts (*maggördlar*), not less than seven heavy shawls. A complete cure was obtained after she had gradually laid aside this excess of clothing, and had accustomed herself to sponging of the trunk with ice-cold water. A novel popular remedy is here introduced to our notice, viz. a bath in the freshly-

plucked leaves of the birch tree, until perspiration breaks out over the whole body. Such popular remedies often succeed, when medical treatment has altogether failed. In the local palsies, so common in spinal irritation, Professor Huss has freely employed the endermic method, using strychnia in preference to any other remedy.

*Internal remedies.* Of antiphlogistics our author has only used mercury and tartar emetic, and these solely in plethoric subjects. The former was occasionally useful when pushed to salivation, but in many instances it utterly failed of effect. *Purgatives* he thinks absolutely necessary in most cases, to relieve the constipation and fæcal accumulation so common in females labouring under this disease, but those of the more drastic kind are decidedly to be rejected. The laxative mineral waters are, of all others, perhaps the most efficacious, though, for obvious reasons, they produce more benefit when the patient journeys to the springs. Much reliance is placed by Dr. Huss on *Tonics*, especially after fæcal accumulations have been removed, and of these he prefers the preparations of iron, while he considers quinine to have obtained an undeserved reputation in this disease. Many have regarded *narcotics* only as palliative remedies in spinal irritation. Our author is, however, deeply impressed with their great value as a means of cure. Small doses of opium or of morphia are decidedly the most successful means of treating the convulsive movements or tremors of the extremities; while cramp of a more tonic character, resembling tetanus, is equally amenable to this powerful drug. It should be given in small doses at first, and be continued for at least a fortnight to effect a cure. In cases of paralysis, strychnia has seemed to be a very valuable remedy, when given in doses of  $\frac{1}{12}$ th to  $\frac{1}{12}$ th of a grain two or three times a day. In one case marked good effects were obtained by the use of the Faba Sti. Ignatii. Belladonna proved only a palliative in cases of spasmodic asthma, from spinal irritation. From the preparations of arsenic, zinc, and copper, Dr. Huss has obtained no satisfactory results.

As an antispasmodic, the preparations of valerian deserve, he thinks, their reputation. The root, or rather *the fibrils of the root*, of *Artemisia vulgaris*, which appear to be the only active part of the plant, are much lauded by Dr. Huss for antispasmodic qualities. The root itself, he says, is nearly, if not quite, inert. Arnica is also in Sweden, as in Germany, a favorite remedy, especially in debilitated constitutions. Phosphorus and musk are favorably mentioned by our author, in convulsive affections, especially in laryngismus stridulus.

*Exercise and the due regulation of the diet* are strongly insisted upon by Dr. Huss; and he warns us against yielding to the unwillingness of patients to move, or to rise from their beds; as the completion of the cure can often only be obtained by exercise of the affected parts. From page 65 to 109 of the volume before us we are presented with a long and minute record of cases of spinal irritation; under which name our author seems to include more diseases than are generally ranked as such by English practitioners. Thus tetanus, chorea, and many forms of paralysis, are here described. One case of traumatic tetanus recovered under the free use of opium and of tartar emetic, one grain of each having been administered every hour. Of fifteen cases of tetanus (our author does

say they all were the consequence of wounds) five recovered under this plan of treatment.

Dr. Huss considers chorea to be a disease in which the spinal cord acts independently of the brain, and that the impulse in this malady is produced alike upon the extensor and upon the flexor muscles; while in tetanus the former only are affected. A kind of partial paralysis often, he says, continues long after severe attacks of either disorder. We would willingly extend to greater length our analysis of the interesting cases detailed here, but our notice of this malady will, perhaps, be already considered too long, we will, therefore, extract only one more observation relating to this portion of our subject:

"Rheumatism," says Dr. Huss, "is but too often made the scapegoat of many obscure complaints, and of paralysis among the rest. Partial paralysis may, however, occur from causes identical with those which produce rheumatism. Thus I have seen an individual who had placed his hand, at night, against a cold and damp wall, and on awaking in the morning that hand was completely paralysed. I have also treated a child which, for half an hour, had amused itself with melting ice on a frozen window with one of its fingers, and that member had become paralysed. In both cases I consider the first effect to have been produced on the cutaneous nerves, and to have been transmitted from thence to the spine; from whence, again, the paralysis was extended to the affected members."

In such cases we should, perhaps, have applied our remedies to the spine itself; but they both seem to have recovered by the endermic application of strychnia to the hands.

*Diseases of the respiratory organs.* Our author has already, in former reports, dwelt at great length upon these maladies; and his notice of them here is consequently short. Eight cases of diseases of the larynx and trachea were treated in the course of the year. One case occurred in a male patient, the other seven were females, and there were two deaths. The first was the cure of a girl, thirteen years of age, who had suffered from gradually increasing loss of voice and dyspnea for above a year, when suddenly, after exposure to a cold north-east wind, she was seized with acute laryngitis, which proved fatal, in spite of tracheotomy and all other remedies. On dissection, a mulberry-shaped excrescence was found occupying the ventriculus Morgagni of the left side, while the whole mucous surface of the trachea, and even down to the minute divisions of the bronchi, was closed with a firm, false membrane, about half an inch in thickness. It is curious that the patient, some years before, had had a similar excrescence on the face, which entirely disappeared, according to the report of the child's parents, on the application of a popular Swedish remedy, which was once common, too, in England, viz. the hand of a corpse.

*Pneumonia.* The report of this year presents us with a brief summary of 140 cases, having a mortality of 10 per cent. Of these, 23 were females and 117 males, and by far the greater number were between 20 and 40 years of age. Inflammation of the lungs is surely more frequent in these northern regions than in Great Britain. The months of April and May have for many years afforded in Sweden more than one-third of the cases of pneumonia, and the mortality was highest in June and July, which Dr. Huss ascribes to the great frequency of various abdominal complications

in the early part of the summer. The relative seat of the inflammation was as follows :

Pneumonia of the right lung in 87 cases  
 " left lung in 39 cases  
 " both lungs in 14 cases.

A great disproportion was observed in the mortality of the two sexes, the deaths among the males averaging 1 in 14, and among the females 1 in 4. Pneumonia in the left side is considered by Dr. Huss as much less fatal than pneumonia of the right, the proportions being 1 death in 19 in the former case, and 1 in 10 of the latter form. Dr. Huss acknowledges that he can assign no reason for this difference. His general experience has taught him that females are more liable to double pneumonia, which is always the most fatal.

The treatment of *tubercular phthisis* appears to be in Sweden, as elsewhere, an ever disputed point. Dr. Huss fancies that he has lately obtained more benefit from moxas applied below the clavicles, than from any other mode of counter-irritation. For his general opinions regarding this malady we are referred to the Report of the preceding year.

*Diseases of the circulating system.* In the space of two years, 28 cases of acute pericarditis have occurred in the hospital, and of these not less than 23 were females. Unfortunately Dr. Huss again refers us to the Reports of preceding years for the details of his cases, and the mode of treatment employed. Under the head of phlebitis there is given here a well-observed instance of slow destruction of life by this malady after childbirth. Acute symptoms did not appear until the fourth week, and the patient lingered till the thirteenth week after delivery.

Dr. Huss allows of at least two causes of phlegmasia alba dolens : the one is the compression of the hypogastric and common iliac veins by the head of the infant, the other arises from pus circulating in the system. The former usually comes on soon after delivery ; the latter often does not appear till the second or third week.

*Diseases of the digestive organs.* Simple ulcerations of the stomach are treated by our author by rest, light farinaceous diet, and the salts of silver, copper, or zinc, while counter-irritation is applied externally. He confesses, however, that the diagnosis of this malady during life is extremely difficult, and the great obscurity of the symptoms will cast a considerable shade of doubt over every recorded case of cure.

*Typhoid fever.* Dr. Huss considers the name of *enteritis folliculosa* as by no means universally applicable to the disease in question. He adds his testimony to that of almost all medical writers in this country, that typhus fever is most rife and severe where noxious exhalations from sewers, cesspools, &c., especially abound. During wet winters it has been remarked in Sweden that typhus was very common ; but as soon as the long and steady frosts of these northern regions commenced, there ensued a great and striking abatement of the disease. We fear that travellers have been deceived by the beautiful islets and clear waters of the Swedish capital. " There is no capital in the world, at least, we think, in Europe, where so much refuse is allowed to lie in the open streets, and, above all, in the large open courts of the dwelling-houses (gardar)."

The number of typhus-fever patients in 1842 was nearly double that of

1841, in consequence of an epidemic having broken out at an early period of the year, and which supplied patients to the hospital at the rate of 50 cases per month. The total number of cases was 517, of which 342 were males and 175 females. The mortality was 10 per cent., i. e. 1 in 12 of the males, and 1 in 8 of the females, and, as usual, it was highest in those of advanced age. Of the whole 517, a preponderance of cerebral symptoms was observed in 136 cases; in 208, abdominal symptoms prevailed; and in 173 patients the signs of both affections were combined. The mortality was much the highest among those who exhibited prominent cerebral symptoms, and lowest where the two affections appeared together. Death occurred, too, at a much earlier period in cerebral cases.

The following is an abstract of an analysis of various symptoms of typhoid fever:

a. Dr. Huss insists much upon the withered loose condition of the heart in all cases of death from typhus, the muscular substance of that organ having entirely lost its firmness and elasticity. He suggests that to this loss of power of the heart is to be attributed the pulmonary congestion so constantly observed in typhoid fever.

b. Disintegration of the blood was observed in 47 cases out of 54 that were examined with this view. Dr. Huss remarks that, when death occurs after the third week, the blood is often found to have regained a certain portion of its fibrine, perhaps in consequence of the numerous secondary inflammations that are then liable to occur.

c. The spleen also, which, in typhoid fever, is generally found softened and enlarged, changes, in protracted cases, so as to regain its natural size, though its texture becomes more firm and granular than in the normal state. A softened and enlarged spleen was most frequent in those who died with predominant symptoms of cerebral disturbance.

d. The peculiar typhoid ulcerations of the intestinal canal were found in 39 cases out of 54, which is certainly a larger proportion than generally occurs in England. In 1841 the proportion in Stockholm was as 6 to 1; this year it was  $3\frac{1}{2}$  to 1. Dr. Huss adheres still to the opinion he expressed in former Reports, that in proportion as the typhoid exanthema shows itself more on the external surface of the body, so is it diminished in the intestines.

At page 151 we find an interesting account of the epidemic typhus fever which prevailed in December, 1841, and January, 1842, in the police barracks (*gendarmerie*) at Stockholm. Of 280 men constituting this portion of the force, 64 were affected with typhus fever in the space of six weeks. Thirteen cases illustrating the three classes of typhoid fever before mentioned are here given at great length, and they are followed by an extended analysis of the symptoms of the disease. The cases of most frequent occurrence in this epidemic were of the mixed class, or those wherein the cerebral and abdominal symptoms were of equal intensity, and Dr. Huss here repeats his firm conviction, that all the three classes or varieties of typhoid fever here named are alike the results of a similar morbid process, and are therefore to be considered as one and the same disease.

a. Cerebral form (page 167). Dr. Huss divides his history of the symptoms into two periods. In the first, along with the usual indications of cerebral typhoid fever, hemorrhage from the nose frequently occurred,



but it does not seem to have exercised any peculiar influence on the duration or character of the malady.

A species of typhoid eruption was observed in all the cases, appearing as light red streaks (not the *tâches rosées lenticulaires*) over the chest and shoulders, and spreading from thence over the whole body, but rarely extending to the face. These streaks exhibited a tendency to assume a deeper hue, and at length, in the severest cases, they became of a deep violet, and the intensity of their colour was in exact proportion to the severity of the symptoms.

*b. Abdominal typhus.* In this form the usual well-known symptoms appeared, except that we do not find any distinct mention of the roseate lenticular spots so common in this type of fever. The typhoid exanthema, (?) however, before spoken of, was here again observed, but it never assumed the deep blue or violet colour. In four cases there were, however, spots like those of purpura, which *did not* disappear on pressure. When much diarrhea had occurred, there was generally excessive emaciation, and the recovery was exceedingly slow.

Dr. Huss was often able to foretell the approach of convalescence from the fact of the patient's assuming a posture different from that which they had kept throughout the severity of the disease, which had been generally the recumbent position on the back.

After a minute historical analysis of these three types of typhoid fever, Dr. Huss proceeds to consider the various peculiarities of this epidemic.

1. *Varieties.* Under the first head he acknowledges that there was often considerable difficulty in determining the exact species of fever, as at one time abdominal, and at another cerebral symptoms predominated in the same case. Sometimes, too, abortive cases occurred, where the fever came on severely at first, but disappeared suddenly upon an emetic or purgative being given, and without running its usual course. All epidemics present numerous instances of this kind, but it is still uncertain whether these cases would have become really severe if they had been left entirely to nature.

2. *Complications* occurred but rarely, and were chiefly confined to affections of the respiratory organs. It is remarkable that those of the gendarmerie who had previously exhibited symptoms of phthisis were peculiarly free from the influence of the epidemic.

3. *Sequelæ.* In two cases where the cerebral symptoms had greatly predominated, almost total deafness ensued, and continued for a long period. Oedema of the legs and feet was very frequent where the heart's action was so much weakened, that the first sound was scarcely audible, but it yielded rapidly to tonic treatment.

4. *Terminations and prognosis.* Critical symptoms, especially prolonged sleep and copious perspiration, occurred in twenty-nine cases, or in nearly one half of those affected, and Dr. Huss considers the tendency to crises to have been here much greater than in ordinary typhoid fever.

5. *Causes.* Two thirds of the cases occurred in individuals under 30 years of age. We have, in this particular instance, peculiar advantages for ascertaining the influence of age, temperament, &c., in exciting disease, as all the individuals affected were submitted to the same conditions of life, and all, too, were equally exposed to a similar routine of duty.

One principal exciting cause of the disease is thought to have been the constant rain during the night, when the guard was patrolling, and this severe weather lasted almost without intermission for above two months. The men came in from patrol (a duty of ten to eleven hours' duration) with their clothes perfectly saturated with rain, and their moist garments were hung up to dry in the crowded room in which they slept, which was moreover raised to a high temperature to facilitate the process of desiccation. By this heat and moisture, with the exhalations from so many human beings congregated in one apartment, our author thinks that a typhoid miasm was at length generated. An immediate mitigation of the violence of the symptoms was the consequence of a change of situation, effected by removing the patients to a purer air and to better ventilated apartments, while the cases that subsequently appeared were all of a milder character. The disease did not appear to be of a contagious nature, as it did not spread in a single instance among the other patients, when the sick men were removed to the general hospital. Those who had served in the patrol for only a few weeks or months were as susceptible of the disease as the veterans, who had been for years in the force.

6. *Nature of the malady.* Dr. Huss believes this epidemic to have consisted in an alteration of the blood, both in its physical constitution and in its vital properties, occasioned by the influence of the miasm brought into contact with it in the capillaries of the lungs. A similar epidemic is stated to have occurred among the workmen employed in excavating a canal in the Royal Park at Stockholm, during the months of August and September. In this instance the works were for some time carried on in a deep morass. It is to be regretted that no chemical investigation of the blood of the individuals affected by this epidemic was attempted, for, although few results of great immediate importance have hitherto been obtained by these researches, still it is obviously the line by which great discoveries are to be made, and every additional observation will prove of value. The quantity of serum was apparently in most cases diminished, the coagulum was loose and imperfect, and sometimes did not form at all, indicating a great deficiency of fibrine. In the two fatal cases the usual symptoms of typhoid action were observed—the fluid condition of the blood, the softened spleen, the atrophied state of the heart, and the swollen appearance of the glands of Peyer and Brünner in the abdomen; from which, however, as Dr. Huss observes, we are by no means to infer that these glands were so affected in all those cases which recovered. The more robust the patient, the more intense in general were the cerebral symptoms, while the abdominal type predominated in those of a lymphatic and less sanguine temperament.

7. *Treatment. External remedies.* Venesection was only practised at the commencement of the malady, when there was much evidence of congestion in the brain and lungs. Beyond the fourth day it was never resorted to. When the abdomen was painful on pressure, especially over the iliac regions, cupping over that part was frequently practised with success, and was occasionally repeated up to the seventh day; but not after that period. Blisters, sinapisms, and turpentine epithems, particularly the last, were found to be of great benefit as derivatives, and, above all, were useful in counteracting the frequent tendency to congestion in the

lungs and trachea. Ice, in many cases, was agreeable to the patient, as an external application ; but wherever it irritated rather than soothed the sick, it was wisely discontinued.

*Internal remedies.* Purgatives (not of the drastic kind) were freely given in all forms, unless the diarrhea and abdominal complication was excessive ; but Dr. Huss derived still greater advantages from the free use of the mineral acids, especially of the muriatic acid in decoct. altheæ, and this he continued to give as long as the pulse remained full (whether its beats were sharp or soft), and the sounds of the heart normal. The varied state of the tongue, or an increase of tenderness in the bowels, formed no contraindication to its use ; but it could not be employed where a tendency was exhibited to congestion of the lungs or bronchi. When the pulse became small, and the first sound of the heart weak and short, so as nearly to resemble the second sound of that organ, the mineral acids were no longer available ; and Dr. Huss had then recourse to phosphoric acid, which he combined with infusion of ipecacuan, where diarrhea indicated the presence of abdominal ulceration. Camphor was found to be very valuable in all those cases where the shortening of the first sound of the heart, and the diminution of its intensity, indicated debility of that organ. Dr. Huss considers this symptom, when it occurs, to be a sure indication for the employment of camphor, *and the only real one* ; for no certainty can, he thinks, be obtained in this respect from the appearance of the tongue, from the pulse, or from the nature of the delirium. Where, however, there was diarrhea, accompanied with a deep flesh-red tongue, and also tenderness on pressure of the abdomen, he observed that the use of this drug often gave rise to vomiting and colicky pains in the abdomen. Might not the combination with hyoscyamus have obviated this inconvenience ?

Musk is thought by our author to be indicated when the patient lies motionless on his back, with low, muttering delirium, plucking of the bed-clothes, and subsultus tendinum ; when the pulse is small and scarcely to be felt, and, above all, when the first sound of the heart is no longer to be distinguished. The doses must be large (three to five grains several times a day), or no good results can be expected.

Our author speaks favorably of opium, when the delirium is of a more violent character ; but the skin must be cool and soft, and there must be no other signs of cerebral congestion. Belladonna may be employed in similar cases ; but it seldom produces any effect until its well-known influence on the pupils is developed. Sulphuric acid was employed with favorable results during convalescence, especially when the vital powers were much impaired ; as also in the diarrhea which sometimes persisted long after the febrile symptoms had disappeared.

Dr. Huss considers the employment of the ethereal oil of turpentine to be one of the greatest improvements in the treatment of certain complications of typhoid fever. It is of essential value, he says, in the typhoid form of pneumonia, which is so apt to show itself between the eleventh and fourteenth day of the disease ; particularly in those cases where the debility of the heart's action is announced by the absence or diminution of the first sound of that organ. The weakened heart is then unable to propel the blood through the capillaries of the lungs, congestion ensues, and proceeds

to inflammation of a low typhoid kind. The other indications for the employment of turpentine, in this complication, are cough with viscid and more or less bloody expectoration, dulness on percussion over some portion of the chest, particularly at the back part, with crepitation and tubar, or bronchial respiration there. Dr. Huss gives the following formula as a convenient mode of administering this medicine: *R. Ætherol. terebinth. ʒss; vitell. ovor. No. j; mucilag. gumm. Arab. ʒiij; Aq. destillat. ʒj; M. ft. emulsio. signr.* One teaspoonful to be given every hour in the daytime, and one grain of opium, in two doses, during the night.

8. *Duration.* The average of the whole cases gives twenty-three days as the mean duration of each case.

We regret that we cannot devote more space to the remaining contents of this volume, which chiefly relate to various diseases of the digestive organs. A curious instance is given (along with a well-executed engraving) of adipose tumours occupying a rather unusual seat, viz. the inner surface of the small intestine. These tumours, about twelve in number, hung by a small pedicle from the internal surface of the ileum. They were composed of adipose cells, inclosed in a common cyst, and covered by the mucous lining of the intestine. Their origin, therefore, was probably in the submucous cellular tissue.

The Report of this year is very brief in regard to diseases of the liver. An interesting history is detailed at great length of a man who, during life, had suffered from that rare form of hemorrhoidal tumour, the erectile (described by Earle and Copeland), and in whom, after death, the liver was found studded with tumours of a similar character. Dr. Huss considers these latter to be fully entitled to the much-disputed appellation of hemorrhoids of the liver ("hæmorrhoider pa lefvern"); but it seems doubtful whether the original tumours in the rectum can fairly claim that title.

Our author is, we think, somewhat imbued with the doctrines of Schönlein regarding the influence of the portal system in the production of disease. In one case, which he relates, there was found cirrhosis of the liver to a high degree, while the vena porta was much dilated; and on its inner surface there were observed numerous bony cicatrices, similar to those so often seen in the dilated aorta, while the veins opening into the vena porta presented the like appearance. These alterations Dr. Huss does not allow to be the result of primitive inflammation in these blood-vessels; but he maintains that the cirrhosis of the liver was the primary disease, which, by obstructing the free flow of blood through the vena porta, caused dilatation of that blood-vessel; and this, when long continued, produced a chronic inflammation, with thickening and deposition of osseous matter.

We trust that this brief analysis of Dr. Huss's Report will not have been uninteresting to our readers, and that, in common with ourselves, they will wish for some further acquaintance with this author's writings. A patient spirit of investigation characterizes the whole volume; and while Dr. Huss, in this respect, may take his stand with the most diligent pathologists of Germany, he is far superior to the brilliant writers of France in his intimate acquaintance with the medical literature of other countries.

## ART. XII.

*Mesmerism in India, and its Practical Application in Surgery and Medicine.* By JAMES ESDAILE, M. D.—London, 1846. 12mo, pp. 287.

IN our article on Mesmerism, eighteen months ago, we entered into an extended discussion concerning the validity of mesmeric facts, and the theory of *animal magnetism*; avoiding, however, all detailed examination of mesmeric doctrines in their application to the treatment of disease. We passed lightly over this latter topic, because, in dealing with the subject as a matter of evidence affecting its reality, we had consumed the whole of our space at that time available; yet we expressed a hope that a future opportunity would arise for supplying the omission in question. This opportunity, the appearance of Dr. Esdaile's book now presents to us.

Dr. Esdaile occupies the post of civil assistant surgeon to one of the hospitals in Bengal; a situation which furnishes him with ample means for testing the value of controverted modes of treatment. The experience which he has given to the world in this book is, assuredly, not a little startling: it has relation to the practical application of mesmerism in both surgery and medicine. The statements which he has put forth, we shall summarily notice; and then we shall submit a few remarks upon their credibility, and upon the consequence which, we think, should flow from them.

Our author informs us that, prior to his own experiments, he was almost altogether unacquainted with the subject; having neither read mesmeric books, nor witnessed experiments in the practice of others. The first case which came beneath his notice was quite accidental; and this circumstance is cited by himself as enhancing the credibility of the accounts which he publishes. He states, that had he selected for his first essay one whom he should have deemed to be an appropriate subject, he would most probably have taken "some highly sensitive female of a nervous temperament and excitable imagination, who desired to submit to the supposed influence." He observes that in the instance in question, however, he was guided by no previously adopted theory, but was determined purely by accident in the choice of his subject, who happened to be a Hindoo felon, of the hangman caste, condemned to labour in irons on the roads. Though somewhat lengthy, we will insert the details of this experiment in Dr. Esdaile's own words; previously directing the reader's attention to certain facts upon which the author himself lays great stress, as dissipating, in his own judgment, every source of deception and fallacy; these are: 1st, the purely accidental and unpremeditated nature of the experiment; 2d, all want of consent between the parties; 3d, the operator's want of belief in his own power, and slight knowledge of the subject; 4th, the absolute ignorance of the patient, who could not possibly have heard of mesmerism; and 5th, the impossibility deduced from the foregoing considerations, of the patient's imitating the mesmeric phenomena.

"*First experiment.* Mádháb Kaurá, a hog-dealer, condemned to seven years' imprisonment, with labour on the roads, in irons, for wounding a man so as to endanger his life, has got a double hydrocele. He was ordered to be taken from the jail to the charity hospital, to be operated upon.



"April 4th. The water was drawn off one side of the scrotum, and two drachms of the usual cor. sub. injection were thrown in. On feeling the pain from the injection he threw his head over the back of the chair, and pressed his hands along the course of the spermatic cords, closing his eyelids firmly, and making the grimaces of a man in pain. Seeing him suffering in this way, I turned to the native sub-assistant surgeon, an *élève* of the medical college, and asked him if he had ever seen mesmerism. He said that he had seen it tried at the medical college, but without effect. Upon which I remarked, 'I have a great mind to try it on this man; but as I never saw it practised, and know it only from reading, I shall probably not succeed.' The man continued in the position described, I placed his knees between mine, and began to pass my hands slowly over his face, at the distance of an inch, and carried them down to the pit of his stomach. This was continued for half an hour before he was spoken to, and when questioned at the end of this time his answers were quite sensible and coherent.

"He was ordered to remain quiet, and the passes were continued for a quarter of an hour longer—still no sensible effect. Being now tired (thermometer 85°), I gave it up in despair, and declared it to be a failure. While I rested myself the man remained quiet, and made fewer grimaces, and when ordered to open his eyes he said there was smoke in the room. This roused my attention, and tempted me to persevere. I now breathed on his head, and carried my hands from the back of his head over his face and down to the epigastrium, where I pressed them united. The first time this was done he took his hands off his groins, and pressed them both firmly down upon mine, drew a long breath, and said 'I was his father and mother, and had given him life again.' The same process was persevered in, and in about an hour he began to gape, said he must sleep, that his senses were gone; and his replies became incoherent. He opened his eyes when ordered, but said he only saw smoke, and could distinguish no one; his eyes were quite lustreless, and the lids were opened heavily. All appearance of pain now disappeared; his hands were crossed on his breast, instead of being pressed on the groins, and his countenance showed the most perfect repose. He now took no notice of our questions; and I called loudly on him by name without attracting any notice.

"I now pinched him without disturbing him, and then asking for a pin in English, I desired my assistant to watch him narrowly, and drove it into the small of his back. It produced no effect whatever; and my assistant repeated it at intervals in different places as uselessly. His back had continued to arch more backwards latterly, and he was now in a state of 'opisthotonos;' the nape of his neck resting on the sharp back of the chair, and his breech on the edge of it. Being now satisfied that we had got something extraordinary, I went over to the Kut-cherry, and begged Mr. Russell, the judge, and Mr. Money, the collector, to come and see what had been done, as I wanted the presence of intelligent witnesses in what remained to do. We found him in the position I had left him in, and no hallooing in his ears could attract his attention. Fire was then applied to his knee, without his shrinking in the least; and liquor ammoniæ, that brought tears into our eyes in a moment, was inhaled for some minutes without causing an eyelid to quiver. This seemed to have revived him a little, as he moved his head shortly afterwards, and I asked him if he wanted to drink; he only gaped in reply, and I took the opportunity to give, slowly, a mixture of ammonia so strong that I could not bear to taste it; this he drank like milk, and gaped for more. As the 'experimentum crucis,' I lifted his head, and placed his face, which was directed to the ceiling all this time, in front of a full light; opened his eyes, one after the other, *but without producing any effect upon the iris*; his eyes were exactly like an amaurotic person's, and all noticed their lack-lustre appearance. We were all now convinced that total insensibility of all the senses existed, and I ordered him to be placed on a mattress on the floor, and not to be disturbed till I returned. It was now one o'clock, the process having commenced at 11 a.m.

"I returned at three o'clock, and was vexed to find that he had awoken, and



been carried back to the jail hospital. The native doctor of the jail had come in, and on hearing that the Sahibs could not awake the patient, he set about doing so, and succeeded by throwing water on his face."

Dr. Esdaile maintains that an especial value attaches to early experiments in any new subject of investigation. In reference to his own trials of mesmerism in the first instance, he likens his position to that of the chemist who, having heard that a new elementary substance had been discovered by a certain process, sets his own apparatus to work in the way prescribed, and is rewarded by obtaining the same results as the first discoverer; these, Dr. Esdaile observes, surprise the experimenter even, who feels confident that he only relates what he actually saw, and that he is not seduced by previous theory and prepossession of mind to interpret appearances in support of a foregone conclusion.

Having, in the manner just stated, become satisfied that there was a truth in mesmerism, Dr. Esdaile informs us that he lost no time in turning it to practical account: and the success which he recounts surpasses all previous records of this nature. If we may credit the publication now before us, it would appear that almost every attempt to reduce a patient to the mesmeric condition was more or less successful. Indeed, the statements which are made regarding the facility of inducing insensibility prior to surgical operations are, to say the least, very remarkable, we may say, not a little suspicious. Within the period of eight months, viz. from May 1845 to January of the present year, Dr. Esdaile reports himself to have performed no less than seventy-three painless operations; and some of these were of the most formidable character, as may be inferred from the following synopsis furnished in a prefatory Report of Mesmeric Facts:

*"A return showing the number of painless surgical operations performed at Hooghly during the last eight months. Arm amputated, 1. Breast ditto, 1. Tumour extracted from the upper jaw, 1. Schirrus testium extirpated, 2. Penis amputated, 2. Contracted knees straightened, 3. Ditto arms, 3. Operations for cataract, 3. Large tumour in the groin cut off, 1. Operations for hydrocele, 7. Ditto dropsy, 2. Actual cautery applied to a sore, 1. Muriatic acid ditto, 2. Unhealthy sores pared down, 7. Abscesses opened, 5. Sinus, six inches long, laid open, 1. Heel flayed, 1. End of thumb cut off, 1. Teeth extracted, 3. Gum cut away, 1. Prepuce cut off, 3. Piles ditto, 1. Great toe nails cut out by the roots, 5. Seton introduced from ankle to knee, 1. Large tumour on leg removed, 1. Scrotal tumours, weighing from 8 lb. to 80 lb., removed 17, painless, 14.—Operations, 73."*

This statement, we conceive, involves an amount of success in the production of the mesmeric coma, far beyond any thing that is recorded in this country, or indeed in Europe. Dr. Esdaile assigns a purely hypothetical, we may say visionary cause for this. He attributes its comparative rarity in Europe to the "influence not being at once sufficiently concentrated on the patient, by transmitting it to his brain from all the organs of the operator, and through every channel by which it can be communicated." He says that, in India, with due patience and sustained attention, coma, in a tolerably extensive field of experience, can be produced daily, so that insensibility to the surgeon's knife will ensue; the process adopted by himself is set forth in the subjoined extract:

“Desire the patient to lie down and compose himself to sleep, taking care, if you wish to operate, that he does not know your intention: this object may be gained by saying it is only a trial; for fear and expectation are destructive to the physical impression required. Bring the crown of the patient's head to the end of the bed, and seat yourself so as to be able to bring your face into contact with his, and extend your hands to the pit of the stomach, when it is wished; make the room dark, enjoin quiet, and then shutting your patient's eyes, begin to pass both your hands, in the shape of claws, slowly, within an inch of the surface, from the back of the head to the pit of the stomach; dwelling for several minutes over the eyes, nose, and mouth, and then passing down each side of the neck, go downwards to the pit of the stomach, keeping your hands suspended there for some time. Repeat this process steadily for a quarter of an hour, breathing gently on the head and eyes all the time. The longitudinal passes may then be advantageously terminated, by placing both hands gently, but firmly, on the pit of the stomach and sides; the perspiration and saliva seem also to aid the effect on the system.

“It is better not to test the patient's condition by speaking to him, but by gently trying if the cataleptic tendency exists in the arms. If the arms remain fixed in any position they are left in, and require some force to remove them out of every new position, the process has been successful; the patient may soon after be called upon by name, and pricked, and if he does not awake the operation may be proceeded with.”

It would carry us beyond the limited space which we can give to the present notice, were we to furnish any detailed account of the various operations performed by Dr. Esdaile; it is not, however, necessary to do so. It has been seen in what way he was led to experiment in the first instance, and the particulars of the subsequent cases, in which the knife was employed without eliciting signs of sensibility, do not differ from the generality of this class of facts, exemplified in our former article upon this subject. We may, in the sequel however, have occasion to refer to some particulars of individual cases, in briefly discussing the credibility of these narratives.

We will now refer to the statements which are made concerning the alleged therapeutical action of mesmerism. Dr. Esdaile says, that it is useful in chronic inflammation, by gently stimulating the nerves and capillary vessels to more healthy action; and, with respect to functional derangements of the nervous system, he hopes that, at length, in this mesmeric agency, we have obtained a *direct* nervous remedy. The medical cases adapted for the use of mesmerism did not very often occur in our author's practice; because, says he, “the labouring poor do not usually resort to medical advice for nervous diseases till they are past cure;” he yet furnishes accounts of a number of them which he considers to have been beneficially influenced by the new remedy. The particulars correspond very much with the accounts which are given by other writers on mesmeric therapeutics. They are, on the whole, very unsatisfactory, like their predecessors. Not a few of them leave ample room for doubt, first, as to the reality of the disease treated; and, secondly, admitting their reality, as to the cure being effected by the alleged means.

The following is a summary of the medical cases said to have been treated successfully by mesmerism, within the before-mentioned period of eight months:

“*A return of medical cases cured by mesmerism during the last eight months*

Nervous headache, 3 cured by one trance. Tic-doloureux, 1 ditto. Nervousness, and lameness from rheumatism of 2½ years' standing, 1 by chronic treatment. Spasmodic colic, 1 by one trance. Acute inflammation of the eye, 1 by repeated trances in 24 hours. Chronic ditto, 1 by chronic treatment. Acute inflammation of testes, 1 by repeated trances in 36 hours. Convulsions, 1 by one trance. Lameness from rheumatism, 2 by chronic treatment. Lumbago, 1 by general and local mesmerising for a week. Sciatica, 1 ditto. Pain in crural nerve, 1 ditto. Palsy of one arm, 1 ditto for a month. Ditto of half the body, 1 ditto for 6 weeks. Feeling of insects crawling over the body, 1 by one trance.—18."

Now we maintain that we are not entitled, in reason, to reject the facts, or alleged facts above stated without at least a fair examination. It were very easy for us to chime in with the ordinary professional ridicule, in relation to such statements; but, in common honesty, we conceive that we are bound to take a different course. We think it incumbent upon us, fairly to investigate their consistency and authenticity, and to determine the respect or the derision with which they are to be treated, according to their probable truth or falsehood. For the present we shall confine our attention exclusively to the surgical cases; and the first question we have to discuss is this: *Are the statements FACTS?*

In our former article we adduced reasonable evidence to show that cases of coma, unattended by serious or permanent lesion, did sometimes arise spontaneously as disease; that, in such cases, all indications of sensibility were very often absent; and that, as respects numerous classes of mesmeric cases, the only novelty which attaches to them consists in their artificial production. Whether coma and insensibility can or cannot be induced by the mesmeric process is a matter to be decided by experience only; if we have not ourselves this experience, but rest satisfied with estimating the value of that which is recorded by others, we must, in the formation of a judgment, be guided by the common rules of evidence. In this state of the question we reiterate our former position, that sufficient grounds do actually exist for admitting both the possibility and the probability of coma being artificially produced. The extent to which the statements of particular individuals may justly be credited is, of course, a very different question. But when an honest and intelligent witness circumstantially relates, as having occurred, what is already admitted to be neither impossible nor improbable, the presumption, *a priori*, is certainly altogether in his favour.

In the volume before us we have a well-informed and intelligent surgeon telling us, that within a period of eight months, no less than 73 surgical operations were performed by himself, without there being any indications of suffering on the part of the patients; the whole of the circumstances, moreover, to which reference is made, being abundantly authenticated by respectable names. To suppose the existence of wilful misstatement, on the part of Dr. Esdaile, is out of all question, where detection of falsehood would follow so surely; for our own part we feel confident that, in the instances adduced, there exists, at the very lowest estimate, a *basis* of fact. That our author and his associates should have conspired to delude, and to mystify their friends and their countrymen at home, without any conceivable motive, we hold, indeed, to be too ridiculous to imagine for a moment; and we believe it to be almost as unreasonable to suppose that, in so large a number of instances as those related, individuals should

uniformly, as if by common consent, have repressed the very strongest instincts of our nature in avoiding every expression of feeling under agonizing torture,—and that, too, for the mere purpose of deceiving the bystanders. If, however, we could escape the difficulty by admission of this latter explanation, how must we dispose of the first experiments? The earlier patients, who were exclusively of the lowest and most ignorant castes, could certainly know nothing of mesmeric indications; least of all so completely, as successfully to simulate them. On every sound canon of evidence, then, we must admit the existence of *some* reality in Dr. Esdaile's facts. Let us remember that the statements involve nothing that we should deem to be impossible, even as natural occurrences, for physiological states, analogous to those now under discussion, have arisen spontaneously; besides well-authenticated instances of insensibility to external impressions have been developed here, in our country, by artificial means; we ourselves can, to a limited extent, testify to this circumstance, as affirmed in our former article. Why, then, should we hesitate to receive Dr. Esdaile's statements with calm respect, and with a disposition to give to them an unprejudiced investigation? It is certainly true that, out of the great number of alleged facts, there are many which are by no means proved to be facts, and not a few which were, in our mind, most probably sheer imposture; we yet reiterate our own conviction that it is most unreasonable to doubt that *some* were *bona fide* cases of artificially induced insensibility. If much of what is here averred were not so very unexpected and wonderful, so much opposed to common notions and every-day experience, and—shall we say?—to our prejudices, it is quite sure that we should never dream of calling it in question.

But although, on many accounts, ready to admit a reality in the reported experience of Dr. Esdaile, as well as in that of a corresponding character nearer home, we would yet caution the reader against the *too facile* reception of this class of facts; to the *proof* of which, undoubtedly, the evidence very often is far from being adequate. It is too common for witnesses in this matter, medical not less than lay, in the first instance to disbelieve altogether in the possibility of inducing, in any degree, states called mesmeric, and then, once satisfied of the contrary, to place almost implicit credence in every statement relating to the subject, however improbable, and however imperfectly attested. As past experience has exhibited cases of temporary insensibility to external impressions, irrespective of mesmerism, and as this fact justly disposes us the more readily to credit the reality of such states, when said to be developed under other circumstances, so, in like manner, past experience has repeatedly displayed instances where the most heroic endurance of pain has enabled the sufferer to repress the outward manifestations of it, and at times, too, for no other purpose apparently than the pure love of deception—a disposition very well known to attack some minds as disease. In judging of the fact of alleged insensibility in particular instances, consideration should of course be always directed to this circumstance. We rarely, however, read mesmeric statements, or see mesmeric experiments, without being struck with the fact of how little evidence, ordinarily, satisfies the all-believing professed mesmerist.

Dr. Esdaile, in the work we have under review, supplies, we think,

illustration in some degree of what we have just advanced. He does not appear to us to have been sufficiently aware of the *possibility*, or even the *probability*, of deception,—a circumstance most likely attributable to his having, at the very onset of his career, adopted the general views and the speculations of the animal magnetisers. It is true he states that, prior to his own experiments, his mind was all but a blank to the subject; that he had never seen mesmerism as practised by others; and that he had not even read a mesmeric *book*; but then he acknowledges himself to have read “scraps of newspapers” (most unphilosophical sources of a first impression); he states that he had paid attention to all well-attested reports upon the subject, and that he had ceased to regard it lightly, “ever since Dr. Elliotson declared, years ago, that he should despise himself if he did not declare his conviction of the truth of mesmerism.” We do but refer to all this as showing that Dr. Esdaile’s mind was *not* so divested of prepossession as he himself appears to have imagined; this consideration does not in any way invalidate the statements, but it constitutes a reason for the exercise of caution in determining the *degree* of their credibility. Moreover, there is another circumstance to which we would direct attention: the patients, very generally, were natives of India, and of the lowest and most degraded castes, and sometimes felons. Now it is, we believe, pretty notorious that the Hindoo race is characterised, in a very marked manner, by cunning and deception, and that they have especial satisfaction in deceiving the European; and we should naturally expect these unfavorable traits of natural character to be prominently displayed in the most ignorant and the least moral portion of the community. Neither can we forget the astounding constancy and self-possession with which this race of men endure self-inflicted tortures of the most agonising kind in the exercise of their religious superstitions. Dr. Esdaile does not appear to us to have sufficiently thought of these matters; yet he testifies to the accuracy of the Hindoo characteristic just referred to, where, in speaking of the proceedings of the first French commission of inquiry, he compares D’Eslon to a *Bengal witness*, whom he describes as “not content to tell the truth simply, but added so many corroborating inventions of his own, that no one knew what to believe.”

In confutation of the idea that he himself could have been imposed upon by the patients upon whom he had operated, our author remarks as follows:—

“Since then I have had, *every month*, more operations of this kind than take place in the native hospital in Calcutta in a year, and more than I had for the six years previous. There must be some reason for this, and I only see two ways of accounting for it: my patients, on returning home, either say to their friends similarly afflicted, ‘Wah! brother, what a soft man the doctor Sahib is! He cut me to pieces for twenty minutes, and I made him believe that I did not feel it. Isn’t it a capital joke? Do go and play him the same trick; you have only to laugh in your elbow, and you will not feel the pain.’ Or they say to their brother sufferers, ‘Look at me; I have got rid of my burthen (of 20, 30, 40, 50, 60, or 80 lbs., as it may be), am restored to the use of my body, and can again work for my bread; this, I assure you, the doctor Sahib did when I was asleep, and I knew nothing about it. You will be equally lucky, I dare say; and I advise you to go and try; you need not be cut if you feel it.’ Which of these hypotheses best explains the fact my readers will decide for themselves.”



This quotation, we allow, constitutes a very fair method of dealing with the objections of those who would maintain the whole affair to be *humbug*, and that a state of coma and insensibility had in no case been provoked artificially; yet it is very easy to conceive the existence of not very extraordinary *motive* for individuals to affect and to simulate such a condition, *after* the rumours had gone abroad,—in some instances justly,—regarding the painless character of Dr. Esdaile's operations; and the very fact of such rumours having had, *occasionally*, a solid foundation sufficiently accounts for the popularity which the particular hospital obtained. Individuals, from determination not to be outdone, or even from pure love of deceiving others *might* sham. We do not say that this was actually the case; but the possibility—nay, probability—of the thing suggests itself to us on perusal of the book; and we think that the idea is entitled to attention in forming an ultimate estimate of the statements.

The following case, to our minds, is most unsatisfactory; we insert the account as exemplifying what we deem to be the obvious disposition in our author to be satisfied of the genuine character of his facts upon extremely slender evidence:

"August 13th. Dr. Finch freely applied muriatic acid (such as is furnished by the Company's dispensary) to a sore, covering all the right temple of the woman Gendo (who was mesmerised, in his presence, by one of my assistants) without her showing the smallest degree of consciousness; and it was with great difficulty I awoke her, after he had failed to do so. During the burning with the acid her pulse fell from 85° to 80°, and *her respiration, which was quite natural before she was mesmerised, became entirely diaphragmatic and abdominal; the voluntary and semi-voluntary muscles of the chest being completely paralysed.*

"August 18th. Dr. Bedford, to-day, asked permission to apply the acid to the woman Gendo's sore when she was awake; and though I thought this most irrational scepticism (he having witnessed Dr. Finch's experiment) I consented, in order that it might not be said I interfered to save my phenomena. He accordingly wetted the end of the glass stopper with the acid, and touched the sore with it, and *the woman, for a few seconds, did not show any signs of acute pain.* There could be no doubt about it, she was found out! The arch-deceiver, having set a snare and delusion for me, was now laughing at my beard; and I was not relieved from my thick-coming fancies by Dr. Bedford kindly suggesting 'that she was probably a very insensible person naturally.' I was soon roused from my trance of bewilderment, however, by hearing the woman cry out that 'we had put pepper to her head,' and she sat up, showing signs of great pain; immediately after she declared 'her head was on fire,' and got out of bed, walking about distractedly in great agony. I ordered her head to be bathed, and, as the best anodyne, threw her into the trance. The sore being surrounded by tubercles, which retarded its healing, I took the opportunity to pare them off; and to this she was perfectly indifferent. In half an hour I awoke her with much difficulty, in order that Dr. Bedford might hear her first words, which were, that she knew nothing about what we were talking of, having even forgotten the burning."

The change in the character of the respiration, described as occurring "during the burning with the acid" by Dr. Finch, is precisely such as would take place under an effort to conceal the outward signs of suffering. Why, on the acid being applied in the waking state by Dr. Bedford, did not the patient, for some seconds, exhibit indications of pain? Did the manifestations of acute sensibility, after a brief lapse of time, become displayed by the patient, not instinctively, but designedly, just when she



discovered that the exhibition was anticipated? Why, moreover, did Gendo retain no recollection of the occurrence after mesmerisation. There was no mesmerism at the time. Was it that the woman was mistaken, in supposing that oblivion in this point of view was expected by the doctors? We refer to these circumstances, because they illustrate the feeble points in not a few of Dr. Esdaile's narratives. Still, we would maintain that, judging of the aggregate of the statements by all the accredited rules of evidence, it is impossible to dispose of this matter of painless operations by the assumption of delusion or imposture in every case.

With respect to the accuracy of the facts relating to the efficacy of 'mesmerism in the treatment of medical cases, we experience a much greater difficulty in forming a satisfactory decision than we do in the case of the surgical operations; the sources of fallacy, under the former circumstances, being so very much more considerable. From the previous summary given of Dr. Esdaile's recorded experience, it will be perceived that, as is nearly always the case where unwonted methods of cure are said to be employed, diseases of the nervous system form a very large proportion of the group; a class of cases, in which the powerful agency of excited imagination and hope of improvement, howsoever induced, is so universally recognized. Nevertheless, satisfied as we are that abnormal conditions of the system do at times ensue upon the practice of mesmerism, we are quite ready to believe that it may possibly be serviceable in certain nervous affections by exciting a change of action. It is quite certain that, by employment of the "passes" and other means for continuously sustaining uniformity of impression, sleep—common sleep—can at times be procured, when narcotic drugs would either fail or lead to ulterior mischief; and, under such circumstances, it is not difficult to suppose that actual good should arise from use of the means in question. Still, in all these matters, we are ever sceptical concerning the relations of the *post* to the *propter*; for the ordinary records of mesmeric cures always remind us of the obviously mendacious or exaggerated statements attesting the efficacy of every conceivable kind of quackery that has at any time attempted to identify itself with practical medicine. Thus, there is apparently very good evidence to show that epilepsy, hysteria, and neuralgia have sometimes given way under mesmeric treatment; and it frequently seems reasonable enough to attribute the recovery to the means antecedently employed; but then we have just the same evidence to prove that almost every other disease has, at some time or another, been cured by identical processes; nay, the same testimony is adduced to exemplify the curative agency of water about which the "passes" have been made; and sometimes even the therapeutical potency of the mesmeriser's volition is set forth! What wonder if such extravagances throw us back upon our primitive incredulity?

A good many of Dr. Esdaile's mesmeric cures, even as narrated by himself, appear to us to rest absolutely upon no evidence at all, although they are yet cited by himself as undoubted instances of success. The following cases are of this kind:

"June 6th. I was called at eight o'clock last night to see the wife of Baboo Essanchunder Ghosaul, deputy magistrate of Hooghly. I found her in dreadful convulsions; she was speechless, and suffering from a constriction in the throat, that threatened to suffocate her every minute; and she constantly beat or pointed

at the part. At one moment her body was perfectly rigid, and in another it was bent back like a bow, till she rested on the back of her head and heels only. I never saw such convulsions except in tetanus and hydrophobia, and all I knew of the resources of medicine was useless; for how could she take physic when she could not take breath? I therefore had recourse to my new solvent power, and after nearly an hour's hard work, I left her asleep and catalepted.

"July 1st. She has had no return of the fit. This is the lady for whose relief the conjuror was sent, but came too late.

"June 26th. Alunga, aged 24: she has slight contractions of both elbow-joints, from rheumatism, with acute pain on pressing the ulnar nerve at the elbow. At first she did not bear much handling without awaking; but, on being left alone, the trance deepened, and she permitted me to work her joints like door-hinges, and extend them to the natural degree, without awaking. One arm was much freer after the first trance and extension, and there was no pain.

"June 27th. Complains of considerable pain in her left arm to-day, and the nerve at the elbow is very tender. I passed my fingers along the course of the nerve for a few minutes, which removed the pain, and allowed her to extend the arm; I then held my fingers before her eyes for a few seconds, and she fell into my arms insensible.

"July 3d. This woman's pains fly about, but I can chase them away from any part by holding my fingers over it for a short time. She came limping up to me to-day, to have the pain taken out of her 'tendo Achillis;' and this I did by passing my fingers over the pained part. I then grasped it firmly; she felt no pain, and by looks and words expressed the utmost astonishment and delight. This woman's sensibility is such, that I, or any one, can now make her delirious by merely looking at her for five minutes. But more of this hereafter.

"August 18th. I requested Dr. Bedford to satisfy himself if the woman Alunga had pain in any part of her body. On being asked, she said there was acute pain in one heel; and Dr. Bedford spent a long time in testing the reality of its existence. He at last said that he was convinced that there was considerable pain in that spot. I then passed my fingers over the part for a minute, and grasped the heel as firmly as I could, and she declared the pain had vanished; and Dr. Bedford allowed that it had. He then looked at her steadily, and in a few minutes developed the mesmeric delirium, and desire to sleep-walk, always produced in this woman, if the influence is not quickly concentrated upon her. The other symptoms, tremor of the eyelids, inability to open them when closed, and the mesmeric trance, all followed in due course." (pp. 181-4.)

In the first of the above cases we have a somewhat severe attack of hysteria; and, in one hour after the physician's arrival, the paroxysm is over, and the patient goes to sleep. There is nothing to indicate more than the natural termination of the fit; certainly, there is nothing to show that the cessation of the paroxysm could be attributed to the agency employed. But it is exceedingly difficult to deal, in reasoning, with reports of individual facts, as the unavoidable omission of many little circumstances will often interfere with the formation of an accurate judgment with respect to them.

Having, however, fully admitted the high probability of some of Dr. Esdaile's statements concerning the painless character of the surgical operations; and being, indeed, from many circumstances, well convinced that a great depression of outward sensibility, if not its temporary abolition, will, in some constitutions, result from practice of the mesmeric art, we will now proceed to the consideration of what we deem to be the reasonable corollary, from this admission on our part. We conceive, then, that the evidence attesting the fact of certain abnormal states being induced by mesmerism, is now of such a character that it can no longer

be philosophically disregarded by the members of our profession, but that they are bound to meet it in the only way in which alleged facts can satisfactorily be either verified or confuted,—by observation and experiment. When it is positively affirmed that the mesmeric processes will sometimes render a patient utterly insensible to the surgeon's knife, when detailed illustrations of this fact are recorded almost every day, how can we *fairly* reject such statements, unless we go to nature, observe for ourselves, and demonstrate the *source* of the monstrous fallacy that is deluding members of the profession and the public alike? Indeed, we hesitate not to assert that the testimony is now of so varied and extensive a kind, so strong, and in a certain proportion of cases so seemingly unexceptionable, as to authorize us, nay, in honesty to *compel* us to recommend that an immediate and complete trial of the practice be made in surgical cases. If experience like that which Dr. Esdaile relates to us be but true in one tenth, nay, one hundredth of its particulars, we hold that a case is made out demanding searching inquiry. If mesmerism, even in its humbler pretensions, be absolutely untrue, let it be *proved* to be so. If careful observations and repeated experiment lead to the detection of some hitherto hidden cause of error and mistake that has deluded and mystified the more honest class of mesmerists, what a service will be rendered to humanity and to truth if this can be proclaimed on perfectly just and adequate grounds! In how much better a position shall we be *after investigation* for confuting the imposture, if such it shall turn out ultimately to be, than in continuing to treat the subject with contemptuous disregard! Of one thing let us rest assured, not only the public, but the more sober thinking of the profession will, ere long, hold those at a disadvantage, who, in opposition to *facts*, apparently well authenticated, can or will but adduce mere unsupported argument, or ridicule.

There would appear to be two conditions attaching to any novel practice in medicine, independently of the authority by which it comes recommended, that should influence its title to a fair trial; *first*, the extent of the anticipated benefit, and, *second*, the degree of possible mischief attending its employment. Now the *promised* advantages of mesmerism in surgical operations correspond with these requirements in an eminent degree. If the statements be corroborated, and if insensibility *can* be produced artificially, surely the immense acquisition both to operator and patient is obvious at once; and, according to all the evidence that exists upon this subject, mischief very rarely follows the practice of mesmerism in the event either of success or of failure. "I beg to state," says Dr. Esdaile, "that I have seen no bad consequences whatever ensue from persons being operated on in the mesmeric trance. Cases have occurred in which no pain was felt, even subsequent to the operation, and the wounds healed by the first intention; and in the rest I have seen no indication of any injurious consequences to the constitution. On the contrary, it appears to me to have been saved, and that less constitutional disturbance has followed than under ordinary circumstances." If then *good* is possibly to ensue, and *mischief* is but little to be feared from the experiment, why not candidly make it? Assuredly experiments in therapeutics are constantly made on grounds far less reasonable. If a single practitioner of any eminence recommend some novel and *heroic* treatment

in serious disease, multitudes are ready to try it; however perilous to the patient the trial, *a priori*, may appear. Although, at the present day, it is pretty well made out that pneumonia, in many instances, will come to a successful issue with very little depletion, some dozen years since large numbers of the profession, especially in France, did not hesitate, on the recommendation of M. Bouillaud, to bleed *coup sur coup*; and, about twenty years ago, when Dr. Armstrong bled largely, and administered heroic doses of calomel in the incipient stage of fever, many persons felt themselves authorized in adopting the treatment experimentally. Yet, in these instances, a degree of risk to the patient was incurred in the attainment of the possible benefit, and there was, moreover, an uncertainty in deciding upon the exact nature of the result, which, as regards mesmerism in surgery, would not be experienced. Again, we say, let it be *tried* upon patients about to be submitted to the knife; if true, let us have the benefit of it, and if false let the falsehood be *demonstrated*.

Experiments relating to the treatment of medical diseases by any doubtful remedy could not result in the same decided issue, as in the example of mesmerism applied in operative surgery. We may observe, however, that when full satisfaction has been obtained respecting the efficacy of certain processes in the production of coma and insensibility, it would naturally be expected that, in various morbid states, the induction of such conditions may aid the restorative powers of the system; at any rate, in the present acknowledged paucity of positive therapeutical agents in numerous diseases, especially in those prominently affecting the nervous functions, the trial might very reasonably be made. We think sufficient evidence exists to prove the power of the mesmeric passes—or, we would rather say, the passes of mesmerists—as also of Mr. Braid's hypnotic fascination, in occasionally conciliating sleep, in nervous cases, after the failure of ordinary medical means. We may attach no importance to the *mesmerism* employed in such cases as involving specific agency; the patients may have been made to sleep very likely by the process operating as an appropriate *lullaby*, and the improvement have thus followed. It is, however, satisfactory to know that, under such circumstances, we can sometimes provoke sleep, and thereby effect a mitigation of human suffering.

We have before said that Dr. Esdaile exhibits a full and undoubting faith in most of the phenomena usually recorded as characterizing mesmerism; and thus, in reasoning upon the assumed resultant benefit in medical cases, he constantly avails himself of the theory of animal magnetism. In speaking of the curative agency, he holds it to be due to the transference of a vital fluid from the person of the mesmeriser to that of the mesmerised. We have too recently devoted a portion of our pages to this point, to reopen the discussion upon the present occasion; we can only observe that, after a careful perusal of Dr. Esdaile's book, we do not see that he has strengthened one whit the idea of exoteric animal influence—of objectivity—in mesmerism. We do not *deny* the reality of animal magnetism; we assert, however, that it is not *proved*, at least to our satisfaction.

In dealing with the subject of mesmerism, we are but desirous of coming at the truth; we have already given our reasons for thinking that the

time is come when, so far as it may be false, it can only be put down by calm and unprejudiced inquiry. From the curious and extraordinary accumulation of records amassed by the animal magnetisers, we have from the beginning been very much persuaded that, at the foundation of all the extravagances of the mesmeric disquisitions, there would ultimately be discovered *some* truth. So far back as the year 1839, after presenting to the reader a brief historical account of animal magnetism, we stated that we could not "be so far influenced by the impostures occasionally practised under the name of magnetism, as wholly to deny that some of the phenomena, from time to time produced by all aspirers to the art, seem to result either from some principle heretofore unknown and not yet correctly designated, or from some modification of recognized principles in the animal economy, which cannot yet be accurately limited or defined." In our article of last year, we entered into a more careful investigation of the particular facts, attempting some discrimination of the most probably true from the almost certainly false. In the former category we included the artificially produced *sleep, coma, altered sensibility, spasm, or temporary paralysis of muscles, &c.*—THE SIMPLE PHENOMENA OF MESMERISM; our admission of the reality of these conditions rested, *first*, upon their antecedent probability, as being, in most respects, analogous to abnormal states sometimes arising spontaneously as disease; *secondly*, upon the fact of the evidence attesting them corresponding with all just requirements; and, *lastly*, on the circumstance of having ourselves either seen them or been grossly duped in spite of a very rigid scrutiny. In like manner—exactly on the same grounds—we admitted the reality of mesmeric *somnambulism*, to the exclusion of *clairvoyance* and the other phenomena comprehended in the term *lucidity*. These latter, however, as well as the theory of animal magnetism, we only rejected as *unproved*; because, unlike the simple mesmeric phenomena so designated, we had sought for the reality in vain, and because the evidence, when fully and fairly tested, had always appeared to us to break down—feeble and inconclusive at the best. Within the last eighteen months we have not been indifferent spectators of what has been going on with respect to these matters; we have read, and we have ourselves investigated, with minds open to conviction and indifferent to every consideration but that of *truth*, and our conclusions remain unchanged. We must reiterate that, in our judgment, after making all due abstraction of the roguery and deceit notoriously prevalent in many mesmeric exhibitions, there is but a reality in the simple phenomena as just enumerated; that the induction of these does not necessarily imply the existence of exoteric animal influence; but that, as formerly maintained by M. Bertrand in France, and still more recently by Mr. Braid in this country, the mesmeric states most probably, in all cases, arise from sensible—not occult—impressions; and that the higher phenomena, which imply the receipt of intelligence otherwise than through the customary channels, are not only in the highest degree improbable, but are utterly unsupported by the requisite force of evidence.



## ART. XIII.

*The Brain and its Physiology ; a Critical Disquisition on the Methods of determining the Relations subsisting between the Structure and Functions of the Encephalon.* By DANIEL NOBLE, M. R. C. S.—London, 1846. Post 8vo, pp. 459.

“THE doctrine of the functions of the brain constitutes, most assuredly, a subject of discussion inferior to no other branch of science, either in interest, extent of its objects, or in general importance; and in physiology it must, for many reasons, be esteemed the highest department.”

With this prologue to our author's introductory chapter we need scarcely express our hearty concurrence; and notwithstanding that we have on former occasions treated somewhat in full the subject of the physiology of the brain, both in its phrenological aspect and in connexion with the physiology of the nervous system in general, we feel called upon, by the peculiar position of the question at the present time, to enter again into a somewhat detailed examination of it; with the hope of throwing some new light upon it from our own resources, and of being thus able to assist in clearing the path to further discovery. Nothing can be better than the spirit with which Mr. Noble sets out upon his undertaking, as will be evident from the following extract, which follows a notice of the imperfect views current upon the subject amongst physiologists in general:

“And yet there is no good reason why an inquiry into the functions of the brain, and of its various parts, should not be successfully prosecuted; for there is no obscurity in this department of physiological research, which is not common to many others. Cerebral physiology has its own obstacles and its own difficulties, like any other branch of science, requiring certain mental peculiarities and favorable opportunities for their removal and dissipation; but still, under just circumstances, the discovery, not only of the general office of the brain, but of the functions of its particular parts, is an object of pursuit for the philosopher as legitimate, and as likely to reward the labour of investigation, as the solution of any other physiological problem. This attempt will be found useless, or productive of imperfect or contradictory results, only so long as the method pursued is defective or faulty. All sciences, physiological not less than those purely physical, have sustained both error in their development and retardation in their progress, wherever they have been cultivated, without the guidance of sound principles in the investigation. Under such circumstances talent has but led to confusion, and industry has yielded no fruit.” (p. 10.)

But what are “the sound principles of investigation” which are to guide us in this difficult department of physiology? Are they those which have been found successful in other branches of physiological inquiry, and which would naturally occur to us as our safest indicators in the exploration of a new and unexplored path? Or are we to discard these tried and faithful guides, and to rely exclusively upon others, which have been long since demonstrated to be insufficient to conduct us to a valid conclusion, in almost every other portion of the inquiry into the functions of the human structure? At this starting point, we regret to find ourselves so far from agreeing with Mr. Noble, that our further paths must be widely divergent. For the main object of his work is to prove, that the physio-



logy of the brain of man must be first evolved by the comparison of the relative development of its different parts, in different individuals of the human race, with the proportionate manifestation of the various powers, propensities, &c., in the characters of these individuals respectively ; and that the validity of the inferences derived from comparative anatomy must be tested by their coincidence with those thus deduced from observation on man. We, on the other hand, are prepared to maintain that comparative anatomy is here, as elsewhere, our most trustworthy guide ; and that the validity of any inferences drawn from observation of the coincidence between the development and the functional manifestations of the various parts of the human encephalon, must be tested by their conformity with its obvious and decided indications.

We must do Mr. Noble the justice to say that he makes out what will doubtless appear to a large proportion of his readers a most excellent case. He hits away, right and left, at comparative anatomists and vivisectionists, and at physiologists who pin their faith upon them ; and demolishes all their assertions and arguments with such cleverness, and so completely to his own satisfaction, that none but those who have previously made themselves well acquainted with the subject are likely to do otherwise than accord with him. His confidence in the fundamental correctness of the method of Gall is so great, that he unhesitatingly undertakes to defend it against all comers ; his admission of possible improvements in it being so limited as not at all to affect the ardour in which he upholds it, as affording the only sound exposition of the functions of the encephalon.

But he does not give us any sufficient explanation why, in this particular case, we are to abandon that philosophical method of studying physiology upon the basis of comparative anatomy ; the advantages of which are every day becoming more apparent. His arguments on this point may be reduced to these two ;—the supposed difficulty of determining the real analogies of the several parts of the nervous system, in animals constructed upon different types ;—and the want of materials for a reasonable psychology of the lower tribes of animals. Now in regard to the first point, we do not hesitate to say that the difficulty is more apparent than real. Mr. Noble has made the most of it, by pointing out the discordance on this subject in the writings of comparative anatomists ; but this discordance was much greater in the infancy of the science than it is now, and it is gradually giving way to a very satisfactory harmony of opinion, as the principles upon which the investigation must be conducted are being gradually evolved. Such discrepancies are common to the early stage of all sciences. It would be equally to the purpose to quote the early mistakes of Gall, the unadmitted novelties of Vimont, the host of new organs established (to their own satisfaction) by the mesmero-phrenologists, but not admitted by their more conservative leaders, or the discrepancies between the London, Edinburgh, and Parisian professors of the science, in proof of the impossibility of drawing any safe or valid deductions from the method of observation which they advocate. And concerning the second objection we have to remark that, with regard to the broad distinctions between reflex (or excito-motor), instinctive (or sensori-motor), and intelligent (or volitional) actions—which distinctions, as we shall presently show, are of fundamental importance in the inquiry,—we think that ample

materials exist for determining them, if they are rightly employed. In fact, in the prosecution of both these inquiries the great evil seems to us to have been, that we have taken man as the type, and have endeavoured to refer every thing to our own standard ; whereas, if we had investigated the structure of the nervous system in the different classes of animals, and studied their actions, *from below upwards*, we are confident that much greater progress would now have been made.

In no department of physiology, as it seems to us, is the necessity of a continual appeal to comparative anatomy more obvious, than in the inquiry into the functions of the nervous centres. There is none in which we can more clearly trace the withdrawal of one organ after another as we descend the scale, leaving us a remainder whose operations we can study without the disturbing influences of vivisection. There is none in which we can more readily trace (if we only know how to look for it) the distinction between what is superadded and what is essential—what is apparent and what is real. There is none, then, in which we can so completely make use of the lower animals, in Cuvier's happy expression, as so many experiments ready prepared for us by nature. And there is none in which the neglect of this guide is more likely to lead us astray. We say this with no hostility towards the phrenological school ; for, as will hereafter appear, we think ourselves able to reconcile the leading dogmata of both systems, in a degree which we have not until recently thought possible ; and we have already, on several occasions, expressed our leanings in its favour. But we are desirous that all inductions respecting the physiology of the Encephalon should have the soundest possible basis ; and we are convinced that the best method of ascertaining what are the functions of its several parts, is to study those conditions in which their development presents the most striking contrasts, before proceeding to the comparison of them in different individuals of the human race, amongst whom there is not by any means a like diversity. Thus we are surely better able to estimate the functions of the cerebrum as a whole, and thus to lay the foundation for the inquiry into the distinct operations of its several lobes and minor subdivisions, if we first determine what it does *not* do, by tracing it through the various degrees of degradation which it exhibits as we descend the vertebrated series, until it entirely disappears in the lowest ; and by observing the actions of animals, which are destitute of the remotest analogue of it—than by any observation of even the extremest diversities, which are presented in its development in the human race. From not attaching what appears to us sufficient importance to this kind of investigation, Mr. Noble devotes his almost exclusive attention to the functions of the cerebrum and cerebellum ; his allusions to the existence of any other ganglionic centres not being by any means pointed, except where he combats the opinions of Drs. Carus and Carpenter upon the functions of the tubercula quadrigemina. Consequently, though professing to be a treatise on the brain, his book is really devoted almost exclusively to an exposition of the function of the cerebrum and cerebellum alone. Nevertheless, from some hints let fall here and there, we gather that Mr. Noble is inclined to admit that the primary seat of sensation is not in the gray surface of the hemispheres, but in the masses of ganglionic matter at the base of the brain. Now, as we shall presently endeavour to show, the ex-

istence of a series of ganglia in immediate connexion with the organs of sense, is a neurological fact of far greater generality than the existence of a cerebrum, and is, therefore, of more fundamental importance; and we by no means despair, therefore, of convincing Mr. Noble and those phrenologists who like him are real truth-seekers, of the justice of our views regarding the primary functional value to be attached to these bodies.

Our present object is, rather, to indicate the mode in which the physiology of the encephalon ought, in our opinion, to be studied, than to enter upon a detailed criticism of Mr. Noble's very clever treatise; since there are many circumstances which lead us to refrain from the latter course, whilst we think we can do some service to science by adopting the former. But in justice to Mr. Noble we feel it right to state, that we know of no treatise which is calculated to convey to an intelligent and discriminative reader a better idea of phrenology; as he is alike free from the shallow dogmatisms of some authors, and the bold and ignorant quackeries which are so repulsive in others; and, if the correctness of his fundamental principles be conceded, makes a legitimate and philosophical use of observation for the establishment of the truths of which he is in search. Such might be expected from the author of "*True and False Phrenology*," and of other valued contributions to medical science.

There are some other points, however, which we must discuss with him, before we can altogether dismiss him from our critical bar; deeming him in special need of wholesome correction therefrom. Such, for example, we find in the mode in which he has handled the next object of inquiry,—the use of vivisections, in determining the functions of the nervous system; in commenting upon which Mr. Noble has, we think, greatly underrated the amount of information to be derived from experiments. We cannot call to mind a single point of fundamental importance, that has been established in this department of physiology without such aid. Where would have been our knowledge of the general seats of sensation and of the motor power, if the severance of the nerve-trunks had not revealed to us, that connexion through their means with the nervous centres is essential to the sensibility of every part of the body, and to the subjection of its contractility to the will? Where would have been our knowledge of the relative functions of the fibrous and vesicular (or white and gray) portions of the nervous substance, if experimental research had not informed us, that the former possesses no power save that of conduction, and that wherever the latter presents itself there is an independent power of origination? Where would have been our acquaintance with the relative functions of the anterior and posterior roots of the spinal nerves, had it not been for the vivisections of Sir C. Bell, Magendie, Müller, and others? Mr. Noble thinks that experiments can only be valuable, where there has been some antecedent knowledge gained by analytical comparison of function and structure; but all that anatomical inquiry impressed upon the mind of Sir C. Bell, before commencing his experiments, was that these roots must have different functions; and it is notorious that he had formed an erroneous idea of that difference, and that it rested with experiment to set him right. Where, again, would have been our knowledge of the different functions of the cranial nerves—of the purely motor character of some, of the mixed endowments of others—of the absence of common sensibility in the nerves of special sen-

sation, and of many other similar particulars,—but for experiment? True it is, that minute anatomical inquiry might have rendered many of these inferences probable; but it needed the confirmation of experiment to establish them; and we cannot call to mind a single instance in which there is a discrepancy between the results of these two modes of inquiry. We might advert to the purely motor character of the facial nerve, the purely sensory endowments of the first and second branches of the fifth pair, the mixed endowments of its third division, the almost purely afferent character of the glosso-pharyngeal, the relative afferent and motor powers of the superior and recurrent laryngeal nerves, the specially motor endowments of the pharyngeal branches of the par vagum, and many other such determinations, as resting with equal security upon both these foundations. Where, again, would have been our whole knowledge of the independent endowments of the spinal cord, and of its functional analogy with the ganglionic chain of the articulata, but for vivisections? And yet, Mr. Noble would have his readers to believe that little reliance is to be placed upon them; and why? Because the inferences which have been drawn from them do not always accord; the true interpretation of the results not having always been given at first. We wonder that his penetrating mind should have overlooked the fact, that where the first inferences have been demonstrated to be fallacious, that demonstration has been afforded by *additional experiments*.

Mr. Noble quotes, among other illustrations of the uncertainty of such results, the well-known error into which Sir C. Bell was at first led, in regard to the supposed motor functions of the infra-orbital nerve. The ass, in which this nerve had been divided on both sides, stooped down to the oats that were placed before him on the ground; and though the oats touched his lip, he made no effort to pick them up: whence Sir C. Bell inferred that the power of moving the lip had been destroyed by the operation. Now we could not have a better illustration of the necessity of cautiously separating the phenomena, from the inference founded upon them by the experimenter. Every subsequent vivisector has confirmed this fact; the result of the experiment is invariable. But the inference was wrong, and is now universally admitted to have been so; for it is not the motor power which has been destroyed, but only the sensory; and the animal, not being able to feel the oats, makes no effort for their prehension. And how was this correction supplied? By the additional experiments of Magendie and Mayo; who applied other tests, and proved the persistence of the motor power whilst the sensory was abolished. What ground, then, does this, or any similar fact in the history of neurology, afford for cavilling at the results of experimental research, as uncertain and fallacious? Does Mr. Noble feel less confidence in the now universal accordance of physiologists in Sir C. Bell's determination of the relative functions of the anterior and posterior roots of the spinal nerves, because his inferences were at first incorrect, or because, even when better established, they were for a long time disputed among physiologists; or because whilst *these* are generally received, others of his doctrines (such as his idea of the relative functions of the anterior and posterior columns, or the special respiratory system of the nerves) have not gained equal currency, or are altogether rejected? In no science do we find that the whole truth is disclosed and

established at once, even when the most sagacious investigators are in pursuit of it; and, considering the peculiar difficulties which attend all physiological researches, and especially those in which the nervous system is involved, we should rather feel surprised that so much has been done in the short time that has elapsed since the path of inquiry was first opened up, than sceptical as to the power of experiment and observation, judiciously conducted, to establish much of what we seek to know.

We would not be thought, however, to be over sanguine as to the results we expect from vivisections practised upon the encephalon. We have never been disciples of the school of Magendie; and we should shrink from being supposed to sanction the reckless barbarities of those, who have adopted him as their master and guide. We do not disguise from ourselves that any disturbance of the proper balance of pressure in the contents of the cranium is likely to affect the condition of every part; and that it is difficult, if not impossible, to effect the entire severance of any part from its connexions with the rest, without thereby disturbing the normal functions of those which remain. We cannot, therefore, feel the same confidence in the results of vivisections practised upon the encephalon, taken alone, as we should upon those of the division or irritation of a nerve. But if the results of experiments tally sufficiently well with the influences which seem to flow legitimately from comparative anatomy,—if we find an animal, for instance, from which a certain organ has been removed, to be reduced to the same level with one which is naturally destitute of it, allowance being made for the disturbance produced by the immediate shock of the operation,—or if we find that the removal of another organ uniformly destroys a power, which seems to be possessed by different animals in a degree that accords with the development of that organ,—we unhesitatingly say that it must be very cogent and well-founded evidence, which should lead us to put aside such a determination. The value which we attach to vivisections upon the *brain*, may not, after all, be very different from that set upon them by Mr. Noble; since we have confidence in their results, only when they have been made in harmony with the structural divisions pointed out by nature, and under the guidance of a reasonable idea of the functions of the parts experimented on. But *we* should found these preliminary ideas, both as to structure and function, upon the wide and (in our opinion) secure basis of comparative anatomy; which Mr. Noble would attempt to exclude, as affording no sufficiently positive data. Whether it does so, or not, time and good sense must decide.

Another point on which Mr. Noble has fallen into mistake, through the want of a sufficiently extensive acquaintance with comparative anatomy, and on which we believe that he participates in a common error (which therefore calls the more loudly for correction), is the following. In replying to objections raised against the phrenological system, on the ground that it is unsustained by any direct anatomical proof, he says, (p. 287), “Objections such as these can only arise from singular thoughtlessness, or from utter ignorance of nature’s plan in the development of the nervous system, which, *as a rule, exhibits no mechanical divisions coincidentally with distinctness in functions.*” [The italics are Mr. Noble’s.] We do not overlook the connexion in which this sentence occurs; from which it may be



inferred that Mr. Noble had the cerebrum of vertebrated animals alone in his mind, and merely intended to account for its not being mapped out like a phrenological bust. But the prominence given to the latter part of it is certainly calculated to mislead; for the applicability of the statement is limited to vertebrated animals, the *general* fact being very different. For no one can take even a cursory view of the development of the nervous system throughout the entire animal series, of which the invertebrated classes form *by far the largest part*, without perceiving the almost invariable rule in those classes to be, that mechanical division of the nervous centres *do* coincide with distinctness of function. We have ganglia for respiration, ganglia for mastication and deglutition, ganglia for locomotion, ganglia for special sensation, and ganglia for various actions peculiar to the species (as those connected with the suckers of the cuttle-fish), all of them distinct from each other, and easily recognized by their connexions with the organs specially destined to these functions respectively. In the higher forms of some of the most elevated classes, both of the articulated and molluscos series, we do trace that tendency to fusion and concentration, which is characteristic of the vertebrated sub-kingdom; but even in them the original mechanical distinctness of the ganglia that are functionally different, may be traced by sufficient attention to the history of their development. Thus the large ganglionic masses which occupy the thorax in many insects, and which are almost the only nervous centres in their trunks, are clearly formed by the fusion of some of the ganglia that were distinct in the larva, and by the increased development of the compound centres thus formed, to the exclusion of others which diminish and even disappear altogether. Now in the vertebrated series, which, in any general survey of this kind, must be considered as only *one* out of *four* distinct types, we find a still greater tendency to fusion or union of different ganglionic centres; but the purposes of this are evident,—to place them all under the protection of the cranio-spinal envelope, and to bring them into closer relation, in order to secure greater consentaneousness of action. But does not every physiologist know that in spite of external indications, real mechanical divisions still exist? Are not the segments of the spinal cord as independent of each other, in regard to their power of ministering to the reflex actions of the nerves proceeding from them, as are the distinct ganglia of the ventral cord of the articulates? Is the tract of vesicular matter, which forms the centre of the respiratory nerves, less a respiratory ganglion, because it is imbedded in the medulla oblongata? Is the gray nucleus, into which the root of the auditory nerve may be traced, less a mechanically distinct auditory ganglion because it happens to be imbedded amongst strands of white fibres connecting the organs above and below it?

But even were we not allowed to push our anatomical inquiries so far, as to determine by their means that what bears the external aspect of a single ganglionic mass is in reality formed by the fusion of several, the argument is all the stronger for the functional distinctness of those parts, which *do* remain mechanically distinct, even in the vertebrated series; and we cannot but think that we shall be better guided in our inquiries, by following out the clue afforded us by the divisions which nature has established, and of which she still leaves us the external indications, than



by setting out with the establishment of new and arbitrary divisions of our own. Surely the condition of the sympathetic system could never have occurred to Mr. Noble's mind; presenting to us, in the highest vertebrated animals, precisely what we have represented as the *most general* condition of the nervous centres, viz. functional independence, combined with mechanical distinctness. It is not strange that, in the infancy of comparative neurology, the sympathetic system of vertebrata should have been compared with the entire nervous system of invertebrated animals, consisting (as both do) of ganglia scattered as it were through the entire body. Nor is it difficult, on the other hand, to see why the sympathetic system should retain that condition, even in the highest vertebrata, since its offices are not of such predominant importance as to require the protection of the cranio-vertebral column; nor is there such need of consentaneousness in the actions of its different parts, as to require that close connexion between them, which we see so remarkably effected by the union of the centres altogether making up the cerebro-spinal apparatus.

We have made these remarks at this stage of our inquiry because, as we shall next endeavour to show, there is a much greater amount of mechanical distinctness (we use Mr. Noble's phrase in default of a better) in the encephalic centres of man and the higher vertebrata, than is generally supposed. We cannot have a better opportunity of demonstrating the importance of a due acquaintance with comparative anatomy to him who attempts to specialize the functions of the different parts of the human encephalon, than by contrasting the ideas we are led to form of the fundamental divisions of this complex organ, by the study of its anatomy in man alone, with those which we gain from a more extended survey of its development, in the series of animals constructed upon the same general model. The attention of every mere human anatomist (and we are not aware of any phrenologist who forms an exception to this statement) has been fixed almost exclusively upon the two organs, distinguished as the *cerebrum* and the *cerebellum*, as forming, with the medulla oblongata, the entire encephalon. It is not surprising that it should be so, when the preponderating bulk of these organs is considered, and when it is further borne in mind, that these are the only masses which *appear* to have any distinctness—the only ones, indeed, which can be proved to possess any speciality, except by the very closest scrutiny, guided by the light thrown on the search by comparative anatomy. But, go to the opposite extremity of the vertebrated series, and examine the encephalon of a fish: what is there to be found? A series of at least *four* distinct ganglionic masses, arranged in a line continuous with the spinal cord, three of them in pairs, the fourth and hindmost single. The analogues of these in the human encephalon cannot be determined without the most careful scrutiny, both into the relative connexion of the parts, and into the relative degrees of development, and the changes of form and position, which they present in the several intermediate groups, which connect the lowest with the highest of the vertebrated series. It cannot be said that this study, the importance of which has only been recognized of late years, has yet been prosecuted in such a manner as to place every analogy in its clearest and most definite form; but the following points may, we think, be regarded as es-

tablished by the general consent of those most competent to form a judgment on the subject.

1. The most anterior of the ganglionic masses in the encephalon of the fish, on either side of the median line, is the *olfactory* ganglion, being the centre in which the olfactory nerve terminates. Its connexions are with the most anterior extremity of the medulla oblongata, no evident communication of a direct character being traceable between the olfactive ganglion and the masses which lie immediately behind it. Not unfrequently we find that the olfactive ganglia are separated by a considerable interval from the remainder of the encephalon, being carried forwards in order to be in more immediate apposition with the organ of smell; and being connected with the medulla oblongata by slender peduncles. Hence they have been frequently overlooked, and the peduncles have been mistaken for nerve-trunks, proceeding from the second pair of ganglionic masses. This mistake has been common in human anatomy also; the real ganglionic character of the bulbous expansions, which lie on the cribriform plate of the ethmoid bone, having been overlooked, and the peduncles or commissures, which connect them with the other parts of the encephalon, having been consequently ranked as the first pair of nerves. There can now, however, be no doubt or mistake about the matter, since the presence of vesicular matter in these bulbous expansions, even in man, clearly demonstrates their ganglionic character; and in many of the lower mammalia, in which they are relatively of much greater size, they include a distinct ventricle.

2. The second pair of ganglionic masses in the brain of the fish is now regarded, we believe, by all the most eminent anatomists (whatever may have been their former differences of opinion), as the sole representative of the *cerebral hemispheres* of the higher vertebrata. But even this it is not *in toto*, for the fact appears to be, that only the external lamina of each ganglion, which in most fishes is very thin, can be at all compared with the cerebral hemisphere, the interior mass being rather to be considered, from its aspect and connexions, in the light of a corpus striatum. And we shall presently find reason to believe, that this rudiment of a cerebrum does not represent the entire organ, any more than the early embryo can be regarded as a complete *homunculus*, but that it must be considered as the first indication of the presence of the *anterior lobe* alone.

3. The third pair of encephalic masses has received the name of the *optic lobes*, being evidently the ganglionic centres of the optic nerves. On closely scrutinizing their structure and connexions, it appears that they contain the analogues of the corpora quadrigemina and thalami optici of higher animals, the roots of the optic nerves being especially connected with the former, which must be regarded as the representatives of the optic ganglia of the invertebrata.\*

\* The first attempt at a philosophical comparison between the encephalon of vertebrated and invertebrated animals, we believe to have been made by Mr. Owen in regard to the brain of the cephalopoda; in the illustrated catalogue of the Museum of the College of Surgeons, vol. i, 1835; and in his article "Cephalopoda," in the Cyclopædia of Anatomy and Physiology, vol. i, 1836. In 1838, Mr. Newport, in his prize essay upon the *Athalia centifolia*, stated it as his opinion, that the cephalic ganglia of insects are analogous to the tubercula quadrigemina of the brain of vertebrata; a statement fundamentally true, but requiring (as we shall presently see) some qualification, since the cephalic ganglia are made up of at least four coalesced pairs, of which the optic ganglia form only one.

4. Lastly, we find a ganglionic mass placed on the median line, over that space left by the divergence of the fibrous strands of the medulla oblongata, which constitutes the fourth ventricle. This mass is evidently the analogue of the *cerebellum*. It sometimes presents the rudiments of lateral appendages; and, as we shall hereafter see, it varies more in its relative dimensions in different species of fish, than does any other of the encephalic ganglia.

Now, that the same distinction of parts exists in the human encephalon is clearly proved by the fact, that all these—with the exception of the olfactive ganglion, which always exists in an almost rudimentary state—are to be most clearly made out in the early embryo. The whole condition of the encephalon (making allowance for the difference between an embryonic and an adult structure) is most remarkably accordant with that of the encephalon of the fish, for it is composed of a series of rounded masses, placed in a curved line, which is continuous with that of the spinal cord, and none of them has any evident predominance over the rest. Of the most anterior of these, the superficial portion is the rudiment of the cerebral hemispheres, whilst the deeper-seated part of the mass is evidently the representative of the corpora striata. In place of the single pair of ganglia behind these, which usually combines in fishes the analogues of the corpora quadrigemina and thalami optici, we have first a vesicle within which the thalami optici and the third ventricle are formed, and then another, which gives origin to the corpora quadrigemina. The mechanical division is here more complete, therefore, than that which prevails in most fully developed fishes, though there are some, as the lamprey, which retain more of the embryonic type, and present, even in their adult condition, two distinct lobes for the thalami optici and corpora quadrigemina. Behind the vesicle from which the latter are formed, in the human embryo, is a large vesicle, within which the *cerebellum* is developed.

We have, then, in fishes, and in the early human embryo, this remarkable condition of the encephalic centre,—that its great mass is evidently composed of a series of distinct ganglionic centres, of which the portion representing the cerebral hemispheres is usually the smallest. We have—1st, the olfactory ganglia; 2d, the corpora striata, covered by a thin lamina, which is the rudiment of the cerebral hemispheres; 3d, the thalami optici, inclosing the third ventricle; 4th, the corpora quadrigemina; and 5th, the *cerebellum*. *There is no more general fact in the whole range of comparative anatomy*, than that the encephalon of vertebrata is composed of these elements at the commencement of its development; and however much their relative size and importance may be altered in the progress of their subsequent evolution, their original distinctness remains unaltered. Even in the adult human brain this distinctness may be recognized by careful dissection; for, if we take the medulla oblongata as our guide, and trace its connexions with the several ganglionic masses at its summit, we find it sending distinct fasciculi of fibres into the *cerebellum*, the corpora quadrigemina, the thalami optici, the corpora striata, and the peduncles of the olfactory ganglia. The *cerebellum* is not more “mechanically” distinct from the cerebrum than are the corpora quadrigemina, the thalami optici, and the corpora striata from the true cerebral

hemispheres. Before attempting, then, to determine the functions of the compound mass, which, taken as a whole, we are accustomed to designate as the encephalon, it appears to us essential that we should distinguish the relative offices of the true cerebral hemispheres, and of the chain of ganglia which they inclose and overtop. We shall otherwise be in continual danger of attributing to one what are really the attributes of the other; and of reasoning upon that as a whole, which is really made up of a series of parts, alike mechanically and functionally distinct. Common sense, we apprehend, would dictate to us, that in the study of a complex piece of machinery, we should first make ourselves acquainted with the actions of the parts, which are sufficiently isolated to enable us to observe their operations independently of those of the remainder, and to put aside from the sum total all that can be fairly attributed to these, before endeavouring to analyse the operations of the portion that most defies our scrutiny. It is absurd to suppose that there is any such complete alteration in man of the relative functions of the several parts, as shall do away with the necessity for regarding them as distinct; when the whole history of the development of the human encephalon indicates its precise accordance with the general type of structure which prevails in the nervous centres of vertebrata, and when nothing is needed but careful scrutiny, to discover the same distinctness in these parts in the *complete* brain, as in the *embryonic*.

We must not, however, quit our account of the fish's encephalon without remarking that, although the ganglionic masses we have enumerated are those most constantly distinguishable, there are others which are probably always present, though usually *fused* into the medulla oblongata,—such as the ganglia of the auditory nerve and of the par vagum, which are “mechanically” distinct in some species, especially those of the carp kind,—whilst there are others, again, which are evidently called into existence to supply some special requirement. Under this last category we may rank the electrical lobe, an additional ganglionic mass peculiar to the gymnotus, from which proceed the greater part of the nerves that supply the electrical organs; a median ganglion, occupying nearly the same situation, but having a function (as known by its connexions) entirely different, in the remora, or sucking-fish, the remarkable suctorial disk on the head of which is supplied from it; and a series of ganglionic enlargements in the medulla oblongata of the trigla, or gurnard, which supplies nerves to the peculiar digitiform prolongations of its pectoral fins. We have noticed these facts to show that the connexion between “mechanical distinctness” and diversity of function is still a very obvious one, notwithstanding the tendency to the fusion of different parts of the nervous centres, which is generally so remarkable in vertebrata.

If, then, we consider the encephalon of vertebrata under its *most general* aspect—that, namely, which all vertebrated animals present at an early period of their development—we find it to consist essentially of the following parts. In the first place, of the medulla oblongata, or cranial prolongation of the spinal cord, which usually includes the respiratory, auditory, and gustatory ganglia, and of which the several strands have distinct connexions with the other ganglionic masses in the encephalon. Secondly, of olfactory and optic ganglia, whose functions are known by

their immediate connexion with the nerves of those senses respectively. Thirdly, of masses representing the thalami optici and corpora striata of the human brain, which are here seen to have an existence quite independent of that of the cerebrum, instead of being mere appendages of it. Fourthly, of the cerebellum. And fifthly, of a rudiment of the cerebral hemispheres. When it is borne in mind, that of the second pair of ganglionic masses in the fish's encephalon (which is very frequently smaller than the optic lobes, and seldom much larger) only the external layer or crust is the representative of the real cerebrum, we at once see what a very subordinate part it must perform in the sum-total of the operations of the nervous centres of these animals. The main instruments of these operations must evidently be the *chain of ganglia connected with the sensory organs*, in which, for the reasons we shall presently mention, we include the thalami optici and the corpora striata; and the encephalon is found to be more and more exclusively made up of them, the lower we descend in the scale, until the rudiments of the cerebral hemispheres disappear altogether.

We shall next briefly inquire into the analogues of these organs in the invertebrata, with the view of showing how uniform is the plan on which the encephalon is constructed in the whole animal kingdom. Wherever we have a distinct *head*—that is, wherever we find the mouth placed upon a prominent part of the body, which is furnished with organs of sense adapted to the selection of the food and the direction of the movements of the trunk—there do we find a pair of cephalic ganglia, which evidently have somewhat of the same dominating influence over the whole remainder of the nervous centres, as that which is possessed by the encephalon of vertebrata. That influence, however, seems to be often much less powerfully exerted; most of the movements of the body being due to the simply reflex action of the ganglia in immediate connexion with the several organs, any one pair of which frequently surpasses in size the whole of the cephalic mass. But in proportion to the development of the organs of sense in the head, do we find the size of the cephalic ganglia increasing; and, when perfect and complex organs of vision are present, as in the higher articulata and mollusca, we find the optic nerves attaining a very large size, whilst the cephalic masses are evidently in great part to be regarded as the ganglionic centres of those nerves. This becomes particularly obvious in watching the metamorphoses of insects, as Mr. Newport long since pointed out. For there are many larvæ which have very imperfect eyes, whilst the perfect insect possesses fully developed organs of vision; and the enormous increase in the size of the cephalic ganglia, which takes place during the metamorphosis, obviously corresponds with that development. But we must not regard the cephalic ganglia of insects, &c., as entirely analogous to the optic ganglia of fishes, since it is obvious from their connexions, that they are also the ganglionic centres of other organs of sense. And as, in the higher invertebrata, the enormous development of the optic ganglia causes all the rest to be swallowed up into them (so to speak)—just as the great development of the cerebral hemispheres in man obscures many other parts of his encephalon—it is needful to go to the simpler, and, above all, to the embryonic conditions of the encephalon, to discover their real signification. Now, when the



head of a myriapod is examined in an early stage of its development, as Mr. Newport has so admirably shown,\* we find its encephalon in a condition so precisely analogous to that of the lower fishes, that it is impossible not to be struck with the correspondence. The head consists of two principal divisions—the *cephalic* proper, and the *basilar*,—each of them composed of four segments. Each of these segments has its pair of appendages, and each possesses its own pair of ganglionic centres. The four ganglia of the cephalic portion subsequently coalesce to form the cephalic ganglia of the perfect animal, whilst the four of the basilar portion coalesce to form the first subœsophageal ganglia. Now, the four pairs of ganglia, included in the cephalic, lie one behind the other in regular succession, like the ganglia of the encephalon of the fish, and are just as functionally distinct; for the first pair is that which furnishes the *antennal* nerves, and the second the *optic*; the third pair supplies the internal maxillæ, and the fourth pair the maxillary palpi. Thus, the cephalic ganglia of the adult animal combine the functions of three pairs of ganglia, which are connected with distinct sensory organs; and is thus obviously analogous to the chain of sensory ganglia, which makes up by far the largest part of the encephalon of the fish. There is here, as might be anticipated from the gradual disappearance of those organs in the lowest fishes, no rudiment of the cerebral hemispheres, which are at once distinguished from other ganglionic masses by their *superadded* character, having no direct connexion with any of the nerves, but being implanted, as it were, upon the summit of the strands which pass upwards from the nervous centres of the trunk. Such a rudiment seems to present itself, however, in the encephalon of the highest mollusca, the cuttle-fish tribe, which present so near an approach to fishes in the development of individual organs, although the plan of their arrangement is different; for, in addition to the highly developed optic ganglia, and the ganglia from which the arms, masticating apparatus, and other parts are supplied, there is a single cordiform mass, placed on the median line, which, from its position and connexions, may be looked upon as the nearest analogue of the cerebrum that is presented among the invertebrata. It is worthy of remark, in reference to the position in which we nearly always find the auditory ganglion of vertebrated animals (which is imbedded, as it were, in the medulla oblongata, on either side of the fourth ventricle), that the first rudiments of the *organ of hearing*, which present themselves in the gasteropod mollusca, themselves occupy this very situation; being a pair of minute sacs, containing numerous minute *otolithes* floating in fluid, which are imbedded in the posterior lobes of their cephalic ganglia, which answer to the medulla oblongata of vertebrated animals.

Quitting, now, these forms of the nervous system, which represent the encephalon of the vertebrated animal deprived of its cerebrum, let us briefly examine into the nature of those more complex forms, which are characteristic of the higher vertebrata. It is not enough to pass at once from the encephalon of the fish to that of man. The true character of *his* brain cannot be known without a patient and gradual survey of the whole ascending series, nor without a careful comparison of the gradations

\* Linnæan Transactions, vol. xix; see also our Twentieth Volume, p. 493 et seq.



thus encountered, with those which are presented to us in the progress of embryonic development in the most perfect and complex form. A very cursory examination yields to us this important deduction,—that the increased complexity of the encephalic structure, in the higher vertebrata, is almost entirely due to the extraordinary development of the cerebral hemispheres, and of their commissural connexions with the other ganglionic masses; the development of the sensory ganglia not making by any means the same proportional advance, or rather, advancing rapidly up to a certain point, and then ceasing. A new and special series of observations are much required, for the sake of determining the relative sizes of the several organs we have indicated in different animals, both with respect to each other and to the bulk of the body. Thus, it is well known that the general statement, that the brain of man is larger in proportion to the entire weight of his body, has been disproved by the fact that a higher proportion is presented by several small birds and rodents. But if the statement had been made, that the *cerebrum* of man is heavier in proportion to the weight of his body than that of any other animal, we apprehend that it would have been strictly correct. For the encephalon of birds is still made up chiefly of the chain of ganglia we have described, which attains, we believe, its highest development, relatively to the weight of the body, in that class. On turning aside the cerebral hemispheres (which do not yet altogether cover in the optic lobes), we find that they really form but a thin lamina, beneath which lies a series of large protuberances, which the study of their connexions at once discovers to consist of the corpora striata, thalami optici, and optic ganglia. If these were weighed, exclusively of the cerebrum proper and cerebellum, we are assured that they would bear a larger proportion to the whole body of the smaller birds, than they would in any other class.

Before passing from this subject, we must notice an extraordinary error, into which Mr. Noble has been led whilst criticising certain opinions of Dr. Carpenter's, in reference to the functions of the corpora quadrigemina of man. "In birds," he remarks, "the optic tubercles (the *nates*) alone are found; the certain analogues of the *testes* do not exist;" whence he infers that Dr. Carpenter must be wrong in attributing to these organs any share in the instincts and emotions, having demonstrated to his own satisfaction that such a class of functions must be limited to the *testes*, if appertaining to the corpora quadrigemina at all. And, as birds have instincts and emotions, but no *testes*, Dr. Carpenter must be wrong in his allocation. Now we shall hereafter discuss the general question of the connexion of the instincts and emotions with the sensory ganglia, and shall only stop to point out the fallacy of Mr. Noble's argument. The *corpora bigemina* of birds are actually *far larger* in proportion than the *corpora quadrigemina* of mammals; and although the duplex enlargements of the latter might lead to the idea that these organs receive an increased development in that class, yet there is no proof whatever that the peculiarity is anything but superficial, the connexions of the optic ganglia in mammals and birds being exactly alike. We shall presently find that the *very large* proportional size of the optic ganglia in birds is an important argument in favour of the very doctrines of which Mr. Noble, (doubtless owing to having derived an erroneous notion of them,

from the imperfect manner in which they were first expressed by Dr. Carpenter) has given, in sundry parts of his work, a very strange misrepresentation.

The development of the cerebral hemispheres in the ascending vertebrated series, and in the human embryo, has lately been elaborately investigated by Professor Retzius of Stockholm; and he has given the most complete confirmation of the statements of former comparative anatomists in regard to the order in which the different lobes of the cerebrum are evolved, whilst he has shown that the same order prevails in the embryological development of the human encephalon. Notwithstanding the contradiction of Mr. Noble and the phrenological luminaries whom he quotes, we rely upon the testimony of those who *have* studied Nature and put her to the question in this matter, for the universal truth of that which a very limited amount of observation serves to indicate; namely, that the part of the cerebrum which is *most* developed in man, in comparison with other animals, is not the anterior but the *posterior*. This was long ago asserted by the opponents of Gall, upon the ground that the cerebrum of none of the lower mammalia extends so far backwards as to cover the cerebellum; and the objection was met by the phrenologists with the assertion that there is no anatomical division between the middle and posterior lobes, and that it is quite conformable to our general knowledge of the disposition of the several parts of the encephalon to suppose, that the parts corresponding to the posterior lobes of man do exist in the cerebrum of the lower mammalia, but that they are not extended so far backwards, because the whole mass of the cerebrum in man is pushed backwards by the increased development of the anterior and middle lobes. Now although there is no external line of separation between the middle and posterior lobes, such as exists between the anterior and middle, there are two points of internal structure which afford as definite an indication as can be required; these are the development of the posterior cornua of the lateral ventricles, and the situation of the hippocampus major. By these means it may be unequivocally determined, that the development of the *posterior* lobe of the human cerebrum is far greater in proportion to the whole mass, than it is in the highest of the other mammalia; and that in the lower mammalia, as in all inferior vertebrata, *the posterior lobe is totally wanting*. Further, the philosophical anatomist well knows that the rudiment of a cerebrum which exists in fishes, represents the *anterior lobe only*; that this enlarges as we ascend through the classes of reptiles and birds, but does not change its character; that the middle lobe is only developed as we enter the mammalian class, presenting itself at first in a very rudimentary form, and attaining increased development as we ascend; and that the posterior lobe is developed from the back of the middle lobe, making its first appearance in the carnivorous group.

This history *finds an exact parallel in that of the embryonic development of the human cerebrum*; the rudiment which presents itself at a period when the chain of sensory ganglia has attained an advanced development, having been shown by Professor Retzius to be the representative of the anterior lobe only, the development of this making considerable progress before the middle lobe begins to be evolved, and the posterior lobe being the latest in order of evolution. Strange as this asser-

tion may be to phrenologists, we have, nevertheless, an implicit confidence in its truth, arising from the knowledge which personal acquaintance has lately enabled us to form, of the remarkable perseverance and sagacity with which the eminent professor of Stockholm pursues his investigations, and of the habits of philosophical deduction which he exercises in drawing conclusions from his observations. The following summary of these we extract from the Monthly Report of the Royal Academy of Sciences at Stockholm, to which they have been recently communicated. We may mention that they fully coincide with the observations made by Tiedemann thirty years since; which were, however, neither so precise nor so complete.

“In the first period, which corresponds with the second and third months, only, the anterior lobes form; in the second period, which is comprised in the end of the third month, in the fourth, and in a small portion of the fifth, the two middle lobes appear; and after this time the posterior lobes. During the first period the descending horns of the lateral ventricles and the pedes hippocampi are wanting; these are added in the second period. During a great portion of the first period the hemispheres do not cover the thalami nervorum optico-choroidales; in the second period they completely overlap these parts, approach the large corpora quadrigemina, cover their anterior part, and then descend by the side of the cerebral nucleus (cone or stem), and, as it were, fold round it. If we examine a brain at this period of development we might, from its external appearance, imagine that the posterior margin of the hemispheres corresponds to their persistent posterior ends and margins, i. e. to those which are their posterior margins in their perfectly developed state. But it is not so. If we open the brain we come at once to the descending horns of the lateral ventricles, in which are the rudiments of the great pedes hippocampi. At a later period, in the fourth month, a small superficial notch is formed at the posterior margins of the hemispheres; and that part of this margin which is above the notch is the first rudiment of the posterior lobes of the hemispheres. These, which are thus for a long time only rudimental, begin above the middle lobes, gradually take in their posterior margin, follow it down, as development advances, by the sides of the cerebral nucleus, and terminate at that part of the middle lobes which meets the pedes hippocampi. Even in the brain of the mature fœtus, as well as in the fully developed brains of older persons, the posterior lobes are very clearly separated from the middle lobes by a branching furrow, which is especially distinct, on the vertical side of the hemisphere which lies next to the falx.”

It is interesting to observe that these researches find a parallelism in another sub-kingdom; for they coincide as to the direction of the development with the account which has been already given by Mr. Newport, in the Linnæan Transactions, of the manner in which the *cephalic* portion of the head is developed in the myriapod and other articulata, by an extension of the coalesced segments of which it is formed, from before backwards, so as to overlap the basilar portion, in exactly the same way as the cerebrum is now shown by Professor Retzius to overlap the corpora quadrigemina and cerebellum.

Now we affirm that no phrenologist is entitled to dispute such a position, without having followed up a similar investigation with similar perseverance and with equal capacity. And at present the explanation to which we just now alluded, as having been offered by the followers of Gall, is capable of being turned pointedly against themselves; for if, as they maintain, the increased development of one lobe of the brain may cause the

established by the general consent of those most competent to form a judgment on the subject.

1. The most anterior of the ganglionic masses in the encephalon of the fish, on either side of the median line, is the *olfactory* ganglion, being the centre in which the olfactory nerve terminates. Its connexions are with the most anterior extremity of the medulla oblongata, no evident communication of a direct character being traceable between the olfactory ganglion and the masses which lie immediately behind it. Not unfrequently we find that the olfactory ganglia are separated by a considerable interval from the remainder of the encephalon, being carried forwards in order to be in more immediate apposition with the organ of smell; and being connected with the medulla oblongata by slender peduncles. Hence they have been frequently overlooked, and the peduncles have been mistaken for nerve-trunks, proceeding from the second pair of ganglionic masses. This mistake has been common in human anatomy also; the real ganglionic character of the bulbous expansions, which lie on the cribriform plate of the ethmoid bone, having been overlooked, and the peduncles or commissures, which connect them with the other parts of the encephalon, having been consequently ranked as the first pair of nerves. There can now, however, be no doubt or mistake about the matter, since the presence of vesicular matter in these bulbous expansions, even in man, clearly demonstrates their ganglionic character; and in many of the lower mammalia, in which they are relatively of much greater size, they include a distinct ventricle.

2. The second pair of ganglionic masses in the brain of the fish is now regarded, we believe, by all the most eminent anatomists (whatever may have been their former differences of opinion), as the sole representative of the *cerebral hemispheres* of the higher vertebrata. But even this it is not *in toto*, for the fact appears to be, that only the external lamina of each ganglion, which in most fishes is very thin, can be at all compared with the cerebral hemisphere, the interior mass being rather to be considered, from its aspect and connexions, in the light of a corpus striatum. And we shall presently find reason to believe, that this rudiment of a cerebrum does not represent the entire organ, any more than the early embryo can be regarded as a complete *homunculus*, but that it must be considered as the first indication of the presence of the *anterior lobe* alone.

3. The third pair of encephalic masses has received the name of the *optic lobes*, being evidently the ganglionic centres of the optic nerves. On closely scrutinizing their structure and connexions, it appears that they contain the analogues of the corpora quadrigemina and thalami optici of higher animals, the roots of the optic nerves being especially connected with the former, which must be regarded as the representatives of the optic ganglia of the invertebrata.\*

\* The first attempt at a philosophical comparison between the encephalon of vertebrated and invertebrated animals, we believe to have been made by Mr. Owen in regard to the brain of the cephalopoda; in the illustrated catalogue of the Museum of the College of Surgeons, vol. i, 1835; and in his article "Cephalopoda," in the Cyclopædia of Anatomy and Physiology, vol. i, 1838. In 1838, Mr. Newport, in his prize essay upon the *Athalia centifolia*, stated it as his opinion, that the cephalic ganglia of insects are analogous to the tubercula quadrigemina of the brain of vertebrata; a statement fundamentally true, but requiring (as we shall presently see) some qualification, since the cephalic ganglia are made up of at least four coalesced pairs, of which the optic ganglia form only one.

4. Lastly, we find a ganglionic mass placed on the median line, over that space left by the divergence of the fibrous strands of the medulla oblongata, which constitutes the fourth ventricle. This mass is evidently the analogue of the *cerebellum*. It sometimes presents the rudiments of lateral appendages; and, as we shall hereafter see, it varies more in its relative dimensions in different species of fish, than does any other of the encephalic ganglia.

Now, that the same distinction of parts exists in the human encephalon is clearly proved by the fact, that all these—with the exception of the olfactive ganglion, which always exists in an almost rudimentary state—are to be most clearly made out in the early embryo. The whole condition of the encephalon (making allowance for the difference between an embryonic and an adult structure) is most remarkably accordant with that of the encephalon of the fish, for it is composed of a series of rounded masses, placed in a curved line, which is continuous with that of the spinal cord, and none of them has any evident predominance over the rest. Of the most anterior of these, the superficial portion is the rudiment of the cerebral hemispheres, whilst the deeper-seated part of the mass is evidently the representative of the corpora striata. In place of the single pair of ganglia behind these, which usually combines in fishes the analogues of the corpora quadrigemina and thalami optici, we have first a vesicle within which the thalami optici and the third ventricle are formed, and then another, which gives origin to the corpora quadrigemina. The mechanical division is here more complete, therefore, than that which prevails in most fully developed fishes, though there are some, as the lamprey, which retain more of the embryonic type, and present, even in their adult condition, two distinct lobes for the thalami optici and corpora quadrigemina. Behind the vesicle from which the latter are formed, in the human embryo, is a large vesicle, within which the *cerebellum* is developed.

We have, then, in fishes, and in the early human embryo, this remarkable condition of the encephalic centre,—that its great mass is evidently composed of a series of distinct ganglionic centres, of which the portion representing the cerebral hemispheres is usually the smallest. We have—1st, the olfactory ganglia; 2d, the corpora striata, covered by a thin lamina, which is the rudiment of the cerebral hemispheres; 3d, the thalami optici, inclosing the third ventricle; 4th, the corpora quadrigemina; and 5th, the *cerebellum*. *There is no more general fact in the whole range of comparative anatomy*, than that the encephalon of vertebrata is composed of these elements at the commencement of its development; and however much their relative size and importance may be altered in the progress of their subsequent evolution, their original distinctness remains unaltered. Even in the adult human brain this distinctness may be recognized by careful dissection; for, if we take the medulla oblongata as our guide, and trace its connexions with the several ganglionic masses at its summit, we find it sending distinct fasciculi of fibres into the *cerebellum*, the corpora quadrigemina, the thalami optici, the corpora striata, and the peduncles of the olfactory ganglia. The *cerebellum* is not more “mechanically” distinct from the cerebrum than are the corpora quadrigemina, the thalami optici, and the corpora striata from the true cerebral



hemispheres. Before attempting, then, to determine the functions of the compound mass, which, taken as a whole, we are accustomed to designate as the encephalon, it appears to us essential that we should distinguish the relative offices of the true cerebral hemispheres, and of the chain of ganglia which they inclose and overtop. We shall otherwise be in continual danger of attributing to one what are really the attributes of the other; and of reasoning upon that as a whole, which is really made up of a series of parts, alike mechanically and functionally distinct. Common sense, we apprehend, would dictate to us, that in the study of a complex piece of machinery, we should first make ourselves acquainted with the actions of the parts, which are sufficiently isolated to enable us to observe their operations independently of those of the remainder, and to put aside from the sum total all that can be fairly attributed to these, before endeavouring to analyse the operations of the portion that most defies our scrutiny. It is absurd to suppose that there is any such complete alteration in man of the relative functions of the several parts, as shall do away with the necessity for regarding them as distinct; when the whole history of the development of the human encephalon indicates its precise accordance with the general type of structure which prevails in the nervous centres of vertebrata, and when nothing is needed but careful scrutiny, to discover the same distinctness in these parts in the *complete* brain, as in the *embryonic*.

We must not, however, quit our account of the fish's encephalon without remarking that, although the ganglionic masses we have enumerated are those most constantly distinguishable, there are others which are probably always present, though usually *fused* into the medulla oblongata,—such as the ganglia of the auditory nerve and of the par vagum, which are “mechanically” distinct in some species, especially those of the carp kind,—whilst there are others, again, which are evidently called into existence to supply some special requirement. Under this last category we may rank the electrical lobe, an additional ganglionic mass peculiar to the gymnotus, from which proceed the greater part of the nerves that supply the electrical organs; a median ganglion, occupying nearly the same situation, but having a function (as known by its connexions) entirely different, in the remora, or sucking-fish, the remarkable suctorial disk on the head of which is supplied from it; and a series of ganglionic enlargements in the medulla oblongata of the trigla, or gurnard, which supplies nerves to the peculiar digitiform prolongations of its pectoral fins. We have noticed these facts to show that the connexion between “mechanical distinctness” and diversity of function is still a very obvious one, notwithstanding the tendency to the fusion of different parts of the nervous centres, which is generally so remarkable in vertebrata.

If, then, we consider the encephalon of vertebrata under its *most general* aspect—that, namely, which all vertebrated animals present at an early period of their development—we find it to consist essentially of the following parts. In the first place, of the medulla oblongata, or cranial prolongation of the spinal cord, which usually includes the respiratory, auditory, and gustatory ganglia, and of which the several strands have distinct connexions with the other ganglionic masses in the encephalon. Secondly, of olfactive and optic ganglia, whose functions are known by



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Quitting, now, these forms of the nervous system, which represent the encephalon of the vertebrated animal deprived of its cerebrum, let us briefly examine into the nature of those more complex forms, which are characteristic of the higher vertebrata. It is not enough to pass at once from the encephalon of the fish to that of man. The true character of *his* brain cannot be known without a patient and gradual survey of the whole ascending series, nor without a careful comparison of the gradations

\* Linnæan Transactions, vol. xix; see also our Twentieth Volume, p. 493 et seq.

thus encountered, with those which are presented to us in the progress of embryonic development in the most perfect and complex form. A very cursory examination yields to us this important deduction,—that the increased complexity of the encephalic structure, in the higher vertebrata, is almost entirely due to the extraordinary development of the cerebral hemispheres, and of their commissural connexions with the other ganglionic masses; the development of the sensory ganglia not making by any means the same proportional advance, or rather, advancing rapidly up to a certain point, and then ceasing. A new and special series of observations are much required, for the sake of determining the relative sizes of the several organs we have indicated in different animals, both with respect to each other and to the bulk of the body. Thus, it is well known that the general statement, that the brain of man is larger in proportion to the entire weight of his body, has been disproved by the fact that a higher proportion is presented by several small birds and rodents. But if the statement had been made, that the *cerebrum* of man is heavier in proportion to the weight of his body than that of any other animal, we apprehend that it would have been strictly correct. For the encephalon of birds is still made up chiefly of the chain of ganglia we have described, which attains, we believe, its highest development, relatively to the weight of the body, in that class. On turning aside the cerebral hemispheres (which do not yet altogether cover in the optic lobes), we find that they really form but a thin lamina, beneath which lies a series of large protuberances, which the study of their connexions at once discovers to consist of the corpora striata, thalami optici, and optic ganglia. If these were weighed, exclusively of the cerebrum proper and cerebellum, we are assured that they would bear a larger proportion to the whole body of the smaller birds, than they would in any other class.

Before passing from this subject, we must notice an extraordinary error, into which Mr. Noble has been led whilst criticising certain opinions of Dr. Carpenter's, in reference to the functions of the corpora quadrigemina of man. "In birds," he remarks, "the optic tubercles (the *nates*) alone are found; the certain analogues of the *testes* do not exist;" whence he infers that Dr. Carpenter must be wrong in attributing to these organs any share in the instincts and emotions, having demonstrated to his own satisfaction that such a class of functions must be limited to the *testes*, if appertaining to the corpora quadrigemina at all. And, as birds have instincts and emotions, but no *testes*, Dr. Carpenter must be wrong in his allocation. Now we shall hereafter discuss the general question of the connexion of the instincts and emotions with the sensory ganglia, and shall only stop to point out the fallacy of Mr. Noble's argument. The *corpora bigemina* of birds are actually *far larger* in proportion than the *corpora quadrigemina* of mammals; and although the duplex enlargements of the latter might lead to the idea that these organs receive an increased development in that class, yet there is no proof whatever that the peculiarity is anything but superficial, the connexions of the optic ganglia in mammals and birds being exactly alike. We shall presently find that the *very large* proportional size of the optic ganglia in birds is an important argument in favour of the very doctrines of which Mr. Noble, (doubtless owing to having derived an erroneous notion of them,

from the imperfect manner in which they were first expressed by Dr. Carpenter) has given, in sundry parts of his work, a very strange misrepresentation.

The development of the cerebral hemispheres in the ascending vertebrated series, and in the human embryo, has lately been elaborately investigated by Professor Retzius of Stockholm; and he has given the most complete confirmation of the statements of former comparative anatomists in regard to the order in which the different lobes of the cerebrum are evolved, whilst he has shown that the same order prevails in the embryological development of the human encephalon. Notwithstanding the contradiction of Mr. Noble and the phrenological luminaries whom he quotes, we rely upon the testimony of those who *have* studied Nature and put her to the question in this matter, for the universal truth of that which a very limited amount of observation serves to indicate; namely, that the part of the cerebrum which is *most* developed in man, in comparison with other animals, is not the anterior but the *posterior*. This was long ago asserted by the opponents of Gall, upon the ground that the cerebrum of none of the lower mammalia extends so far backwards as to cover the cerebellum; and the objection was met by the phrenologists with the assertion that there is no anatomical division between the middle and posterior lobes, and that it is quite conformable to our general knowledge of the disposition of the several parts of the encephalon to suppose, that the parts corresponding to the posterior lobes of man do exist in the cerebrum of the lower mammalia, but that they are not extended so far backwards, because the whole mass of the cerebrum in man is pushed backwards by the increased development of the anterior and middle lobes. Now although there is no external line of separation between the middle and posterior lobes, such as exists between the anterior and middle, there are two points of internal structure which afford as definite an indication as can be required; these are the development of the posterior cornua of the lateral ventricles, and the situation of the hippocampus major. By these means it may be unequivocally determined, that the development of the *posterior* lobe of the human cerebrum is far greater in proportion to the whole mass, than it is in the highest of the other mammalia; and that in the lower mammalia, as in all inferior vertebrata, *the posterior lobe is totally wanting*. Further, the philosophical anatomist well knows that the rudiment of a cerebrum which exists in fishes, represents the *anterior lobe only*; that this enlarges as we ascend through the classes of reptiles and birds, but does not change its character; that the middle lobe is only developed as we enter the mammalian class, presenting itself at first in a very rudimentary form, and attaining increased development as we ascend; and that the posterior lobe is developed from the back of the middle lobe, making its first appearance in the carnivorous group.

This history *finds an exact parallel in that of the embryonic development of the human cerebrum*; the rudiment which presents itself at a period when the chain of sensory ganglia has attained an advanced development, having been shown by Professor Retzius to be the representative of the anterior lobe only, the development of this making considerable progress before the middle lobe begins to be evolved, and the posterior lobe being the latest in order of evolution. Strange as this asser-

tion may be to phrenologists, we have, nevertheless, an implicit confidence in its truth, arising from the knowledge which personal acquaintance has lately enabled us to form, of the remarkable perseverance and sagacity with which the eminent professor of Stockholm pursues his investigations, and of the habits of philosophical deduction which he exercises in drawing conclusions from his observations. The following summary of these we extract from the Monthly Report of the Royal Academy of Sciences at Stockholm, to which they have been recently communicated. We may mention that they fully coincide with the observations made by Tiedemann thirty years since; which were, however, neither so precise nor so complete.

"In the first period, which corresponds with the second and third months, only, the anterior lobes form; in the second period, which is comprised in the end of the third month, in the fourth, and in a small portion of the fifth, the two middle lobes appear; and after this time the posterior lobes. During the first period the descending horns of the lateral ventricles and the pedes hippocampi are wanting; these are added in the second period. During a great portion of the first period the hemispheres do not cover the thalami nervorum opticorum; in the second period they completely overlap these parts, approach the large corpora quadrigemina, cover their anterior part, and then descend by the side of the cerebral nucleus (cone or stem), and, as it were, fold round it. If we examine a brain at this period of development we might, from its external appearance, imagine that the posterior margin of the hemispheres corresponds to their persistent posterior ends and margins, i. e. to those which are their posterior margins in their perfectly developed state. But it is not so. If we open the brain we come at once to the descending horns of the lateral ventricles, in which are the rudiments of the great pedes hippocampi. At a later period, in the fourth month, a small superficial notch is formed at the posterior margins of the hemispheres; and that part of this margin which is above the notch is the first rudiment of the posterior lobes of the hemispheres. These, which are thus for a long time only rudimental, begin above the middle lobes, gradually take in their posterior margin, follow it down, as development advances, by the sides of the cerebral nucleus, and terminate at that part of the middle lobes which meets the pedes hippocampi. Even in the brain of the mature fœtus, as well as in the fully developed brains of older persons, the posterior lobes are very clearly separated from the middle lobes by a branching furrow, which is especially distinct, on the vertical side of the hemisphere which lies next to the falx."

It is interesting to observe that these researches find a parallelism in another sub-kingdom; for they coincide as to the direction of the development with the account which has been already given by Mr. Newport, in the Linnæan Transactions, of the manner in which the *cephalic* portion of the head is developed in the myriapod and other articulata, by an extension of the coalesced segments of which it is formed, from before backwards, so as to overlap the basilar portion, in exactly the same way as the cerebrum is now shown by Professor Retzius to overlap the corpora quadrigemina and cerebellum.

Now we affirm that no phrenologist is entitled to dispute such a position, without having followed up a similar investigation with similar perseverance and with equal capacity. And at present the explanation to which we just now alluded, as having been offered by the followers of Gall, is capable of being turned pointedly against themselves; for if, as they maintain, the increased development of one lobe of the brain may cause the



displacement of another, the fullness of the *forehead* in man may be attributed to the development, not of the *anterior* lobes of his cerebrum, but of the *posterior*, these being the last to be evolved, and the parts whose superior development is most marked in him.

Following out our principle of endeavouring to determine, in the first instance, the functions peculiar to divisions of the nervous centres, whose "mechanical distinctness" is obvious, we shall proceed to inquire into the relative endowments of the cerebral hemispheres, and of the ganglia of sensation: and shall investigate the evidence afforded on this subject; first, by comparative anatomy; and, secondly, by experimental inquiry. Now the first remark that is forced upon us, in the most cursory examination of the structure of the cephalic ganglia of different animals, is this; that the *essential* part of the encephalon is formed by the collection of sensory ganglia, and that the cerebrum is an organ *superadded* for some purpose, which is only fully attained in the higher vertebrata. We see the cerebrum dwindling away as we descend the scale; until, in the lowest of the class of fishes (the amphioxus, or lancelet) no rudiment is left of it. But we find no such corresponding diminution in the sensory ganglia, even in those which have been commonly most regarded in the light of appendages to the cerebrum. And on proceeding to the invertebrata, we find the absence of the cerebrum to be the almost invariable rule, even where the sensory ganglia attain their highest development. We never find a cerebrum without the sensory ganglia; but, *in by far the greatest number of animals, we find the sensory ganglia without the cerebrum*; and it is only in mammalia, that the development of the cerebrum is such, as to give to it any obvious predominance over them.

We think ourselves fully entitled, then, to infer from these facts, that *the cerebrum is not the seat of sensation*; since we must otherwise believe that those animals which are destitute of it—insects, for example—are destitute of sensibility. In like manner, we may infer that the several classes of impressions are communicated to the consciousness through the medium of the ganglia, in which the nerves that convey them terminate; the optic ganglia, for instance, being the seat of visual sensations, the auditory, of the sensations produced by sounds, and so on. For there is no single organ that seems to be sufficiently the centre of the rest, for us to regard it as the special sensorium. Guided by this view, it seems to us not difficult to assign a function to the thalami optici; for whilst we have distinct olfactive, optic, and auditory ganglia, it seems but reasonable to expect, that we should have a ganglion for the formation of the sensations communicated by the nerves of touch; and the connexion of the thalami with the spinal cord into which all these nerves pass, is precisely what we should anticipate on such a view of its offices. Moreover it is interesting to remark that the connexion between the thalami and the proper optic ganglia is everywhere very close; and this, again, harmonizes well with the fact which is familiar to every one, that the sensations of sight and touch are more closely associated, both in the origination of mental *perceptions* (or general notions of the characters of the objects of sensation) and in the direction of muscular movements, than are any other pair of sensations.



But we think it may be further asserted with confidence that the ganglia in question are the sources of all those movements, which, following directly upon sensations, may be termed *consensual*. This is a class of actions which has, we think, been too little scrutinized by physiologists; and we are much mistaken if, under the guidance of comparative anatomy and experiment, combined with pathological observation, the subject is not capable of considerable elucidation. We must content ourselves with here briefly indicating the outline of the doctrines, which have been long in progress of development in our own minds, and which have now attained a completeness and consistency, that warrants our expressing them with some degree of confidence. Putting aside the purely reflex actions, in which a movement is directly excited by an *impression*, without the intervention of a *sensation*, a little observation of our own corporeal phenomena will bring under our notice many instances, in which movements are excited with equal directness and constancy by *sensations*, without the intervention of anything that can be properly called an *act of mind*, the simple *consciousness* being alone requisite. We may take, as illustrations, the closure of the eyelids to a dazzling light, the sneezing which is excited by the same cause, the start produced by sudden loud sounds, the vomiting induced by certain nauseous tastes or odours, and the movements of the respiratory muscles (as in laughter) which are brought on by the sense of tickling. We have purposely adduced examples of the class of movements in question excited through each of the senses, in order to show how universal is the principle for which we are contending. Knowing, as we do, that the sensory ganglia not only receive the sensory nerves, but are connected, by their implantation on the fibrous tracts of the medulla oblongata, with the motor system, we can at once understand the channel through which sensations should thus produce movements, without involving any higher act of the mind, or any exertion of the will. In the case of common or tactile sensation, there seems good reason for regarding the corpora striata as the motor portion of the ganglionic mass, of which the thalami optici constitute the sensory; the relation between them being, as well pointed out by Messrs. Todd and Bowman, the same as that which subsists between the anterior and posterior peaks of vesicular matter in the spinal cord.

Now if we inquire what movements in the lower tribes of animals are analogous to these consensual actions in ourselves, we think that a little consideration will make it at once apparent, that we are to refer to this category all those movements which are purely *instinctive*. We have on a former occasion pointed out the general characters, by which these actions may be distinguished from the volitional (Vol. XI, p. 96); and we need here only refer to their constant and immediate dependence upon sensations, and the absence (so far as we can at all discern) of anything like *an idea* of the purpose, towards which their operations are directed. We can, in fact, discover no middle view that can be taken, between the supposition that the purely instinctive actions (in which category we must include nearly the whole of those performed by insects) are immediately and necessarily prompted by sensations; and the notion that the sensations excite ideas, that the ideas are made the subject of reasoning processes, and that these reasoning processes issue in an effort of the will,

*designedly directed* (as in almost every action of adult man) towards the attainment of the object. Now if this be the case, it leads us to an important view respecting the mechanism, so to speak, of instinctive actions. Our usual method of reasoning upon these subjects leads us to class these actions as manifestations of different instinctive propensities; and a learned entomologist has enumerated between thirty and forty instincts which must be attributed to the bee. Upon phrenological principles all these ought to have their separate location in its minute cephalic ganglia. But we fearlessly assert that this classification of the several actions in question, as manifestations of different propensities at all corresponding with those of which we are conscious in ourselves, is entirely of our own making, and has no real existence in any brain but our own. We might as well expect to find a distinct organ in our own encephalon, for the consensual movement of the eyes, for laughter, sneezing, or any other association of muscular actions called into play by the direct agency of sensation. The sensory ganglia are, evidently, taken as a whole, the instruments by which the instinctive or consensual actions are performed. But why should we suppose different parts of the optic ganglion to be concerned, when the sight of an object of food prompts the movements requisite to obtain it, and when the sight or sound of one of the opposite sex calls into action the muscles, that bring the insect into close relation with it? What is needed is a ganglionic mass for the reception of sensations and for the origination of muscular movements; *what* movements shall follow *what* sensations, must be determined, according to the expression of Prochaska, by the "laws written upon the nervous pulp," just as in the case of reflex actions. We see no reason to doubt that insects and other animals, whose actions are chiefly instinctive, possess feelings of pain and pleasure in connexion with particular sensations, resembling those which *we* experience in connexion with the sensations that excite consensual actions in ourselves. But we can find less ground for the belief that they possess feelings of *desire* for any objects, analogous to those which form so important a part of our own mental operations. For these feelings result from a distinct *idea* or *conception* of the object; and their purpose is, evidently, to prompt to the reasoning processes which are required for its attainment. We do not *feel a desire* to close our eyes against a strong light, unless our attention be directed to the matter, and we attempt to resist the natural action which is prompted by the sensation. We do not *feel a desire* to start on a loud and sudden sound; on the contrary, the action *necessarily* and immediately results from the sensation, unless we have expected the sound, and put the will strongly in action to resist the impulse. We do not *feel a desire* to vomit when we taste or smell anything nauseous, for our desire is naturally the other way, and nothing but the strength of the sensation can overcome it.

This class of phenomena receives a remarkable illustration in certain morbid conditions of the system. Every one of our readers must have witnessed cases of unusual susceptibility to light, sound, tactile impressions, &c., evidently originating, not in disordered conditions of the organs themselves, but in affections of those parts of the sensorium, which are the centres of their respective nerves. A very instructive case of this kind,

in which a variety of curious movements were directly excited by sensations, has lately been recorded by Dr. Cowan.\* We trace the same influence in hydrophobia, in which the convulsive paroxysms are brought on by the *sight* or the *sound* of liquids. And we have another very instructive illustration, in the act of yawning, which, when the encephalic centres are in a state of preparation, induced by previous fatigue, is excited simply by the *sight* or the *sound* of the action in another person, without the attention being at all directed to the sensation, or any idea being attached to it. Upon phrenological principles, this action must be referred to a propensity to imitation, which has its local habitation in the cerebrum; but we cannot ourselves perceive any reason whatever for regarding it in any other light than as a consensual action, in which the optic or auditory ganglion (as the case may be) is alone concerned.

Our location of the centre of the actions of this class, in the cephalic ganglia, to the exclusion of the cerebrum, has been made upon grounds derived from comparative anatomy alone; but we shall find it completely borne out by the results of experimental inquiry upon the higher vertebrata. In addition to the general observations which we have already made upon this subject, we think it well to add one or two remarks upon the special applicability of experiment, to the determination of the functions of the encephalic centres. In the first place, we freely admit that no good purpose whatever can be derived from any hap-hazard incisions or slicings, or from the partial removal of any single organ. But we do affirm that, if we take the guide which Nature affords us, and remove *entire* organs, whose distinctness she herself indicates to us in language not to be mistaken, we may obtain important information, confirmatory of that which we derive from other sources. But it is to be steadily kept in view, that here, as in other instances, when we bring together and compare the results obtained by different inquiries, we must be careful to separate the *phenomena* which actually presented themselves, from the inferences drawn from them by those who have recorded them. By so doing, we shall be able to reconcile many apparent contradictions; and shall find a much greater amount of accordancy, than was to be anticipated from a cursory survey of the apparently discrepant accounts. Of this we shall presently have a striking instance, in reviewing the results of experiments upon the cerebellum. Moreover, we must draw a marked line of distinction between the *positive* and *negative* value of the evidence we accept. If we find that, after the removal of an entire organ, some function supposed to depend upon it still exists, with little or no impairment, we have a right to say that the function *cannot* be dependent upon that organ. On the other hand, the evidence afforded by the disappearance of a certain function or power, after the removal of any particular division of the nervous centres, is less satisfactory, and needs to be sifted with great care. But we are not entitled to reject it as unworthy of consideration, because we find the results in some degree discordant; still less if we find that, after the repetition of such experiments by a considerable number of independent vivisectioners, the phenomena themselves present such an accordancy, as cannot be accounted for by the general disturbance of the functions of the nervous centres, produced by the severity of the lesion itself.

\* *Lancet*, 1845, vol. ii, p. 364.

When the cerebrum of one of the higher vertebrata is entirely removed, the animal is reduced to the level of one of the invertebrata, which is naturally destitute of the organ; and it is important to ascertain, if the actions which it then performs are of the same nature as those, which are so characteristic of the latter. But, in judging of the results of such vivisections, it is important to bear in mind, that the functions of the remaining organs cannot but be impaired by the removal of a large part of the encephalic mass; and also that these functions, in the un mutilated animal, are by no means so obvious in its actions, where a cerebrum is present, as where it is altogether wanting. Thus, whilst nearly all the actions of insects, which are not reflex, may be regarded as the direct results of the exciting sensations, the actions of birds, whilst presenting a general similarity with them, are more obviously influenced by intelligence, and are, therefore, for the most part referrible, not so much to the consensual group as to the volitional. The variability of the actions of different individuals of the same species, under similar circumstances,—the actions themselves all tending to the same end, which is suggested and kept constantly before the mind by the prevailing sensation,—becomes very striking in the class of birds, when we closely scrutinize their habits, and contrast them with those of the other class, to which they bear so remarkable a general analogy. Making such allowances, then, we think that experimental inquiry affords satisfactory evidence of the identity in the condition of a bird or mammal, whose cerebrum has been removed, with that of an insect or other invertebrate animal which naturally possesses none. In the first place, we think it impossible to analyse and compare the results obtained by different vivisectioners, without coming to the conclusion that *sensibility*, both of a general and a special kind, remained after the removal of the cerebrum. Doubtless there are fallacies attending the deduction of any such inferences from the movements performed by the animal under various stimuli; since a large proportion of these movements may fairly be set down as of a reflex character, and as not indicating sensation. It is safer, therefore, to draw our conclusions from movements consequent upon such impressions made upon the organs of *special* sense, as cannot (so far as we are aware) excite movements without the intervention of consciousness. And it appears to us to be an inevitable deduction from these phenomena, that the animals retained the possession of the senses of sight and hearing, though in a very imperfect manner. It is not a little curious, that M. Flourens, who at first drew a contrary inference from his experiments,—maintaining that the removal of the cerebrum destroys all sensibility,—should have since abandoned this opinion, in favour of the one put forward by Cuvier and his colleagues, in their well-known report upon M. Flourens' first Memoir; having substituted, in the second edition of his 'Experimental Researches,' the term *perception* for *sensation*, whenever he speaks of the function which is destroyed by the removal of the cerebrum.—Secondly, in animals deprived of the cerebrum, consensual movements were excited by the sensations. Thus, a bird would follow the light, and it would avoid objects placed in its way; it would start from its nocturnal sleep, raising its head and opening its eyes, upon the slightest noise; and it spent a great part of its time in pruning its feathers and scratching itself. Even if we regard

the last of these actions as reflex, the preceding can scarcely be so considered. Making a fair allowance, then, for the disturbance in the condition of the whole encephalon, which is necessarily induced by the removal of so important a part, and for the small proportion which the simply consensual actions bear in the general movements of these animals, we think ourselves entitled to affirm, that the conditions essential for these actions remain after the removal of the cerebrum, and that we are not to regard that organ as having any necessary participation in them.

Other experiments prove how much the ordinary movements of birds and the lower mammalia are *directly influenced* by sensation. Thus, vertiginous movements may be induced in pigeons and rabbits, not merely by destroying one of the optic ganglia, but by simply blinding one eye, either by evacuating its humours, or by temporarily bandaging it. And we believe that the similar movements produced, in the curious experiments of M. Flourens, by lesions of the semicircular canals, are readily explicable upon the same principle,—perverted sensations in regard to the direction of sounds (of which it seems to be the special object of that part of the auditory apparatus to take cognizance) being thus communicated to the sensorium, and producing abnormal movements, the direction of which bears a constant relation to that of the canal. These and other similar experimental results, which are referred to by Mr. Noble with a sort of contempt, as not affording us the slightest knowledge of a precise kind in regard to the functions of the different parts of the encephalon, appear to us, on the contrary, when surveyed from the point of view to which we are led by comparative anatomy, to be in admirable harmony with the conclusions indicated by that mode of investigation.

Although the development of the cerebrum in the vertebrata appears to *supersede* the simple and direct actions of the sensory ganglia and the motor fibres proceeding from them,—and this, in a degree comparable to its superiority of size,—it by no means *alters* their function. The simply consensual movements gradually form a smaller and smaller proportion of the whole actions of the animal as we ascend the scale; just as in the invertebrata, we find the simply reflex operations of the lower gradually brought under the guidance and control of the consensual, which are so characteristic of the higher. The very same gradation may be traced in the human infant. No one can watch its actions with an educated physiological eye, without perceiving, that they are at first almost purely reflex; the nipple of its nurse being seized by the lips when they are applied to it, and the act of suction being performed, as Mr. Grainger's experiments prove, under circumstances that forbid any other idea being attached to them. But we soon find that it is guided to the nipple by the sense of sight, and also (we think) by that of smell, which appears to us to have much to do with this action; and the first movements which it performs, when it begins to take notice of surrounding objects, are evidently to be referred to the simply consensual group. This is just what we should expect from the fact, that the spinal cord and the sensory ganglia at its summit are the parts of the nervous centres which are first developed, and which most rapidly advance towards maturity. Their actions are sufficient for the human infant for a long period after its birth; and it is not until it is



several months old, that we can discern any, which can be unequivocally attributed to a dawning intelligence. From the time when this manifests itself, the simple consensual movements become less and less obvious; and at last they escape our notice altogether, if we do not carefully scrutinize our own actions. But it does not hence follow, that the sensory ganglia and their motor nerves are thrown out of use; or that their only function is to receive sensations and impart them to the cerebrum, without having any concern in the movements which it originates. For we think that all recent observations and experimental inquiries tend towards this conclusion—that *the cerebrum has itself no direct connexion either with the sensory or motor nerves*; and that as it receives sensations through the medium of the various ganglia, in which the sensory nerves terminate, so it executes the mandates of the will through the motor fibres which originate from those same ganglia, or from others in immediate connexion with them. The belief has long prevailed, that the motor power of the cerebral hemispheres is chiefly exerted through the corpora striata; and we have already proved, from the comparative anatomy of these organs, that they are by no means to be regarded as mere appendages to the hemispheres, but are independent ganglia, of which the cerebrum makes use (so to speak) to perform its movements. Bearing in mind their intimate connexion with the thalami optici, we think it probable (as already remarked), that they constitute the motor portion of the ganglionic centre of the tactile sensations and respondent movements; and, if this be the case, the reason of its peculiar connexion with the cerebrum at once becomes apparent. For all voluntary movements require *the guidance of sensations*; and most of these are of the tactile kind. The “muscular sense” of Sir C. Bell is a necessary condition of our keeping up any voluntary contraction of a muscle, unless some other sense is substituted for it,—as in the case of the woman, affected with anæsthesia, who could hold her child upon her arm as long as she looked at it, but could not, by the strongest effort of her will, sustain it after withdrawing her eyes. In the movements of the eyeballs, the guiding sensations (as Dr. Alison has shown) are ordinarily those received through the sight; and in the movements of the larynx and muscles of articulation, the guiding sensations are ordinarily those received through the hearing; but, in either case, if these senses are wanting, a sufficient amount of guidance seems to be derivable from the muscular sense. No instances occur to us, in which voluntary muscular movements are dependent upon the guiding sensations, derived from the smell or the taste.

Thus, then, the spinal cord with the ganglia at its summit,—forming altogether what may be termed the *cranio-spinal axis*,—to the exclusion of the cerebrum and cerebellum, furnishes all the conditions for the reception of sensations, and the performance of muscular movements; and the two latter organs may be regarded as superadded for some special purpose, receiving their stimulus to action through the medium of the cranio-spinal axis and its nerves, and exerting their influence on the muscular system through the same channel. In adopting this view, which is by no means a novel one, we think that we can harmonize completely all the results afforded by anatomical research and by experimental inquiry. It corresponds particularly well with the varying intimacy of the connexion



between the cerebrum and the different ganglia at its base, and with the relative development of the latter. For the connexion is *most* intimate with the thalami optici and corpora striata, which we regard as together constituting the ganglionic centre of tactile and muscular sensations, and of the respondent motions; and these ganglia are in man by far the largest of the whole series; both of which facts seem to correspond with the great predominance of the movements guided by these sensations over all others. Next, in order of intimacy of connexion with the cerebrum, and also of size, come the optic ganglia or tubercula quadrigemina; and this again corresponds with the fact that, next to the tactile sensations, the visual are of most importance in regulating the muscular movements. The auditory ganglia, lodged in the substance of the medulla oblongata, are of smaller size, and have a less intimate connexion with the cerebrum; in accordance, as it would seem, with their inferior concern in the regulation of the voluntary movements. But they are in immediate relation with the central connexions of the nerves exciting the actions of the larynx and of the muscles of articulation; and thus serve the important purpose, as we have already pointed out, of regulating the movements of the organs of speech. And the olfactive ganglia, of which the ganglionic matter in man is present in comparatively small amount, are the most removed, of all the ganglia of special sense, from intimate connexion with the cerebrum; and they are the least concerned in the direction and regulation of the voluntary movements. In regard to the sense of taste, we need only remark that (as most of our readers are probably already aware) there is strong reason for regarding it as only a modification of that of touch; and if it has a proper ganglionic centre, this will be a portion of the vesicular nucleus of the medulla oblongata, in which the sensory division of the fifth pair, and the glossopharyngeal nerve find their termination. This position, too, would correspond with the slight participation which the sense of taste possesses in the regulation of the voluntary muscular movements; those of the tongue, in the examination of a sapid body, being the only ones which we can attribute to it.

Before we quit the subject of the sensory ganglia, we must remark that they appear to us to be the seat of the simple or elementary *feelings* of pleasure or pain, which are connected with different sensations. If we deny this, we must deny the existence of such feelings in animals whose encephalon consists entirely of sensory ganglia; which, we imagine, few will be disposed to do in regard to insects, to say nothing of inferior tribes. We shall presently attempt to show that, in animals possessing a cerebrum, these feelings may be associated with *ideas*, the impressions made by which are transmitted from it *downwards* to these ganglia; as well as with the *sensations*, whose impressions are received in an upward direction from the sensory nerves.

We now come to consider the general functions of the cerebrum; in which determination we are guided by subtracting from the sum-total of the functions of the nervous system all that it can be ascertained *not* to do. It would be interesting, if we had space to dwell upon such a topic, to trace the gradual limitation which has taken place in the physiological idea

of the functions of this organ ; from the period when it was considered as the centre of the whole life of the body,—the elaborator of the “ animal spirits,” whose circulation through the system by the nerves was regarded as essential to every vital action ; to the time of our present writing, in which the real extent of its agency is so much better understood. The withdrawal of the organic functions from its support or control, was the first step ; the proof of the independent endowments of the spinal cord was the next ; and the persistence of sensibility and of the power of consensual movement, after its removal, was the third great advance in this limitation. The cerebrum, then, has no concern in the purely excitomotor actions. It is *not* the sensorium, or instrument, by which the mind becomes conscious of sensory impressions. Nor does it participate in the performance of the movements, which are directly connected with these sensations. Nor does it seem necessary for the feelings of pain and pleasure, which appear inseparably connected with them. What then is its office ? In the first place we may attribute to it the formation of those *notions* or *ideas* respecting the objects of sensation, which are the materials upon which all the higher processes of mind operate ; the power concerned in which process, the process itself, and the result of it, are all known by the term perception. We may attribute to it, also, that power of recording sensations and the ideas connected with them, which we call memory ; and that power of recalling them, which is designated conception. Now it is easy to understand, that the direct communication between the cerebrum and the sensorium should cause the ideas excited in the former by sensations, to produce feelings of pleasure or pain in the latter, just as do sensations themselves ; and the same result will take place from the recall of past sensations and ideas, or the conception of those which are future, founded as this is upon the experience of the past. We lay much stress upon this view, that the *ideas* are *cerebral*, whilst the *feelings* of pleasure or pain connected with them are *sensorial*, for reasons that will presently become apparent. Its correctness seems to us borne out by a variety of considerations. We cannot see any reason for supposing, that there is a different seat of pleasure or pain for a remembered or anticipated sensation, from that in which these feelings are aroused by an actual sensation. The intermediate position of the sensory ganglia, receiving as they do the fibres from the sensory organs on the one hand, and from the cerebrum on the other, points them out to us as the seat of *consciousness*, not only of what is taking place in the *external world*, but also of the *operations of the cerebrum itself* ; and if of consciousness, then undoubtedly of the pain or pleasure so immediately connected with it. But we have a stronger ground than this, namely, that the very same consensual movements may be excited by remembered or conceived sensations as by the actual ones ; showing the exact analogy between the downward action of the cerebrum, and the upward action of the sensory nerves, in exciting changes in the sensorium. We might cite numerous examples of this kind ; the following will suffice. In persons with irritable stomachs, the act of vomiting may be excited by an offensive smell, a nauseous taste, or a disgusting sight ; and the remembrance or conception of similar sensations will produce the same effect. When the nervous system is in the condition, that renders the act of yawning easily excitable, it may be induced, not merely

by the sight or sound of yawning in another person, but by the mere *idea* of it, called up by the mention of the term. In like manner, in hydrophobia, the *idea* of liquids, called up intentionally or accidentally, is frequently as efficient in exciting convulsive paroxysms, as the actual sight or sound of them.

These considerations appear to us to lead to a view of the nature of the propensities and emotions, which is different from that ordinarily entertained, and which seems to us to harmonize better with our knowledge of mental operations. As we have already noticed, we can only judge of these propensities in the lower animals by the actions they perform; and we are likely to be in great error, if we always attribute to them the same desires or emotions as we ourselves experience, when we execute similar actions. For these desires or emotions involve *an idea* of the objects of them, such as is formed in ourselves for the purpose of stimulating to action the reasoning powers and the will; and in animals which have no reasoning powers, and in which the action is immediately prompted by the sensation, there is no reason to believe that such ideas or notions are formed. Let us take, for example, the sexual propensity. This, in the human being, may take the form of a desire, excited by sensations, some of them internal and some of them derived from without; and this desire stimulates the reasoning powers and the will to the operations necessary for the indulgence, or, it may be, for the restraint of that desire. But in the acts immediately preceding the *emissio seminis* (which is itself reflex) we trace the *direct* operation of sensations; and this appears to be the whole history of the process in those tribes, whose instincts are not restrained or directed by intelligence,—*all* the movements concerned being the direct result of the exciting sensations, in other words consensual. We may easily, of course, believe that the sensations are painful, when sexual intercourse is prevented, as they are pleasurable when it takes place; but we affirm that there can be nothing like a desire, unless there be a capability of forming an idea of the object of that desire, which the sensory ganglia alone are unable to do. Instead, then, of being that simple propensity which some would regard it, the so-called amateness is of a complex nature. In so far as the action referred to it is simply consensual, the abstract principle (as we have already shown in regard to the propensities in general, p. 504) has no real existence. And in so far as the propensity has the form of a desire, of which the mind is conscious, this desire is resolvable into the formation of an idea or train of ideas, which the mind feels it pleasurable to entertain. The first operation, the formation of the idea, is purely *intellectual*; the second, the feeling of pleasure connected with it, which is the cause of our continually dwelling upon it, and of its powerful operation upon the reasoning processes and will, appears to us evidently *sensorial*. In certain disordered states of the nervous centres, the sexual sensations may be so highly excited, that they act directly upon the muscular system, not only without the aid of the intellect and will, but even in opposition to it, as is seen in the violent semi-convulsive movements, excited in a nympho-maniacal patient by the sight of one of the opposite sex.

In like manner, we might analyse the propensity to imitation; and might show that it is partly sensorial, partly intellectual. Thus there is

some imitation, which is altogether consensual; the sensation, in each case, immediately exciting the movement. This is the mode in which infants learn to perform many of the actions which they witness in adults; and in which many adults are led to imitate involuntarily, and even unconsciously to themselves, under circumstances where they would least wish to do so. Take, for example, the case of yawning, before referred to; in which the operation of this consensual power is peculiarly evident, as it is impossible to execute a thoroughly good yawn by an act of volition alone. Now it seems absurd to refer such an action to an abstract principle of imitation, and to attempt to localize this principle; when we can trace a connexion as direct and immediate between the sensation and the muscular movement, as we can between the impression and the respondent action in the case of a movement excited through the spinal cord. On the other hand, there is a propensity to voluntary imitation,\* as shown in a fondness for mimicry, &c.; and this, like the amative desire, is resolvable into the intellectual *idea* and the *sensorial pleasure* accompanying it.

In like manner we might analyse all the propensities, emotions, and moral feelings; between which and the intellectual operations, phrenologists would establish such a marked distinction. What is benevolence, for example, but pleasure in the contemplation of the happiness of others? What is the whole class of selfish propensities, on the other hand, but the feeling of pleasure in the entertaining of various ideas connected with self? What is combativeness, but the pleasure of setting one's self in antagonism with others? Or what is veneration, but the pleasure of contemplating rank or perfections superior to our own? What is hope, again, but the pleasure derived from the anticipation of future enjoyment? Or what is cautiousness, but a mixed propensity, resulting from fear or anticipated pain, (to which phrenology does not assign a separate organ,) combined with pleasure in the contemplation of precautions adopted to ward it off? The connexion between the elementary feelings of pleasure and pain, and the operation of the various propensities, is universally admitted; the usual explanation of this fact is, that the indulgence of the propensities is a source of pleasure, whilst the restraint of them is attended with pain. Now, on the other hand, we consider a proper analysis of the processes in question to show, that the propensity cannot exist without a distinct *idea* of the objects to which it is directed; that the existence of such ideas, like that of the sensations from which they originate, is attended with pleasure or pain; and that, according to the respective degrees, in which these feelings attend particular classes of ideas, (as to which there is obviously a difference among individuals, analogous to that which exists with regard to the feelings of pleasure or pain excited immediately by sensations,—the likes and dislikes of different articles of food, for example,) will be the disposition of the mind to entertain them, the frequency with which they are brought before it, and the influence they are allowed to exercise in the formation of our voluntary determinations.

We believe, then, that Dr. Carpenter is perfectly correct in maintaining

\* In this manner we can understand a fact, of which, though sufficiently common, we never met with a professed phrenologist who could give us an explanation,—that the tendency to involuntary imitation may exist very strongly in persons who are utterly unable to mimic or imitate voluntarily, and *vice versa*.

that the offices of the cerebrum are restricted to *intellectual* operations; understanding, by that term, the operations which are concerned in the formation of a voluntary determination; and the movements consequent upon which are expressly adapted to an end distinctly conceived in the mind of the individual. But this allows full weight to the operations, through the instrumentality of the cerebrum, of what are commonly termed the emotions, as the *active principles* in the guidance of our conduct; since, as just shown, the emotions are not simple but composite states, the formation of ideas being the part of them that is performed by the cerebrum, whilst the sense of pleasure or pain, which is the real *feeling* connected with them, has its seat in the sensory ganglia. And the objection, brought by Mr. Noble and Mr. G. Combe against Dr. Carpenter's views,—that the most active principles of the mind, the mainsprings of the greater number of intellectual operations, are thereby located in a portion of the nervous centres, whose size bears no relation to their importance,—falls to the ground when those views are thus qualified. In the form, in which we have now endeavoured to develope them, they fully explain the mode in which emotions, directly excited by sensations, or recalled by an act of memory, may act upon the nervous system, in such a manner as to produce movements, which themselves belong to the consensual group. We may take the case of laughter as an illustration. The consensual nature of this action is proved by the facility with which it may be excited, in most persons, by the act of tickling; this peculiar impression (conveyed through the ordinary spinal nerves) being competent to excite reflex actions in the part itself, and in other muscles, through the medium of the spinal cord; but only having the power of exciting laughter, when it reaches the sensorium and is *felt*. In like manner the action may be directly excited by sights or sounds, to which no idea is at first attached, and which do not therefore operate by calling up an emotional state. We say that such things are ludicrous, when we come to consider them, simply because they excite involuntary laughter; and we can give no further account of them; and, as in the case of pleasure or pain connected with particular sensations, a sight which provokes one person to laughter may be perfectly inert as regards another. In many cases of this kind, the *will* is strongly exerted to check the consensual movement; successfully or otherwise, as the case may be. The character of the action is further shown by observation of infants, in whom smiles and laughter obviously result from the stimulus of sensations, before they can have any ideas connected with them. But the more ordinary cause of laughter in the adult is a ludicrous *idea*, which is aroused by the sensation; and the seat of this we regard as cerebral. The sensation itself, which was conveyed to the sensory ganglia, was not of a kind to excite the respondent motion, and passed on to the cerebrum without doing so; but the idea excited there reacts upon the sensory ganglia, and produces in them the condition which was excited in the former case by the sensation, and thus calls forth the respondent motion. And the same results will ensue, when the idea has been long stored away in the mind, and recurs involuntarily, or is called up by an exertion of the will. In any such case the presence of the idea, instead of acting upwards upon the reasoning processes, operates downwards upon the body, and discharges itself (so to speak) in an in-



voluntary or emotional action. The emotional states, when they do not thus immediately affect the muscular system, become the subjects of higher mental operations, and influence the determinations of the will, so as to be most important agents in the regulation of the voluntary movements. This double action of the emotions, which was long ago pointed out by Bishop Butler, and which has not been (in our opinion) sufficiently considered either by physiologists or metaphysicians, appears to us to find its best and simplest explanation in the two series of operations which we have attempted to develope,—the one involving only the very simplest cerebral action (the formation of ideas), which at once reacts downwards upon the body through the sensory ganglia, which give to those ideas the feelings connected with them,—whilst in the other, those ideas act upon the higher mental processes, and affect the body only through the will. And there is a well-known fact in our psychological and physiological history, of which this view seems to afford a particularly apposite explanation,—namely, the relief afforded to violent mental emotions, by giving free scope to the bodily manifestations of them. Every one knows the discomfort of suppressing a yawn, how much it adds to our fatigue, and thus makes us increasingly desire the cessation of the discourse to which we are listening. We know, too, how a ludicrous idea continues to haunt us, if we do not let it explode with a hearty “guffaw.” And we believe that it is a matter of equally familiar experience on the other side of the Irish Sea, that, when the combative and destructive propensities are strongly excited, they commonly evaporate speedily with the free play of the shillelagh; being much more likely to produce an enduring resentment, which shall call the intellect into exercise for its gratification, if the desired fight be prevented. This we believe to be the true physiology of the relief which some irascible persons find in a hearty d——n, in a violent slamming of the door, or in any other bodily effort to which the emotional condition prompts. The depressing emotions, in like manner, are often worked off with a good fit of crying, sobbing, &c.

We have dwelt long upon these topics, because we are not aware that they have been anywhere handled in a manner at all satisfactory; and having thus endeavoured to limit the functions of the cerebrum to that which we have a fair ground, from anatomical, experimental, and metaphysical evidence, to believe it to perform, we shall briefly inquire how far these views are applicable to the phrenological system as at present held, and what modifications they tend to introduce into it. But it may be desirable to recapitulate briefly the positions upon which we have now dwelt.

1. That the sensory ganglia supply all the conditions requisite for the reception of sensations, in the higher animals as in the lower; and that there is a class of actions excitable through them by the direct influence of sensations; to these we give the name of consensual.

2. That the sensations which excite these actions, also excite the feelings of pleasure and pain, which have their seat in the same ganglia. These feelings may receive different designations, according to the nature of the objects towards which they are displayed. Thus, attachment and dislike, affection and rage, joy and sorrow, and many other simple and elementary feelings, are but modifications or phases of pleasure and pain-



which receive their different designations according to the character of the objects which excite them, the ideas which they arouse, and the mode in which they are manifested.

3. That sensations, the simple feelings connected with them, and the consensual movements to which they prompt, make up the sum-total of those operations to which the term *instinctive* is properly applicable; that these take place through the instrumentality of the *sensory ganglia*; and that none of those higher operations which involve the formation of ideas, reasoning processes, and volitional determinations, can take place without a cerebrum.

4. That the cerebrum is the seat of the formation of *ideas*, or elementary notions originating from sensations, and of all those higher *intellectual operations*, of which those ideas form (as it were) the pabulum.

5. That the occurrence of *ideas* in the cerebrum may produce feelings of pleasure or pain in the sensory ganglia, analogous to those which are produced by *sensations*.

6. That the tendency to the recurrence of a certain class of ideas, constantly connected with feelings of pleasure or pain, constitutes what is known as an emotion, desire, or propensity; and that this is composite in its nature, involving the cerebrum for the formation of the ideas, and the sensory ganglia for the feelings with which they are associated.

7. That certain ideas, which thus strongly excite the feelings, may also produce motions through the instrumentality of the sensory ganglia and their nerves; that these movements are involuntary in their character, and are excited by emotional states in the same manner as they are by direct sensations; and that they consequently belong to the consensual group.

8. That intellectual operations *may* take place, in which the feelings do not participate (as, for example, in mathematical or scientific ratiocination); but that the motives which regulate our personal conduct are, in great part, derived from the feelings attached to particular ideas or classes of ideas. When the emotional states thus act, in affecting the further course of the mental operations, they have no immediate agency upon the body; their influence being exerted through the will.

9. That the exertion of the reasoning powers, and the final determination which, in its action upon the body or the mind, we call volition or will, operates solely through the instrumentality of the cerebrum.

10. That the cerebrum has probably no direct connexion, however, either with the sensory organs or with the muscular system; but that it depends upon the sensorial ganglia for the reception of sensations, and for the execution of voluntary movements; this execution being still guided by the sensations received through these ganglia, and the act of muscular contraction being dependent upon their continuance.

The first and chief point of collision between these views and the ordinary phrenological system, is that which relates to the localization of the passions, emotions, &c.; but it would not, we think, be found difficult to reconcile the two, so far as this question is concerned. For it may freely be admitted that there *are* such classes of ideas as those grouped together under the terms benevolence, combativeness, philoprogenitiveness, or destructiveness; although the pleasure attending the act of entertaining them

which causes them to be habitually kept before the view of the mind, and thus gives them the character of propensities, be not seated in the cerebrum, but elsewhere. Now if we find reason to adopt the phrenological system as a whole, the only modification it would require, would be to regard the different divisions of the cerebrum, commonly termed *organs*, in the light of instruments for the formation of the several classes of *ideas*, instead of being the instruments of the emotions or propensities, taken as a whole, into which the ideas so largely enter. And we think that our view further harmonizes well with the fact, that the exercise of *every* faculty, perceptive or intellectual, as well as moral or animal, may be attended with pleasure or pain; and that we may put the intellectual faculties into the form of propensities, just as readily as we do the passions and emotions. For instance, there are individuals who feel a delight in following out a beautifully-connected train of scientific reasoning, which terminates in evolving a simple principle adapted to explain a great variety of complex phenomena,—or in recognizing an analogy between facts apparently remote, which becomes the key to generalization or induction,—fully as keen as that which another will experience in carrying into operation a plan dictated by enthusiastic benevolence, or which will fill the whole nature of a third, in devising some scheme from which he anticipates a large amount of personal benefit. The tendency to exercise the reasoning powers, or to search for these analogies, which result from the pleasure thus experienced, must be admitted to be as much a *propensity* as benevolence or acquisitiveness; and if it be admitted that the former is a complex state, involving the capability of a certain kind of intellectual operation, and the capability of gratification from the exercise of it, we can see no reason why the same method of analysis should not be applied to the other. The different classes of ideas may still be called perceptive, intellectual, moral, and animal, according to the character of the objects to which they relate.

We have no *a priori* objection to make to the doctrine, that these different classes of ideas may be formed by the special instrumentality of different parts of the cerebrum. We freely admit, too, that there is a general correspondence between certain forms of cerebrum, arising from the relative development of its different portions, and certain leading diversities of character; which might not unfairly be regarded as indicating that these several divisions are the special instruments of particular groups of intellectual or moral faculties. We by no means desire to hold back from the inquiry, whether this inference is not a most legitimate one, and whether it may not be further carried out, by the subdivision of these regions amongst the elementary faculties, tendencies, &c., so as to be able to assign to each part of the surface of the cerebrum its special function, and, on the other hand, to point to the instrument of every one of the mental operations. That every act of mind, of whatever character, is accompanied by a *material change* in the nervous centres—the two being so necessarily linked together in our present state of existence as to be altogether inseparable—we most firmly believe; and there is no medium, in our apprehension, between the supposition, that the whole cerebrum acts together in every mental change, however simple, and the idea that different parts of it are appropriated to different elementary processes,

and that they are called into distinct or conjoint action, according to the nature of the mental action that is going on. The latter view appears to us to derive support from various anatomical facts, especially from the frequency with which we are enabled to prove, that large ganglionic masses are made up of distinct parts, having really dissimilar functions; and from the gradual increase in the complexity of the cerebral structure, arising from the successive addition of new parts, which we encounter in tracing it from its first appearance at the bottom of the vertebrated scale, up to its highest form in man. Comparative anatomy thus seems to lend us important aid in establishing the fundamental principles, or axioms, of phrenological science; for if the cerebrum were an organ as homogenous in *function*, as it appears to be in *structure*, we should expect that its higher development would consist only in an increase of *size*, instead of involving (as it manifestly does) an increase in the number of its lobes, the formation of convolutions, and a gradual increase in the number of the latter, together with analogous complications of internal arrangement. The doubts which we feel called upon to express, as left in our minds after a careful review of the whole subject, with the aid of Mr. Noble's replies to the various objections and criticisms quoted by him, relate, therefore, not to phrenology or cerebral physiology as a proper object of pursuit, or as a legitimate department of scientific inquiry, but to the reception of the present system, as one founded upon a correct induction of the phenomena which it should comprehend, and as expressing the whole, or nearly the whole, of the truth regarding the psychical nature and operations of man. These doubts we shall class under two heads, as they arise from physiological or from metaphysical considerations.

In the *first* place, the facts of comparative anatomy seem to be very far from coinciding with the *main features* of the system, in regard to the respective functions of the three lobes of the cerebrum; since, as we have already stated, the rudiment of the cerebrum which exists in fishes, and which is still more developed in reptiles and birds, is the representative of the *anterior* lobe alone of the human brain; the middle lobe being first developed in the lower mammalia; and the posterior being not merely restricted to the higher, but being more developed in man, relatively to the remainder of the mass, than it is in any other animal. Now, as upon the usual phrenological allocation, the lower or animal propensities are situated in the posterior lobe, their instrument would seem altogether undeveloped in the beings in which they apparently possess the greatest force; whilst they attain their most complete evolution in that species, which is distinguished by his power of keeping them in subjection. We have already shown that the usual phrenological explanation of this difficulty, confirmed as it is by the history of human embryological development, is altogether incompatible with facts. But we will offer another which may tend to remove the apparent discrepancy. We have already pointed out that we are not always to regard those *actions* of the lower animals, which correspond with our own, as indications of the existence of *propensities* in them corresponding to those from which they emanate in ourselves; these propensities being, in fact, the *states* intervening between the exciting sensations and the resulting will, and being compounded of ideas and feelings. Now in beings which are altogether destitute of a

When the cerebrum of one of the higher vertebrata is entirely removed, the animal is reduced to the level of one of the invertebrata, which is naturally destitute of the organ; and it is important to ascertain, if the actions which it then performs are of the same nature as those, which are so characteristic of the latter. But, in judging of the results of such vivisections, it is important to bear in mind, that the functions of the remaining organs cannot but be impaired by the removal of a large part of the encephalic mass; and also that these functions, in the un mutilated animal, are by no means so obvious in its actions, where a cerebrum is present, as where it is altogether wanting. Thus, whilst nearly all the actions of insects, which are not reflex, may be regarded as the direct results of the exciting sensations, the actions of birds, whilst presenting a general similarity with them, are more obviously influenced by intelligence, and are, therefore, for the most part referrible, not so much to the consensual group as to the volitional. The variability of the actions of different individuals of the same species, under similar circumstances,—the actions themselves all tending to the same end, which is suggested and kept constantly before the mind by the prevailing sensation,—becomes very striking in the class of birds, when we closely scrutinize their habits, and contrast them with those of the other class, to which they bear so remarkable a general analogy. Making such allowances, then, we think that experimental inquiry affords satisfactory evidence of the identity in the condition of a bird or mammal, whose cerebrum has been removed, with that of an insect or other invertebrate animal which naturally possesses none. In the first place, we think it impossible to analyse and compare the results obtained by different vivisectioners, without coming to the conclusion that *sensibility*, both of a general and a special kind, remained after the removal of the cerebrum. Doubtless there are fallacies attending the deduction of any such inferences from the movements performed by the animal under various stimuli; since a large proportion of these movements may fairly be set down as of a reflex character, and as not indicating sensation. It is safer, therefore, to draw our conclusions from movements consequent upon such impressions made upon the organs of *special sense*, as cannot (so far as we are aware) excite movements without the intervention of consciousness. And it appears to us to be an inevitable deduction from these phenomena, that the animals retained the possession of the senses of sight and hearing, though in a very imperfect manner. It is not a little curious, that M. Flourens, who at first drew a contrary inference from his experiments,—maintaining that the removal of the cerebrum destroys all sensibility,—should have since abandoned this opinion, in favour of the one put forward by Cuvier and his colleagues, in their well-known report upon M. Flourens' first Memoir; having substituted, in the second edition of his 'Experimental Researches,' the term *perception* for *sensation*, whenever he speaks of the function which is destroyed by the removal of the cerebrum.—Secondly, in animals deprived of the cerebrum, consensual movements were excited by the sensations. Thus, a bird would follow the light, and it would avoid objects placed in its way; it would start from its nocturnal sleep, raising its head and opening its eyes, upon the slightest noise; and it spent a great part of its time in pruning its feathers and scratching itself. Even if we regard

the last of these actions as reflex, the preceding can scarcely be so considered. Making a fair allowance, then, for the disturbance in the condition of the whole encephalon, which is necessarily induced by the removal of so important a part, and for the small proportion which the simply consensual actions bear in the general movements of these animals, we think ourselves entitled to affirm, that the conditions essential for these actions remain after the removal of the cerebrum, and that we are not to regard that organ as having any necessary participation in them.

Other experiments prove how much the ordinary movements of birds and the lower mammalia are *directly influenced* by sensation. Thus, vertiginous movements may be induced in pigeons and rabbits, not merely by destroying one of the optic ganglia, but by simply blinding one eye, either by evacuating its humours, or by temporarily bandaging it. And we believe that the similar movements produced, in the curious experiments of M. Flourens, by lesions of the semicircular canals, are readily explicable upon the same principle,—perverted sensations in regard to the direction of sounds (of which it seems to be the special object of that part of the auditory apparatus to take cognizance) being thus communicated to the sensorium, and producing abnormal movements, the direction of which bears a constant relation to that of the canal. These and other similar experimental results, which are referred to by Mr. Noble with a sort of contempt, as not affording us the slightest knowledge of a precise kind in regard to the functions of the different parts of the encephalon, appear to us, on the contrary, when surveyed from the point of view to which we are led by comparative anatomy, to be in admirable harmony with the conclusions indicated by that mode of investigation.

Although the development of the cerebrum in the vertebrata appears to *supersede* the simple and direct actions of the sensory ganglia and the motor fibres proceeding from them,—and this, in a degree comparable to its superiority of size,—it by no means *alters* their function. The simply consensual movements gradually form a smaller and smaller proportion of the whole actions of the animal as we ascend the scale; just as in the invertebrata, we find the simply reflex operations of the lower gradually brought under the guidance and control of the consensual, which are so characteristic of the higher. The very same gradation may be traced in the human infant. No one can watch its actions with an educated physiological eye, without perceiving, that they are at first almost purely reflex; the nipple of its nurse being seized by the lips when they are applied to it, and the act of suction being performed, as Mr. Grainger's experiments prove, under circumstances that forbid any other idea being attached to them. But we soon find that it is guided to the nipple by the sense of sight, and also (we think) by that of smell, which appears to us to have much to do with this action; and the first movements which it performs, when it begins to take notice of surrounding objects, are evidently to be referred to the simply consensual group. This is just what we should expect from the fact, that the spinal cord and the sensory ganglia at its summit are the parts of the nervous centres which are first developed, and which most rapidly advance towards maturity. Their actions are sufficient for the human infant for a long period after its birth; and it is not until it is



are employed, so as to distinguish between the merely consensual actions and those which result from the operation of the will guided by psychical propensities. Until all the knowledge has been acquired that this kind of concurrent investigation is capable of yielding, we think it altogether unprofitable to attempt to investigate the cerebral physiology of animals on whose hemispheres there are no convolutions. We shall not stop to discuss here how far the respective observations of Gall, Vimont, Tiedemann, Leuret, and others, are valuable for the end we have in view; our conviction being, for the reasons already stated, that the whole subject must be investigated anew; and the figures and descriptions of all the authors we have referred to being far from sufficient, in our estimation, as the foundation of such an investigation.

Thirdly. We cannot feel at all satisfied, that the method of determination hitherto employed, as regards the human cerebrum alone and its functions is sufficiently trustworthy. The greater part of the observations upon which the details of the present system of phrenology rest have been made upon crania alone, or upon casts, which are still more fallacious. Now although we do not charge this method of observation with being as open to objection, in comparing different individuals of the same species, as it is in comparing different species with each other, yet still we think, that its results are likely to be erroneous and imperfect; for the following reasons. There appear to be, in different races of men, *typical forms* of skull, the general contour having little or no relation with the comparative development of different parts of the encephalic contents, but simply modifying their disposition in the cranial cavity. This assertion we make upon the authority of Professor Retzius, who has studied this department of ethnology more profoundly than even Dr. Prichard himself, and who has shown that the classification of nations, formed according to the shape of the cranium, has so little correspondence with that which would be based upon their correspondence or diversity of psychical characters, as to indicate that no inferences can be drawn from one class of facts in regard to the other. We do not think that phrenologists can hesitate in admitting the possibility of two brains, possessing the same relative development of their respective parts, being moulded into two different shapes, according to the form of cranium peculiar to each race; since they have always admitted the possibility of much more extensive displacements as results of disease. But further—and this is, perhaps, our most serious class of objections to the *present system* of phrenology—no external examination of the cranium, such as can alone be made on the living head or the unopened skull, can give any account of the form of the *base* of the hemispheres, which, being covered with convolutions equally with any other part, must be supposed to have a like participation in the cerebral functions. And even when the examination is made upon the cerebrum itself, the large surface of each hemisphere in apposition with the falx is altogether neglected. Further, as the inner surface of each hemisphere is always flattened against its fellow, no increased development of any part of it can manifest itself, except by thrusting out a corresponding portion of the external side of the hemispheres; so that a protuberance of a certain portion of the latter may be due to a cause altogether irrespective of increased development of



the organ to which it seems to belong. Now from the difficulty of determining the development of the basal portion of the cerebrum, and from the absolute impossibility (in the present state of our anatomical knowledge) of judging, even from the cerebrum itself, of the development of the internal portion of each hemisphere, it has happened that scarcely any attempt has been made to map out the *base* of the brain into organs, and no attempt whatever has been made to show what share is taken by the internal surface of the hemispheres in the psychical operations of man or any other animal; so that we are scarcely beyond the mark in asserting that nearly (if not quite) *one half* of the cerebral surface is *totally unappropriated*. Yet not the slightest ground can be adduced for the supposition that these unappropriated portions have any less participation in the operations of the intellect, the exercise of the moral feelings, or the influence of the animal propensities, than have the external and superior portions of the respective lobes. We have met with some phrenologists who were candid enough to admit this to be a difficulty of which they could not give any explanation; their belief being that further observation would lead to increased knowledge of the subject. But in no system of phrenology that we have consulted have we met with an admission on this point, at all adequate to the extent of the difficulty; for, be it observed, one of the great claims which is set up in behalf of the phrenological system is the completeness of the system of psychical philosophy which it presents; the whole series of mental phenomena being arranged under certain categories, each of which has its corresponding representative on the cerebrum. Now if this be the real state of the subject, what are we to believe as to the geography of the terra incognita not yet explored by phrenologists? Are there no "organs" in it? If there be, for what sort are we to look? And if there be not, how can their absence be explained?

The fourth and last of our sources of doubt, in regard to the physiology of the present system of phrenology, arises from the want of sufficient accuracy in those observations upon the human subject upon which it professes to be founded; and from the prevalence of the habit of attending to and recording the *coincidences* between certain cerebral developments and psychical manifestations, without due regard to the cases in which there is *no* coincidence. The difficulty of precisely recognizing the relative sizes of different organs from external examination of the head has often been acknowledged and regretted by phrenologists themselves. It leads to the formation of very different estimates of character from the same data, as we can testify from our personal experience; three extremely diverse accounts having been given of our own "developments," by three well-known phrenologists, in the course of a few months. In another case, which happened to no less an authority than Spurzheim, two very different characters were given of the same individual (a young lady) within half an hour, under the following remarkable circumstances. Dr. Spurzheim was requested to examine the heads of two young ladies, sisters, who so closely resembled each other in person that the wearing of a cap by one of them was necessary to enable even their parents to distinguish them, and who yet differed considerably in psychical character. The capless young lady having undergone the doctor's manipulation, left the room, put on her sister's cap, and returned for a second scrutiny, which was made under the impression that

the other sister had appeared ; and an extremely different statement of her character was then given. Now either Dr. Spurzheim *intentionally* varied his account, to meet what he believed to be the difference in character between the sisters ; or he was unconsciously influenced by the expectation of finding a difference in the developments, the want of any accurate method of measurement causing him to make an involuntary error in his estimate. Our readers may adopt which explanation they please ; but the fact, which we have heard upon the most excellent authority, indicates (however explained) that the absence of such a method, by which fallacies of either kind could be detected, invalidates all the observations of Dr. Spurzheim, and leads us to the suspicion that, in other cases also, the process of taking the developments is open to serious error. And further, there is an equal, if not a greater difficulty, in a large proportion of cases, in the acquirement of such a correct knowledge of the character of the individual, and of the *hidden springs* of his actions, (which are often very different from those which would be supposed to be in operation, even by his most intimate relatives and friends,) as shall enable us to ascertain with certainty whether a coincidence exists or not. Every one knows how readily, in phrenological examinations, as in mesmeric performances, such coincidences are caught at ; and we are satisfied, from instances which have fallen under our own observation, that a certain coincidence of the *conduct* with the line marked out by phrenological prediction, may result from a *character* very different from that, which the cranium was supposed to indicate, and which gave no indication of actions and feelings known only to the individual himself. All observation, then, which has reference to points of character, about which there can be any doubt or mistake, is valueless for establishing any organological system ; and the only valid indications are to be derived from the observations of individuals, in whom the manifestation, or the contrary, of particular faculties or propensities is so decided, that there can be no reasonable hesitation as to their existence to an unusual degree, or their deficiency. We must do Gall the justice to remark, that this was the method adopted by him in his investigations ; and if his followers had restricted themselves to the same practice, until a determination of organs had been thus effected, which should be generally satisfactory to phrenologists and metaphysicians, we believe that phrenology would be at present in a state of more real, though less apparent, advancement.

But all observations, however carefully made, however sagaciously interpreted, are utterly valueless, unless the failures are chronicled equally with the successes. Will any professed phrenologist, of known candor, come forwards, and say that he has done so, and that he has given his *entire* experience to the public ? We do not address our query to those whose minds are so engrossed by the spirit of system, as to consider their science so perfect and unassailable, that no objections *could* be brought against it, which they would be unable to explain away. Nor do we desire an answer from those who have long considered the propagation of phrenology the sole business of their lives, and whose notoriety entirely depends upon their advocacy of it. But we put the question, in thorough good feeling, to those who have become persuaded of the truth of the present phrenological system, after what they deem a sufficient course of inquiry ; who can say

in singleness of heart that they have no personal motive for upholding it ; and who are competent, from their habits of observation, from their knowledge of human character, and from their acquaintance with their own mental constitution, to judge when there is a real coincidence and when there is not. We lay great stress upon this last qualification. We all know more or less from our own experience the truth of the saying " we easily believe what we wish," and unless we carefully watch ourselves we are continually being influenced, in our scientific observations and deductions, by our feelings regarding the question upon which these bear. This influence may be exerted quite unconsciously, and therefore innocently, on the part of the individual himself, though it is frequently obvious enough to others ; and we ought, therefore, to be doubly on our guard against it. We think we perceive many traces of it, for instance, in the present work ; though no man is more capable than our author of pointing out a similar fallacy in the pretensions of the mesmeric system, or of detecting the sophistry that lurks in all accounts of wonderful cures resulting from the employment of a particular remedy or method of treatment. We believe that phrenologists, like mesmerists, and too many doctors, are continually subject to this kind of influence, which has the more scope for its exercise, the greater is the difficulty of making an *exact* system of observations. The chemist who can weigh his reagents with the greatest nicety, or the astronomer who can measure the movements of the heavenly bodies to a marvellous degree of accuracy, is far less subject to errors of this class ; yet neither is exempt from it in the process of forming deductions from such observations, as a survey of the history and present state of chemical and physical science would most abundantly show. In phrenology this source of error is liable to operate alike in cranioscopical observations, in the process of deduction from it as to the probable character, and in the estimation of the real character ; and it needs but a very little acquaintance with the doctrine of chances to be aware that the introduction of even a slight element of uncertainty into three distinct processes bearing upon one another, most seriously impairs the probability of correctness in the final result. We could not, therefore, by any means trust to the coincidences, even if honestly recorded with *all* the failures ; unless we knew the character and qualifications of the party by whom the observations were made. And our present estimate of the phrenological mind is not sufficiently favorable to lead us to accept the published records of the coincidences as a fair expression of the entire evidence on the subject. For example, we know of an instance in which Dr. Spurzheim pronounced the organ of number to be *deficient* in an individual who was at that time known as the " calculating boy," and who is now an engineer distinguished for his readiness at computation ; and we have known the *absence* of the organ of colour to be stated by an eminent London phrenologist as the only remarkable point about the head of a man who was possessed of such powers as a modeller as to be able to produce an exact coloured representation, by the aid of memory alone, of any object to which his attention had been directed. We have heard of many such failures, but they do not find any record in phrenological publications. And the reason why they are not duly chronicled by the opponents of the system, and set in array against the imposing

results of Mr. A.'s visit to a prison, Dr. B.'s to a madhouse, and Mr. C.'s to a school or factory, is simply because no one has thought it worth while to accompany Mr. A., Dr. B., and Mr. C., upon their various perambulations, and to note the results throughout. We do not mean to assert that the failures *would be* so numerous as to invalidate the positive results, when both are fairly collected and compared; but we do assert that, *until* such a collation has been made, no more weight is to be attached to the records of positive or coincident observations, than we should admit to the marvels of clairvoyance, or to the accounts of the cure of consumption by a particular remedy, both which are detailed to us by numerous witnesses, who would not knowingly propagate an untruth. There is one more fallacy, which we would point out as resulting from the want of sufficiently accurate means of measuring craniological developments, and from the facility with which the mind of the observer is influenced by extraneous considerations; and this is, the information conveyed by the expression of countenance and general deportment, which, even when not a word is spoken, is often sufficient to afford a fair general knowledge of the character of the individual. We believe that many of the collections of observations, which seem to indicate the triumphant success of the present system of phrenology, especially those made in prisons and lunatic asylums, might be made with nearly, if not quite, equal success, by a good physiognomist; and until a fresh and impartial collection of such observations shall have been made, in which this source of fallacy shall have been excluded by the interposition of a mask, or by some similar means, we cannot feel that they ought to be relied upon as contributing, in any important degree, to the support of the existing phrenological system.

It is against *this system*, and not against phrenology, or cerebral physiology as a legitimate subject of investigation, that we have felt called upon to adduce the foregoing objections, derived from the want of correspondence between its dogmas and the unquestionable facts afforded by comparative anatomy, and from the fallacious and insufficient character of the evidence on which it mainly rests. And we believe that it will share the fate of other systems, religious, ethical, and physical, which have been upreared with similar haste and upon a like insecure foundation; being destined, if not to be undermined and overthrown by more persevering and well-directed inquiry, at any rate to be reconstructed on a plan which shall embrace wider foundations, and be followed up with more securely-laid courses. It is no disgrace to the early framers of the system that it should now require such extensive modification; for the whole science of physiology has been remodelled within the same period, and the almost utter ignorance which prevailed when they pursued their labours, in regard to the special functions of the nervous system, has been in great part cleared away. That the Gall-and-Spurzheim system of phrenology should require, therefore, something more than the slight additions and changes it has undergone at the hands of Mr. Combe and his coadjutors, to reconcile it with the present comparatively advanced state of our knowledge of comparative anatomy and of general neurology, cannot be deemed a bold assertion. Nor should those who volunteer their aid in this reconstruction,—by bringing as materials their stores of knowledge, acquired by

modes of investigation totally neglected by the professed followers of Gall and Spurzheim, and by offering to unite their labours with those of the latter, if they can only be assured that a fair use will be made of their work, and that it will not be thrust aside for contributions of really inferior value, but presenting a more specious appearance,—be treated as opponents and destructives. We have endeavoured to earn for ourselves the credit of directing the public mind in the estimation of the present claims of mesmerism; and more recently we have ventured to question the time-honoured systems which at present constitute what is known as medical science. In both cases we have endeavoured to separate the specious from the real, to show how one doctrine after another, that at first commands the general assent of those to whom it is presented, is scattered to the winds when its pretensions are fully and fairly analysed, (a few grains of corn being perhaps left, after a whole bushel of chaff has been thus dissipated,) and to indicate the methods which will be more likely, in our estimation, to evolve solid and substantial truth. We have been endeavouring to do as much for phrenology, and would claim, on that account, to be regarded as the real friends of the science *as it may be*, in spite of the opposition of the professed advocates of the science *as it is*, for which we are fully prepared.

In order that we may put our readers in possession, so far as possible, of the whole state of the question, we must ask their attention to certain metaphysical difficulties, or rather deficiencies, attendant on the present system of phrenology. We should not think it necessary to point out these, were it not for the claims which phrenologists have set up, as the expounders of a new system of psychical philosophy, far more perfect than any which had been offered to the world from any other quarter; capable of explaining all the diversities and contrarieties of human character, and therefore better adapted than any system that can be formed independently of phrenology, to be the guide in education, in the treatment of criminals, and in many other momentous decisions which deeply involves the welfare of the human race. Such claims can only be set up by persons of very shallow acquaintance with the conclusions formed by intelligent observers of human character (to say nothing of professed metaphysicians, who are not always the best guides in such matters), antecedently to phrenology, or independently of it. We have known an individual, for example, who had been practically and most successfully engaged in education for upwards of forty years, assured by a leading professor of the phrenological system of education, that the “new philosophy” would afford him vast assistance in his vocation; but after sedulously attending the course of lectures, in which this system was expounded, our informant declared that he had not heard a single principle enunciated which had not been constantly in his view, from a time when the claims of phrenology were unknown in this country. We believe that it would not be difficult to show, too, that the advances made within the last thirty years towards a better understanding of the nature of mental derangement, and the improvements in the treatment of that condition, are due fully as much to the general progress of inquiry upon these subjects, as they are to purely phrenological investigation. But we are quite ready to admit, on the other hand, that the writings of phrenologists have done much to



accelerate that progress, especially by fixing attention upon the *propensities* as the chief springs of human action, and by bringing into prominent view the fundamental diversities in the characters of different individuals, resulting from original differences in the relative force of those propensities. And we are quite ready to give full credit to Gall, as the first (we believe) to enunciate clearly the true relations between the psychological character of man and that of the lower animals, by pointing out that, in the former, as in the latter, instincts are at work, as incitements to action; and that, in many of the latter, as in the former, these instincts are placed under the guidance of reasoning powers, more or less developed. But all these views may be and are entertained quite independently of the organology of phrenology, and cannot therefore be said to be in any way peculiar to the upholders of that science.

With regard to the merits and deficiencies of the purely psychological portion of the phrenological system, we cannot here enter upon them at any length. For a very able, though brief, examination of them, we may refer our readers to a valuable and impartial work,\* lately published by a gentleman, whose extensive acquaintance with the existing systems of psychology, and whose unprejudiced judgment, render him peculiarly capable of pronouncing an opinion on this topic. That he is by no means insensible to the benefits which science has derived from phrenological investigation, appears from the following conclusion to his preliminary remarks upon the subject: "It has, in a word, thrown a light upon our knowledge generally as to the functions of the encephalon, which did not exist before, and so far has conferred a benefit upon the science of man, which it were uncandid not to acknowledge." "But with these, its physiological researches," he continues, "as it appears to us, the whole of its advantages terminate." It is clearly shown by the author to whom we refer, that no organological comparison can suffice to evolve a system of psychology. "If every organ had its name and nature inscribed upon it by the Creator, then we should have a system of psychology at once; but, so long as this is not the case, we must observe and classify our mental phenomena by reflection before we can begin to map out the locality in which they are to be found." "Strictly speaking," he continues, "phrenology cannot reveal a single intellectual fact which was not equally known before; it cannot trace any points of human knowledge to their primary elements; it cannot perform in any case a single analysis of our complex notions; in a word, it can do nothing, allowing its facts to be true, but point out a certain connexion between two parallel series of mental and physical phenomena." If it be urged that the very circumstance of different feelings or faculties operating in connexion with certain portions of the brain is a clue to a correct classification of them, it may be fairly replied, with Mr. Morell, that "we did not require any phrenological aid to convince us that the animal passions, the moral feelings, and the intellect, present three different classes of phenomena, which cannot be perfectly resolved into each other." And in regard to the more detailed allocation, if we take into account the uncertainty and indefiniteness of phrenological investigation, and the admitted sources of error attending its determinations,

\* *History of Modern Philosophy*. By the Rev. J. D. Morell, A.M. 1846.



even if the fundamental principles on which they rest are conceded, we may well doubt "whether the slightest aid could ever be afforded by phrenology in analysing our mental phenomena;" nor can we yet believe "that a classification grounded upon the positions of the organs can be in any way so satisfactory as one which is grounded upon an accurate observation of the mental phenomena themselves." In fine, with regard to some of the most important problems of metaphysics and morals, phrenology has never attempted a solution, and cannot afford the slightest aid in the search for it. "The only thing it attempts is to ridicule the questions themselves; which is a method of treating them alike easy and ignoble." After glancing at some of these questions, Mr. Morell thus concludes: "We repeat, therefore, that phrenology ought to have taken its place as one branch of physiological investigation; that viewed in such a character it has succeeded in educing many interesting and valuable facts respecting the material changes which accompany the exercise of thought and feeling; but that, in attempting to take its stand as a system of intellectual philosophy, it has entirely mistaken its proper place, and totally failed in throwing any light whatever upon moral or metaphysical researches."\*

Only one portion of the encephalic mass now remains for our consideration, namely, the cerebellum. The anatomical distinctness of this from the other nervous centres is so marked that no physiologist entertains a doubt as to its speciality of function. With regard to the general nature of that function, there is an almost universal agreement amongst those physiologists who attach most weight to comparative anatomy and to experiment; whilst, on the other hand, an entirely different function is attributed to it by those who place the greatest confidence in observations upon man alone. In regard to the nature of the evidence supplied by comparative anatomy, we shall here only observe *in limine* that it affords us the means of readily contrasting the extreme states of development of the organ within the limits of the vertebrated series; for we may trace its gradual diminution, as we descend the scale, through the higher mammalia, the lower mammalia, birds, reptiles, and fishes, until we lose it altogether in the lowest of the latter class. Such a comparison we must regard as more likely to evolve the truth in regard to its function than any collection of observations upon the human cerebellum, in which the size and weight of the organ have not been most rigorously determined. Mere craniological observations we must regard as totally valueless, at any rate until a much more complete knowledge has been attained as to the influence of particular types of conformation of the skull; for the observations of Professor Retzius upon the varieties of form which the cranium presents in different races have indicated this among other facts,—that the *position* of the cerebellum may vary considerably, being much more horizontal in one case and more vertical in another, so as to correspond with a greater or less posterior protuberance, without any corresponding variation in the size of the organ itself. And we shall presently see that Gall was misled by his craniological observations, in estimating the influence of castration upon the condition of the cerebellum.

\* Op. cit., vol. i, pp. 411-26.

In regard to experiment, again, this organ affords peculiar facilities. It can be removed, partially or completely, without any very severe lesion of the remainder of the encephalon; and animals may long survive the operation, and indicate the results of the ablation of the organ, after the immediate effects of the injury have passed off. We do not hesitate to affirm that there is as great an agreement between the actual *results* of experiment upon the cerebellum, as there is in regard to those of any other class of physiological experiments, involving numerous precautions, and attended with peculiar sources of fallacy; although the *inferences* deduced from these results have varied, according to the preconceived notions of the respective experimenters. Of these variations the closest followers of Gall have not been slow to avail themselves, by way of excuse for putting aside the experiments altogether, as totally valueless for yielding any results that can at all be depended on; yet there have been not a few of the upholders of his general system, who have admitted the force of the evidence, and have adopted those deductions from it which are now received by most phrenologists, without finding it necessary to give up the doctrine of Gall regarding the connexion of this organ with sexual desire. Mr. Noble, however, is not one of these; and we must own ourselves completely at a loss to understand how his usual acuteness of perception should have so unaccountably deserted him, as it seems to us to have done, in the analysis and contrast of the experiments of Rolando, Flourens, Magendie, and others, into which he enters near the commencement of his book. The only solution, in fact, that we can offer of his want of perception of the fundamental accordance of *all* these experiments, is that he has been so blinded by his preconceived views, as to be able to perceive only the points of difference, which exist, as we shall presently show, rather in the minds of the several interpreters of the experiments than in the results themselves. Thus he says:

"It will thus be seen that no two of the above instances presented anything like coincidence in the results; but that, on the contrary, direct contradictions occur. Rolando's paralysis is met by Bouillaud's no paralysis; Flourens's *inability* to *regulate* movement is counterpoised by Magendie's *capability*, confirmed by Fodera's experience; and the same contradiction is seen throughout the entire history of these vivisections. There is no single fact recorded by one operator which is not counteracted in its tendency to any conclusion by the experience of some of the others." (p. 25.)

The ordinary physiological view of the functions of the cerebellum, founded upon the experiments above referred to, and confirmed by numerous others, as those of Hertwig, Budge, and Longet, which Mr. Noble does *not* cite, is that the cerebellum is the organ for combining and re-regulating *voluntary* muscular actions, in which the united action of many muscles is required; especially those concerned in locomotion, and in the maintenance of the equilibrium of the body. This harmonizes well with the fact, that *no cerebellum exists where there is no cerebrum*; and that there is a pretty general conformity between the relative development of the two organs. Where the movements are purely *consensual*, still less when they are *reflex*, no cerebellum can be required to regulate them; and we therefore are surprised that Mr. Noble should have quoted (p. 255,) the regular movements executed by the decapitated trunks of insects, as

having any bearing on the question. He surely does not mean to affirm, that in a man, whose spinal cord has been divided in the dorsal region, and whose limbs execute the most powerful reflex movements, any kind of stimulation will cause these limbs to execute the combined movements requisite for the maintenance of his body in the erect position, still less, to perform the act of progression. If this cannot be effected, the balancing and co-ordinating power must have its seat in the encephalon of those animals, whose movements are not purely reflex. We see abundant proof that, in the invertebrata, the encephalic power over the movements of the body is limited to the control and direction of the actions, which, in themselves, are individually reflex; thus, in one of Mr. Newport's experiments upon a centipede, the removal of the ventral cord from the middle of the trunk caused the complete paralysis of the members immediately connected with the abstracted ganglia, whilst the posterior members continued to move and to force the body onwards, in harmony with the members anterior to the part of the cord removed, so long as the latter were in action; but when the former *ceased*, in consequence of an obstacle placed before the animal, of which it evidently took cognizance through the medium of its eyes, the latter *continued*, in consequence of their removal from the control of the cephalic ganglia. We have no reason to believe, then, that the purely *consensual* actions, however complicated, require such an instrument for the combination of the muscular movements needed to execute them; since the complete absence of the cerebellum in invertebrata shows that all which the sensory ganglia effect may be accomplished without it. We may, therefore, infer with certainty, that all the consensual movements in man and the higher animals,—such as sneezing, laughing, vomiting, and the like—are independent of this organ, the muscles being called into combined action by the direct influence of the sensations. But a little consideration will show, that movements executed in obedience to the will, of which the cerebrum is the instrument, are likely to need some such special organ for their regulation; for the will seldom exerts itself *directly* upon the muscular system, if, indeed, it ever does so at all,—its power being restricted to the issuing a general mandate (so to speak) for the execution of a certain action, of which the intellect has conceived the idea; and the selection and co-ordination of the various instruments, by which that action is to be accomplished, being effected altogether without any power on our own parts to direct it. We doubt whether we can will to put *any* muscle into action, independently of the rest; our power being limited to the execution of a particular movement, in which we know that muscle to participate, but in which its action is accompanied by that of others, from which we cannot isolate it. The only cases in which we *appear* to have such a power over individual muscles, are those in which the movements are so simple as to require only the single action of those muscles for their performance.

It appears to us perfectly conformable, therefore, with our general knowledge of the relative operations of the encephalic centres, to suppose that an organ should exist for co-ordinating the movements of the different muscles, in the various actions, most of them possessing a considerable degree of complexity, which are performed by the centres of the motor system of nerves, in response to the mandates of the will. And the

anatomical connexions of the cerebellum with the sensory ganglia, through which the sensations are conveyed that *guide* the individual movements, and with the medulla oblongata, through which the motor impulses are conveyed to the nerves, harmonize well with such a supposition. The peculiar connexion of the cerebellum with the sensory ganglia was supposed by Foville to indicate that it is the actual seat of sensation; an idea at once negatived by the entire absence of the organ in invertebrated animals. We shall now proceed to inquire how far the above doctrine is borne out by the results of experiment, and how far the discrepancies pointed out by Mr. Noble should lead us to reject those results as valueless. We would first remark, however, that we must carefully bear in mind the difference between the results of the simple ablation of the organ, and the direct results of the injury which that ablation involves. All convulsive movements fall under the latter category; for, as will presently be shown, they subside as the shock and irritation pass off; whilst we are to look in the residual phenomena for the simple results of the loss of the organ. How much may be learned from the latter, after making all necessary allowance for the former, will appear, we think, from the following analysis.

In the experiments of Rolando, made in the infancy of our knowledge of the physiology of the nervous centres, the severe injury or removal of the cerebellum caused inability on the part of the animal to sustain itself upon its legs, or to execute any movements of progression; the animals experimented on soon died, however, with convulsive spasms and hemorrhage. Rolando was *previously* possessed with the view, that the cerebellum is the sole source of the muscular movements, and he represents the ablation of the cerebellum as producing hemiplegia or complete paralysis (according as one or both hemispheres were removed), disregarding the fact which he records, that movements both of the anterior and posterior extremities, *not* of a convulsive nature, were observed after the operation. These movements he attributed to the mere mobility of the muscular fibre, or to some remnant of the cerebellum! We may safely affirm, then, that these results are in no respect contrary to those of Flourens and others, in which no paralysis was indicated. Between the results of the experiments of Flourens, Bouillaud, Budge, and Longet, there is a complete coincidence in regard to the essential fact, that a mammal or bird, deprived of its cerebellum, cannot maintain its equilibrium or execute any regular movements of progression, although it can still move its limbs with energy in all directions. And parallel experiments upon the removal of the cerebral hemispheres fully prove, that the severity of the operation is itself not the cause of the result, since this much more severe operation does not produce it, animals being able to stand, walk, fly, &c., as well after the removal of the cerebrum as when it is entire. "Take two pigeons," says Longet, "and remove from one of them the whole of the cerebral hemispheres, and from the other only one lobe of the cerebellum; the next day the first will be firm upon his feet, whilst the second will exhibit the unsteady and uncertain gait of drunkenness." But, it is urged by Mr. Noble and other adherents of Gall's views, all these results are in direct opposition to those of Magendie and Fodera; in which the effects of the removal of the cerebellum appeared to be confined to the imparting of a tendency to a peculiar *direction* of movement,

the power of executing combined movements being asserted still to exist. Magendie found that division of one of the crura cerebelli caused the animal to continue rolling over towards the corresponding side, and that deep wounds of the organ occasioned a tendency to retrograde movement, even when the animal made an effort to advance. These disorders may continue for many days, or even weeks. In the experiments of Fodera, there was an evident disturbance of the motor function, but no constant result. The animals seem to have all died from hemorrhage within a very short period after the operation. Now, these results are readily explicable on the idea, that the sensory ganglia are the proper centres of all movement which is not reflex, for we find that any disturbance in the condition of the latter, or of the *guiding* nerves connected with them, may produce the same result. We have already referred to the tendency to move in particular directions, as a consequence of section of the nerves of the semi-circular canals, of the destruction of one eye, and even of a temporary obscuration of vision; the same result follows deep wounds reaching to these bodies; and it is not surprising, therefore, that lacerations of the commissural fibres connecting them with the cerebellum should produce a similar effect. Now that the irritation of these fibres is the cause of the peculiar movements in question, is shown by the experiments of Budge, who found that the movements would spontaneously subside after a time, but that they might be re-excited by irritating the cut surfaces of the crura cerebelli. With regard to the regular combined movements, said to have been observed by Magendie, we find none particularized but this,—that, on vinegar being held under the nose of a guinea-pig, whose cerebellum and cerebrum had been removed, it sneezed and rubbed its nose with its paws. These actions are evidently either consensual or purely reflex; and we might just as well quote the respiratory movements, or the efforts made by a decapitated frog to push away the probe that irritates its cloaca, or to grasp the female, as adduce the sneezing of a guinea-pig and the rubbing of its nose, when an irritating odour was applied, in proof of the power of co-ordinating the muscles for *voluntary* movements. In no record of experiments upon the cerebellum, save in those of Fodera, do we find that the capability of sustaining the equilibrium, or of executing combined and regular movements of progression, remained after the removal of the organ. The experiments of Fodera, as narrated by Gall, seem to have given but very imperfect results, partly in consequence of the very early death of the animals which were the subjects of them; and the narration is not sufficiently distinct to allow the real character of the movements to be made out. And as it is proved that the severity of the operation does not produce the result, we have no resource but to believe that the cerebellum is the organ directly concerned in the combination of muscular actions required to sustain the equilibrium or to produce these movements; or, on the other hand, to adopt Mr. Noble's hypothesis, that the admitted disturbance is a result of sympathetic affection of the motor apparatus. In regard to this hypothesis, we may remark, that it is partly founded on the imperfect discrimination, to which we have already alluded, between reflex and consensual movements on the one hand, and co-ordinate voluntary movements on the other; and partly also upon the want of due discrimination between the constant *loss* of power, consequent upon the



removal of the organ, and the occasional, but not necessary *disturbance* of the motor apparatus, which is equally occasioned, as we have seen, by other causes acting upon the ganglionic masses at the summit of the medulla oblongata. Between the two hypotheses, we think that the data supplied by comparative anatomy may assist us in forming a choice.

Before discussing these, however, we must make a few remarks upon the doctrine of Gall,—that the cerebellum is the seat of the sexual propensity. It seems to us that there are certain *a priori* objections to this, which have not been sufficiently considered, and which are not without weight, the uncertain character of the evidence in favour of the doctrine being a matter of notoriety. We readily admit that no *a priori* objections should stand in the face of anything like positive proof; but we cannot imagine that any candid phrenologist, who has duly considered *both* sides of the question, will affirm that such has been afforded. The first point we would urge is the evident *speciality* of the organ; not only as to its position and connexions, but also in regard to its internal structure. These seem to us to mark it out as an organ more distinct in function from the cerebrum, than the phrenological hypothesis would represent it, especially considering its very slight direct connexion with the hemispheres. For it cannot but be admitted, that the sexual propensity is as closely connected with many other propensities and desires,—e.g. with philoprogenitiveness, adhesiveness, and in the lower animals especially with combativeness,—as are any of the phrenological organs with each other; and that, in man, it has a continual operation upon the intelligence, the reasoning faculties, and the will. And yet we cannot point out any structural connexions by which this combination can be effected. Again the size of the organ, and especially its amount of vesicular matter, in comparison with the cerebrum, looking at man only, is much greater than we can fairly expect it to possess, if we contrast the degree in which this propensity exerts its influence in the well-regulated mind, with the constant and energetic operation of others. Though its force is, doubtless, greater in some individuals, and its operation on the character and conduct more constant than external observers might suppose, yet in other instances, the propensity appears to be totally absent during the whole of life. Thus it is recorded of Newton, that he never gave the slightest indication of the existence of the sexual desire. And we believe that the early repression of it by a voluntary effort, as in the case of Catholic priests, disposes to its entire cessation for the remainder of life. Now there is an utter absence of proof as to any *strongly-marked* deficiency in the size of the cerebellum,—ascertained not by external observation of the skull, but by the dimensions and weight of the organ,—in cases of this kind; in which there ought, one would think, to be an almost entire absence of it, either congenitally, or by subsequent atrophy. We are continually told by the followers of Gall, that we should form our estimate from *extreme* cases only; and yet the most extreme cases on record, as to the size of the organ in man (as ascertained, we again repeat, by direct measurement), are not to be put in comparison with the difference in relative size that presents itself as we pass from man to the animals most nearly allied to him. To one other point we have yet to allude, namely, the early development and completeness of the cerebellum in the human embryo and



infant; which is very remarkable when contrasted with the evolution of the cerebrum. We refer not merely to size, but to texture; the cerebellum acquiring the adult condition in this respect at a time when the cerebrum is still very soft and pulpy. This is precisely what we should expect, if its chief office be to regulate the movements of equilibrium, &c.; but is inexplicable if we suppose the cerebellum to be merely the organ of the sexual instinct.

We shall now take a brief review of the comparative anatomy of the cerebellum, with the view of determining to which of the preceding doctrines the data supplied by it give the most support. In the first place it is to be remarked that the organ is entirely absent in the invertebrata; yet sexual instincts are manifested most strongly in their actions. Here, as in the former case, we think a distinction must be drawn between the *instinct* and the *propensity*; the former being our expression for a certain series of phenomena directed towards a given purpose, but not really involving any other physiological or psychological actions than sensations and respondent movements; the latter being a desire for the gratification, involving an *idea* of the object. If we are right in this analysis, the sexual actions of insects, &c., may be purely consensual, and may not involve a *propensity* properly so called; and the absence of a cerebellum in them is not inconsistent with the phrenological doctrine. Neither is it opposed to that of Flourens and his followers, since all the movements of these animals are evidently of the consensual and reflex character.—The class of fishes may be looked to as furnishing important evidence; because, on the one hand, we meet with a greater diversity in the development of the organ than we encounter in any other class; whilst, on the other, there are equal diversities in regard to both classes of functions referred to it. The cerebellum presents itself in some fishes as a simple lamella, implanted transversely on the medulla oblongata, behind the optic lobes. In general, however, it is more extended longitudinally, and covers in the fourth ventricle. Its highest development is attained in the cartilaginous fishes; in some of which it is marked by transverse furrows, dividing it into lamellæ more or less distinct, and thereby increasing its extent of surface. Now it is well known that in a large proportion of the class of fishes there is no sexual congress; the fertilization of the ova being accomplished simply by the diffusion of the seminal fluid through the water in the neighbourhood of the spot where the eggs are deposited—the two sexes being led by their instincts to frequent the same localities for the discharge of the contents of their genital organs. Our knowledge of the habits of fishes in this particular must be admitted to be very limited; but still there would seem a broad line of demarcation between the copulating and non-copulating fishes, which might be reasonably expected to be indicated by a difference of size or conformation in the cerebellum, if this were really the organ of the sexual feeling. Again, the diversities in the mode of locomotion of fishes are extremely great. It is to be remembered that the correspondence between the specific gravity of the water and that of the body of the animal, renders the support and the balancing of the body a matter of comparative facility in all circumstances; and the chief differences are to be looked for in the manner of propulsion, and in the facility of combining different movements for the change of direction, or for the attainment of any other special object. In the eels and other fishes of a

similar shape, the vermiform character manifests itself as much in the movements as in the aspect of the body. The propulsion is almost entirely effected by the flexion of the entire trunk, the special organs being nearly undeveloped; and this flexion may well be regarded as being equally reflex in its character with the similar movements of articulated animals. But in fishes of higher orders the movements of the body become more and more complex, additional members are developed, and a much greater combination of different actions is required, in order to produce any desired result; whilst at the same time the variety of movements of which the animal is capable are very greatly augmented. This is especially the case with the most active predacious species, which are not satisfied with lying in wait for any unwary fishes that may pass near them, or with gulping down the small fry dispersed in the waters through which they move, but which pursue particular objects for vast distances, follow them with the greatest address, and adapt their movements most remarkably to the capture of their prey. There can be no hesitation, we think, in assigning to the shark tribe a pre-eminence in this respect.

Now on contrasting the size and development of the cerebellum in the copulating and non-copulating fishes respectively, there would seem, at first sight, to be something of that correspondence which might be expected on phrenological principles. For the sharks, which all copulate, possess the cerebellum in its most developed form; and the rays, which are nearly allied to them, and which also copulate, are also remarkable for the size of their cerebellum, which, though inferior to that of the sharks, is more developed than that of any other known fish. But here the correspondence ceases. The sharks and rays are by no means the only copulating fish; for a sexual congress takes place also in the chimæridæ, the syngnathidæ, the siluridæ, and in the blennies and eels. In regard to the development of the cerebellum in the greatest part of these, we have as yet no information; but it is well known that in the eels this organ is very small, being much below the average of the non-copulating fishes. Thus among the copulating fishes we find not only the highest but also a very low development of the cerebellum. Again, amongst the non-copulating fishes there are many which approach very closely in this particular to the highest group of the copulating; for whilst several of the rays are destitute of the lamellæ, which distinguish the cerebellum of the sharks, there are many of the non-copulating osseous fishes which have a cerebellum nearly (if not quite) equal in size and development to that of the rays, and far larger in proportion than that of the eels; this is the case, for instance, with the cod and the pike. These facts would seem to us decidedly unfavorable to the doctrine that the cerebellum is the organ of the sexual instinct. Let us now examine them under another point of view.

On comparing the development of the cerebellum in different species of fishes, with the activity and variety of the movements, a tolerably close degree of correspondence may be readily perceived. In the little amphioxus, which leads the life of a marine annelide, there is not a rudiment of this organ to be detected. In the lamprey and its allies, which are nearly as inert in their habits, attaching themselves to their prey by their suctorial mouths, and possessing but little power of active motion in search of it, we find the merest rudiment of a cerebellum. It is somewhat larger in the eels, which are fishes of considerable activity, though their

movements are of a very simple character. In the cod and other actively swimming species, in which the impelling movements are still chiefly performed by the undulations of the body and tail, but in which the balancing and directing powers are rather given by the fins (as has been proved by experiment), we find the cerebellum attaining a much greater development; whilst in the sole, turbot, and other pleuronectidæ, which pass the greatest part of their lives with one side of their bodies in contact with the bottom of the sea, this organ is comparatively small. The highest development of the cerebellum, as already mentioned, is to be found in the sharks, a tribe pre-eminent for the vigour and variety of their movements; and in the much-dreaded white shark, it far surpasses, in comparative size and complexity of structure, that of any reptile, nearly equalling that of many birds. It may be thought that the large size of this organ in the rays (which only yield to the sharks in this particular), is opposed to our inference, since these fish are not commonly reputed to possess much energy or variety of movement; but it is to be noticed that their progression through the water is accomplished in a different manner from that of most other fishes, and really involves a combination of movement which is very unusual in that class. The rays, in swimming, make very little use of the flexure of the body, which is the chief means of propulsion in the remainder of the class; the greater part of the vertebral column being unusually solidified, and even the tail serving merely as a rudder. Progression through the water is accomplished by means of the *combined* actions of the pectoral fins; and these are exerted so neatly and easily, that the fish appears to slide along without the slightest effort. "When a skate," says a competent observer, who had no purpose to serve in making the remark, "is swimming at its ease and undisturbed, its motion is peculiarly smooth and gliding, and puts one more in mind of a kite sailing along with motionless wings, than any other motion which we are accustomed to see."\* The same writer, after contrasting the varied and facile movements of the rays with the limited and laborious wriggings of the flounder tribe, thus concludes: "We have been somewhat particular in describing the motions and organs of motion of these animals; because, on account of the peculiarity of their forms, there is a very general notion among persons not acquainted with the economy of the sea and its inhabitants, that they are very unwieldy and helpless creatures in the water; whereas the fact is quite the reverse, and, even among those fishes, which are regarded as models of perfection as swimming animals, there are none who have more command, or even as much command, of themselves as the skate." This testimony from an unprejudiced observer proves that the high development of the cerebellum in the ray tribe most completely accords with the character of their locomotive powers, instead of being opposed to it, as seemed at first sight to be the case.

The cerebellum of reptiles is, on the whole, smaller in proportion than that of the fish. In no instance does it surpass in development that of the higher cartilaginous fish; and in general it is below the average of that of the osseous fishes. Thus in the frog it is a mere transverse lamina, not covering-in the fourth ventricle, and scarcely more developed than the cerebellum of the suctorial fishes. In the lizards and serpents it is but little larger in proportion; and it is only in the crocodiles and turtles that

\* Mudie, in *Cyclopædia of Natural History*, vol. iii. p. 514.

it presents any considerable dimensions. It is well known that all reptiles have a sexual congress: though it is not always that there is an actual intromission of the male organ. Like most other acts in this remarkable class, it is very leisurely performed; often lasting several days, seldom less than one. The strength of the sexual instinct in these animals would seem to be indicated by the remarkable *preludes* which many of them perform; and by the battles of gallantry which are common among others. The frog has often been quoted by anti-phrenologists as an example of an animal possessing a strong sexual instinct, with a rudimentary cerebellum; its sexual instinct being supposed to be indicated by the duration of its copulation, and by the perseverance with which it continues to grasp the female, even after decapitation. But Mr. Noble urges (p. 330), and we think with justice, that this powerful and continuous prehension, being a purely *reflex* act, and excited (as Messrs. Todd and Bowman have shown) by the peculiar impressibility of a papillary organ developed at that period upon the thumb, cannot be taken as an indication of the peculiar strength of the sexual instinct. But he would probably admit that battles of gallantry among animals at other times peaceably disposed, or a long series of preliminary *agaceries*, such as Rusconi has described in the salamander, are unquestionable indications of the force of this instinct; and yet among most of the animals which exhibit them, the cerebellum is relatively smaller than in fishes which do not copulate at all, and in which the emission of the semen seems to be an act performed under the same conditions as the contemporaneous extrusion of the ova. The sexual passion appears to be particularly strong amongst serpents: of which, if surprised during the coitus, the male defends the female, exposing himself to every kind of peril for her safety, and submitting to mutilation rather than desert her. The iguana and some other lizards display an equal attachment; and the affection manifests itself not merely during the time of coition, but subsequently; yet its cerebellum is relatively much smaller than that of the crocodile, in which the attention of the male to the female, after copulation, seems limited to helping her on her legs again—her position during the act having been supine. There is nothing in the known manifestations of the sexual passion in this class, that can at all account for the marked superiority in the relative size of the cerebellum of the turtles and crocodiles over that of the frogs and other batrachia.

Now, if we turn to the locomotive actions of this class, we think that a much closer correspondence is apparent. In the first place, the generally small size of the organ accords well with the very limited nature of the movements, and especially with the fact, that these movements are for the most part of a reflex character. The numberless experiments which have been made upon frogs, serpents, &c., fully prove that, not only the ordinary motions of progression, but almost all the actions performed by these animals—even such as seem the most *adaptive* in their character—are of a simply reflex nature. We should fairly expect, then, to find the cerebellum at its minimum of development, especially as, in consequence of the firmness of the support upon which the limbs and body rest in the terrestrial species, there is no need of any exertion to balance the body. The aquatic and semi-aquatic crocodiles, alligators, turtles, and mud-tortoises, however, need a much greater power of combining and regulating the movements of their limbs to sustain, balance, and propel

their bodies in the fluid medium, in which, though so helpless on land, they move with great facility; and, accordingly, we find that they have the largest cerebella of the entire class. It may be objected, that there are newts and serpents which are also aquatic, and that these, nevertheless, are not distinguished by a cerebellum of larger size than that of their congeners. But to this it may be replied, that *their* propulsion is not so much effected by the combined and regulated movements of several members, as like that of ordinary fishes, by the simple undulations of the vertebral column. The inference we have drawn from the comparatively high development of the cerebellum in the turtles, is remarkably confirmed by the fact, that this superiority is confined to the aquatic species: the land tortoise, which can have little need of any co-ordinating power for its simple and stable method of progression, presenting little or no advance, as regards the development of the cerebellum, upon the average of the class.

The cerebellum makes a great advance in development in the class of birds, being not merely of much larger size, but being also marked with numerous transverse lamellæ, which indicate an extension of the surface of gray matter, and being also furnished with lateral lobes, which are the first indications of the hemispheres of the cerebellum. There is not by any means the same difference in its relative size in the several members of this group, as exists in the two preceding classes; and the inferences to be derived from its study, therefore, are less decided than those with which we seem to be furnished by the comparative investigation of its development in fishes and reptiles. Such indications as are afforded, however, by no means favour the idea that this organ is exclusively connected with the sexual passion; for we find that the polygamous fowls, with whose extensive gallantries we are all familiar, have relatively a much smaller cerebellum than the falcons and swallows, which bestow their sexual feelings on a single female. Among the monogamous birds it is very curious to see how constantly, when they have once paired, the male restricts his amorous attentions to his mate, never going astray after any other female, and how uniformly the female rejects the addresses of any other wooer, affording in this respect, as Buffon long since remarked, an excellent pattern to husbands and wives of more elevated rank in the zoological series. Yet this constancy cannot result from the relatively small size of the cerebellum, for the birds which exhibit it have the largest cerebella of the entire class, whilst the licentious chanticleer has among the smallest. Nor can it be said that birds are on the whole less amorous than mammals, in conformity with the inferior development of their cerebellum; for there are no animals in which the size and functional activity of the sexual organs undergo such a marked increase at the period of propagation as they do in birds. On the other hand, the degree of difference which exists in the relative development of the cerebellum, in the several tribes of this class, seems to accord well with the relative amount of necessity which exists for the co-ordination of different muscular actions in the various movements they execute. Thus, the birds which live chiefly upon the ground usually have smaller cerebella than those which spend much of their time on the wing; and, amongst the latter, those which pursue a living prey, and which have need of unusual power of directing and balancing their movements—such as the falcon, kite, and swallow—have a



cerebellum exceeding in size the average of the class. The birds of the parrot tribe, which are distinguished by the conformation of their feet, and for the use they make of those members in prehension, also have a very large cerebellum—probably the largest in the entire class—although we are not aware that these birds are remarkable for their amative propensities.

Lastly, in the mammalia we may trace a gradual increase in the development of the cerebellum, from a grade scarcely or not at all above that which it presents in birds, to the large size and remarkable complexity which it possesses in man. This increased development is to be noticed chiefly in the lateral portions or hemispheres, which, instead of being mere appendages to the central portion, gradually come to constitute the chief bulk of the mass. The organ presents its lowest development in the rodentia and marsupialia, many of which are remarkable for their amative propensities, and for the persistence of these, under favorable circumstances, through the whole year. It is somewhat larger and more complex in structure in the ordinary hoofed quadrupeds; and its size and complexity present a further increase in the carnivora. But it is as we ascend through the quadumanous series, from the *four-footed* rather than *four-handed* lemurs, through the baboons, the monkeys, and the semi-erect apes, up to man, that we observe the most remarkable increase. Now it cannot be affirmed, that there is any corresponding increase in the sexual propensity; for we should be sorry to believe that the well-known lasciviousness of the monkey tribes is surpassed by that of the human race, in a degree at all proportional to the relative size of the cerebellum, which is at least six times larger in the latter than in the former. But there is, on the other hand, as we ascend this series, just such a rapid increase in the number and variety of movements, which the limbs are capable of performing; with an increasing power of balancing the body upon the posterior extremities, which produces an augmented freedom of the anterior; whilst, at the same time, the development of the thumb gives a vast increase to the variety of purposes to which the hands may be applied. None but those who have studied the subject can be aware, what an extraordinary co-ordination of muscular movements is required, for the most common acts of human progression. The erect figure, the small size of the feet, the length of the stride, and a variety of other circumstances, interpose a set of mechanical difficulties, which no human skill has yet been able to overcome. We have seen automata of various kinds,—a writing and drawing automaton,—a flute-playing automaton, with divers others,—and more recently a speaking automaton; but we believe that no walking automaton was ever constructed, though the attempt has been often made. In the most remarkable collection we have seen, which included singing birds and crawling spiders,—the latter so well imitated as almost to send some of the ladies present into hysterics,—we remember to have seen one of these attempts; which, although the labour of many years had been bestowed upon it, was nothing more than a figure standing upon two legs, and advancing upon a table by the cautious *sliding* of the feet one before the other, the foot being never lifted from the ground, and not being advanced more than its own length at a time. We notice this circumstance, to show how, in this one act alone, simple as it appears, a much greater and more harmonious combination of different muscular actions is required than is involved in the progression of the lower mam-



malia; and we need hardly point out, we think, how remarkably man surpasses all others, in the power of adapting his muscular contractions to perform any movement which he desires to execute.

On the other hand, there is nothing either in the strength nor the constancy of the sexual propensity in man, which can at all account for the great superiority in the relative size of his cerebellum over that of every other animal. For the means we have proposed for reconciling the phrenological doctrine of the functions of the posterior lobes with the fact of their absence in the lower mammalia, and their peculiarly large size in man, will scarcely hold good here; since the formation of *ideas*, which are essential to the action of the sexual sensations upon the will, and which are associated with feelings of pleasure,—the combination of the two forming a propensity,—can scarcely be any thing else than a *cerebral* operation. And we see no reason whatever for placing the seat of the sexual sensations, and of the origination of respondent movements, any where else than in the sensory ganglia, which are evidently their instruments in invertebrata.

It is well known that, among other arguments by which Gall upheld his view of the function of the cerebellum, he maintained that the size of this organ is less in castrated animals than in those possessing their sexual organs entire. He does not, however, appear to have made any very exact observations in support of this position, which he principally rested on a comparison of crania. Subsequent observers have not confirmed the statement. Even M. Vimont admits that the result of his experiments was doubtful. Desirous to put the question to the test, M. Leuret caused his former collaborateur, M. Lassaigne, (of whose qualifications as a scientific observer, Mr. Noble expresses his ignorance,) to weigh the cerebra and cerebella of a sufficient number of stallions, mares, and geldings, and compare the results. A table of these weights, with that of the medulla oblongata, is given by M. Leuret,\* for every one of ten stallions, twelve mares, and twenty-one geldings; and the result is that the cerebellum of the gelding is, on the average, *both absolutely and relatively larger* than that either of the stallion or the mare. The manner in which these results are treated by Mr. Noble is, we are constrained to say, rather too characteristic of the general mode in which phrenologists treat evidence that does not suit their purpose. In the first place, not having seen the entire tables, but only the summary of them given in Dr. Carpenter's "Human Physiology," he makes objections which a reference to the tables would prove to be unfounded. His especial ground of refusal to admit the validity of these results, is the smallness of the number of observations, and the consequent uncertainty of the averages; but if he will refer to the tables themselves, he will see that even the *extremes* are as remarkable for the evidence they afford as are the *means*. Thus, the weight of the cerebellum of the stallion is to that of the cerebrum in the stallion, as from 1 : 7·46 in the *lowest*; to 1 : 6·25 in the *highest*, the *average* being 1 : 7·07. In the mare, the proportional weight of the cerebellum is greater, not from the larger absolute size of that organ, (as Mr. Noble would make out M. Leuret to assert, contrary to what he states is the invariable fact), but from the smaller size† of the cerebrum;

\* Anat. Comp. du Système Nerveux, tom. i. p. 427.

† We have discovered an error of calculation in M. Leuret's table, in regard to the weight of the cerebrum in Mare No 7. The number (deduced from the weight of the entire encephalon) ought to be 436, instead of 336, and the proportion 6·60, instead of 5·09.

it varies from 1 : 6.93 in the *lowest*, to 1 : 6.08 in the *highest*, the *average* being 1 : 6.59. In the gelding, the proportion varies from 1 : 6.44 in the *lowest*, (excluding one case in which the cerebrum was so enormously developed as to reduce the proportional weight of the cerebellum to 1 : 7.44, although the latter organ surpassed, in *absolute* weight, any other which is here recorded,) to 1 : 5.16 in the *highest*, the *average* being 1 : 5.97. We cannot but think that any unprejudiced person, on comparing these results, must admit that they are sufficient, at any rate, to countervail the vague statements of Gall, even though they may not be sufficiently numerous to establish an exact average of the relation. Phrenologists have too often shown a great horror of *precise* comparisons; but we submit that here is a case in which there can be no serious mistake, the cerebellum and cerebrum being so completely isolated by Nature that no accident or partiality on the part of the experimenter can affect the results in any considerable degree. We are surprised that a man of Mr. Noble's general candour and benevolence of disposition should have descended to the unworthy but easy method of invalidating the results of experiments which do not accord with his hypothesis, by imputing intentional deceit to M. Leuret, as being a known opponent of phrenology. It will be quite time, we submit, to do this, when Mr. Noble, or any other phrenologist, shall have brought before the world a more extensive series of observations made on the same plan, by which M. Leuret's results shall be overthrown. And even then it will be, of course, for the world to judge from their relative estimate of the two parties, whether the system-makers or the objectors are most likely to be in error. The results of M. Leuret's inquiries have now been before the public for some years; but as we have not heard that they have been thus invalidated, we cannot but admit them as affording important negative evidence with regard to the phrenological view of the functions of the cerebellum.

We have left ourselves no space to touch upon the bearings of pathological observation, with regard to the two views of the functions of the cerebellum. This, however, we the less regret; since the very small number of well-observed cases at present recorded, does not afford any decided evidence either way; it being quite possible, however, to put this evidence in a form that is very favorable to either side, by neglecting all that bears the other way. This Mr. Noble has done, in favour of the phrenological view; recording a small number of coincidences between injuries and diseases of the cerebellum, and disorder of the sexual organs (among others quoting a case, in which the injury was a *flesh wound* in the neck, and in which there was no proof whatever of the cerebellum being affected), but passing over with very imperfect notice the cases in which there was no such coincidence, or in which the phenomena tended the other way. He gives in detail, however, two cases recorded by Dr. Cowan, which seem to have had some influence with that gentleman,—a professed phrenologist, be it observed, but at the same time a man of too philosophical a mind to be a blind partisan of the system—in leading him to view with more favour the doctrines of Flourens. This forfeiture of allegiance to Gall, on the part of one of his followers, very amusingly excites Mr. Noble's surprise; his conviction of the truth of the system being such, that he cannot understand how any one who has once embraced it can relinquish one tittle of its dogmata. And he devotes some space to

the analysis of the details of Dr. Cowan's cases, with the view of bringing them into harmony with Gall's doctrine ; in this, however, he seems to us to display more ingenuity than philosophy.

On the whole, then, we are disposed to infer, from the remarkable concurrence between the results of experiment and the data supplied by comparative anatomy, that one function of the cerebellum is to regulate and combine the muscular movements required to perform a large number of our voluntary actions. And if the evidence, furnished by observation of the human species only, should prove to be such, as to establish a constant relation between the development of this organ in him, and the strength of his sexual propensity, (a position we by no means dispute, our arguments being only directed against the insufficiency of the evidence on which it rests at present) we must regard this as a *superadded* function, not as the fundamental purpose of the organ.

We conclude, as we commenced, by disclaiming any hostility whatever to the phrenological system in the abstract ; and by freely admitting the *general* coincidence between the indications of human character, which are afforded by cranioscopical examination, and those derived from a direct acquaintance. But we consider that, in building up their system, the followers of Gall have been too disregarding of evidence supplied from other sources than observation of man ; and that they have been misled, as to the fundamental question of the connexion of the cerebrum with the purely instinctive actions, by their inattention to comparative anatomy, which proves that the cerebrum *cannot* be the instrument of those actions ; and have glossed over the important objection which the non-development of the posterior lobes in the lower mammalia and in all the oviparous vertebrata interposes to the location of the animal propensities in them. So far, however, from availing ourselves of these errors, as conclusive arguments against the whole system, we have endeavoured, by a new analysis of the propensities and emotions, to show that the facts supplied by comparative anatomy may be brought into conformity with the physiology of Gall ; and that the phrenological system may be planted upon a much more secure and extended basis than it has yet possessed ; a new and more exact series of observations, however, being required to build it up with anything like firmness and consistency. We cannot regard the question of the functions of the cerebellum as at all fundamental in its character ; and can easily understand how a candid phrenologist like Dr. Cowan may, on this point, embrace the views held (we believe) by all the leading physiologists of the day.

Finally, we commend our review of the subject to the candid consideration of those who think with us that the determination of the *general* functions of the *encephalon* is the *first* question for the physiologist ; that the determination of the share of these performed by the *cerebrum*, to be effected by attention to comparative anatomy and by experiment, is the *second* ; and that the determination of the *special* functions of *different parts of the cerebrum*, to be effected (for the reasons we have stated) by the comparison of the varieties of cerebral (not cranial) conformation in man, with some assistance from that of the lower animals, is the *third*. Upon those who blindly uphold the method of Gall, in disregard of all the improvements which have taken place in our knowledge of

neurology since his time, and who regard the present phrenological system as so perfect as to be incapable of improvement, we certainly despair of making any impression. We would not be supposed to assert our conviction that our own views as now expressed, are so complete as to be incapable of further improvement. Considering as we do that the whole science of encephalic phrenology is in its infancy, that a large amount of information as to the fundamental data on which it must be built up is yet wanting,—that the progress of comparative anatomy, of embryology, and of microscopy, may make many additions to our knowledge of the connexions of different parts of the nervous centres which may tend to modify previously-received doctrines,—and that, on the other hand, an entirely new system of psychological observation must be carried out, in order to bring psychology and physiology into their proper relation ;—it would be absurd in us to attempt to lay down dogmatic conclusions, by which to stand or fall. We would be understood as attempting nothing in this article, but to test the relative validity of two rival *methods* of philosophizing on this subject. Our conviction of the uniformity of Nature is such, that we are thoroughly persuaded that there can be no real contradiction between her various indications, when these are properly brought together and compared ; and our attempt has been to point out the application of the method, which has been elsewhere pursued with complete success, to neurological investigation. We deem it particularly incumbent upon us to point out that even if our views should be proved to be erroneous as to a few minor points,—such, for example, as the offices of the thalami and corpora striata,—our main argument is not affected. The points for which we contend are simply these :—the independent character of the sensory ganglia as the instruments of sensation and of consensual actions,—the superadded character of the cerebrum, as the organ by whose instrumentality ideas are formed and reasoning processes are carried on,—and the mixed character of the emotions and propensities, as compounded of *ideas* and the *simple feelings* of pleasure and pain. On this last point, we venture to think that we have made a real advance in psychology, which will prove to be important ; and we happen to know that several intelligent psychologists are well prepared to receive it, as fixing and defining views which had been previously floating in their own minds. It seems, indeed, to have been glimpsed at by the late Mr. James Mill, in his valuable ‘*Analysis of the Human Mind* ;’ his deficiency consisting in connecting the feeling too much with the sensation, rather than with the intellectual idea. We should be doing injustice to that very pains-taking anatomist, Mr. Swan, were we not to state that, on referring to his general summary of his views of the offices of the nervous centres, we find a very near coincidence with the leading features of our own doctrines regarding the relative offices of the sensory ganglia and the cerebrum ;—doctrines, indeed, to which it would be easy to point out approaches in the writings of many previous physiologists, to whose authority we might refer in support of our own.

## ART. XIV.

*On the Injurious Effects of Mineral Poisons in the Practice of Medicine, &c. &c.* By HORATIO PRATER, M.D. PH.D. &c. 12mo, London, 1846.

IF for once in our lives our gravity might be pardoned for perpetrating a manifest and facile pun, we would say that the work before us is clearly the production of a *Prater*,—an arrant prater of other men's nonsense and his own. It professes to contain an epitome of and commentary on the dietetics of Cornaro, and on the new system of medicine of Raspail, "embracing all the details necessary," as Dr. Prater tells us, "for persons to give this system a fair trial," &c. &c. It is a shrewd compound of twaddle and stuff, intended to be very attractive to noodles and old women, whether in petticoats or breeches. There are some good jokes in the thing, however,—not jokes by the author, who is a dull fellow enough, but statements which his amazing stolidity and ignorance render ridiculous; statements which would figure well in a farce if delivered seriously and in character.

Our author's system of cure participates in the merits of all popular systems, without any of their imperfections. Morison and Mesmer, Priessnitz and Hahnemann, Dickson and Raspail, are all plundered of their finest plumes. Dr. Prater, for example, would give, according to his system, those substances only as medicines which are found as component parts of the body. He would use them separately, but in the very small proportions in which they exist in the human body; and this Dr. Prater observes, makes his system very similar to the homœopathic. He would also increase or encourage the first symptoms of diseases by medicines which induce similar symptoms, especially when the alimentary canal is to be thoroughly cleared out. "But after this is done, we would not continue (as we presume the regular homœopathist would) the use of medicines tending to keep up such actions, but rather the contrary, since, as we have already stated, such symptoms may often keep up, from the effect of a bad habit (so to speak) the vital principle has got into." (p. 14.)

Dr. Prater has an ingenious proposition respecting keeping up "connexions by blood." He thinks the old folk should have transfusion practised from the veins of their offspring. "If there be any thing," he says, "approaching the character of an *elixir vite* in nature, it seems more reasonable to look for it in the blood of the adult and healthy offspring than perhaps to any other source." He would also give "oxygen water and solution of deutoxyd of hydrogen." But further—"Eggs and oysters, as evidently *containing a vital principle*, (the italics are not ours,) may be used in such cases, for from their exciting nature they seem, perhaps, to impart a certain quantity of this to those who use them as food." Nay, he holds more recondite views than these, and thinks other means worthy of trial. "Whether, on similar principles, there can be a communication of nervous influence can only be learned by experiment. Does the optic or frontal nerve of an animal *just killed*, laid on the upper eyelid, benefit amaurosis? or the spinal marrow of the same laid or rubbed

on the spine of the debilitated old man give any additional strength?" (p. 19.) Aye, answer this, ye who can! We certainly cannot, though we doubt not our great philosopher could if he chose.

Raspail is a sort of monomaniac who has given himself up to the influence of the idea that nine-tenths of all diseases are dependent on parasitical animals or worms; an idea, we believe, not unusual amongst medical monomaniacs. The ova of these creatures can penetrate every part of the body; for example, those of the ascarides may enter the lungs with the air at each inspiration. Ascarides never lay their eggs in fæces, "except" (poor things!) "in cases of despair." Acari constitute another very insinuating class of parasites. They will get into flour and cheese, revel in the cracks of wooden pitchers, &c.

"They live in the joinings of *old* furniture, and in houses old or dirty, old wax, collections of dried plants, or animals, in ill-conditioned ulcers, and wherever the *fermentation caséique* can develop itself. Hence it would appear right that the joinings of *all old bedsteads* should be well examined not only on account of bugs, but also of these acari; and no doubt it is chiefly to prevent the entrance of these into the mouth or nostrils that Raspail uses his cigarette of camphor *during the night*, when, as we have observed, they generally begin to move about." (p. 41.)

This theory will inevitably recall to the mind of every one who read medical books so long ago as the year 1810, the history of the man in Haslam's most amusing *Illustrations of Madness*, who always slept with his head in a tin saucepan, "to prevent the intrusion of the sprites."

Enraptured by the vermicular theory, our author has given vent to his feelings in immortal verse, and has printed a portion of his poesy. We subjoin a sample for the delectation of our readers:

"We have heard of embalming after man's death,  
When for ever has left him the vital breath.  
Thus the dead man is kept, no doubt, from decay,  
And all sorts of vermin chased from him away:  
But here, in this book of Raspail, you will read  
That *during our life* of embalming we've need;  
That insects assail us within and without;  
That from these come all ills—both fevers and gout;  
That CAMPHOR and ALOES these only can save us,  
And keep that life sound which great Nature gave us.

We know some love smoking—smoke half the day;  
To all our great smokers here then we say—  
Adopt this new plan, smoke, too, *all the night*  
Your camphor cigar, which ne'er wanteth a light!  
Then by you, in *your life*, no worms will be fed—  
Quite enough to be eaten by them *when you're dead!*"—(p. 49.)

Dr. Prater thinks that a deficiency in the quantity of saline matter in the system may be a principle cause of a great many diseases; "in fact, of all those that have an animate origin." He would rather, for his own part, "take a little less aloes and camphor, and a little more saline medicine as *preventives* of worms, than what Raspail recommends." Raspail



is good; Prater is better. Nevertheless, Prater likes the notion of ladies smoking camphor cigarettes in bed; but if this be impracticable, "a piece of camphor inclosed in a linen bag, and placed under the pillow, or tied so as to hang near the mouth and nostrils, would probably be amply sufficient to keep acari and all night vermin from getting into the nose, mouth, or ears, and depositing their ova there." There is one property of camphor, mentioned by Dr. Prater, which might have induced him to caution certain of his readers to take care and make the bag fast, so that it should not slip away and get down into the bed: that property is, "its remarkable power, when sprinkled in powder over the nates, of taking away all venereal appetite, as admitted by many old authors, and by Raspail himself"!

We could fill pages with drolleries like these; things so solemnly writ, and yet so absurd, that the reader would half suspect the whole matter to be a piece of irony, were it not that the asinine stolidity with which it is set forth bears internal evidence of genuineness. And yet there is every now and then bubbling up from his dark depths a bit of brilliant philosophy so very naked as would pass for cunning in other men, but which must convince, as it is intended, every reader that our great Horatio, at least, is a truly modest, simple-minded, truth-loving philosopher of the first water.

For example, he finds in the squabbles of the Royal Society a good reason for glorifying himself. Referring to the clamour lately raised respecting the adjudication of the medals, he observes, "the calm consideration of these unjust proceedings, and a firm conviction of the gross injustice of many writers, and the ignorance of others, have induced me to enumerate my humble labours on the title-page of the present work." And, sure enough, on the title-page, we have appended to the name of our author the following modest enumeration of what he was—even before the publication of the present treatise, videlicet:

"Horatio Prater, M.D., PH.D., Discoverer of the Fusible Compound of Carbon and Silica; of the peculiar Agency of Lime on the Tonicity of Muscular Fibre; of the Permanent Fluidity of the Blood by a Heat of 140°; of the Acceleration of its Coagulation by Ammonia, Carbonate of Soda, &c.; of the Cause of the Coagulation of Albumen by Heat; of the Diffusive Power of the Gases; and, conjointly with Fizeau, of Möser's Images; and Author of 'Experimental Inquiries in Chemical Physiology,' &c. &c. &c."

Alas, alas,

"The world knows nothing of its greatest men."

Who, before reading these affecting details, could have imagined that such a genius was lying *perdu* amongst us? And yet who, after reading the great work now before us, can doubt that the ill-used author is much more than is here set forth.

"Horatio, thou art e'en as queer a man  
As e'er our observation coped withal."

## ART. XV.

*A Practical Treatise on the Diseases of Children.* By JAMES MILMAN COLEY, M.D., Member of the Royal College of Physicians in London, &c.—London, 1846. 8vo, pp. 467.

WE regret much that we cannot speak of this work at all in terms of commendation; and we confess we cannot see why it was written, or, at least, why it has been published. The preface, it will be seen, is one of considerable pretensions, both with reference to the author and his book; but so far are we from thinking the book deserves to be regarded in the light in which Dr. Coley wishes to represent it—"as a comprehensive work of reference"—that we cannot give it even the credit due to a well-executed compilation.

The surgical diseases of childhood present, with the few exceptions of original malformations, no, or scarcely any, peculiarities requiring special mention, though, in his preface, the author takes much credit to himself for having noticed surgical diseases. And we regret to say that what he professes to do he has done ill. For example, he describes the operation for strangulated hernia, while all the very important questions as to the time when a truss should be applied, how you can best avoid the annoyance to a young infant inseparable from a truss becoming soiled with its discharges, the best form of truss, &c.—questions of real importance, since the age of the patient here really does modify our practice—are left unnoticed. Nothing is said about non-descent of the testicle, &c. &c.

At page 69, external hydrocephalus is used as a synonym for cephal-hæmatoma! while this accident is noticed in the most cursory manner. The author speaks (p. 8) of *many* instances of success of the operation for imperfect anus, when such instances are notoriously very few, &c. &c. If from the chapters on diseases of the eye you take away all that is Mr. Lawrence's, and all that is Mr. Middlemore's, very little will remain for Dr. Coley. From the seventy pages on diseases of the skin we have learned nothing. In the preface the author glorifies himself for his notice of erysipelas. It is only during infancy that we find deviations from the ordinary course of erysipelas, such as—that it has a greater tendency to wander—that it often proves fatal in young children without either vesication or the formation of matter—that the former result is much rarer than the latter, &c. Dr. Coley notices none of these; and we find that the treatment recommended is what might be adopted for a man of 20 or 30. What guide would his directions be to a pupil who had to treat an infant of a month old? The important subjects of measles and scarlatina are slurred over in an extraordinary way; and his remarks on the dropsy after scarlatina are absurdly short.

But it is wearisome to go on noticing a book in which we can really find nothing to praise. Take, for instance, the chapter on gangrene of the mouth (p. 131). A page and a half are occupied with the translation of three post-mortem examinations by Rilliet and Barthez; the remaining

two pages and a half are inferior to what might be expected from a second year's student. Immediately after these follows a notice of "Cancrum oris, sloughing phagedæna of the mouth;" in which the author appears to have confounded simple stomatitis with the affection he has described in the preceding section.

The article on dentition consists of four pages of common-place physiology. Then comes a scrap of French, which, as in many other instances, Dr. Coley leaves untranslated. Many little bits of French are thus introduced, mere scraps, often of no importance whatever (see p. 253). He denies that any other diseases than local inflammation are connected with dentition, and yet he does not notice at all that general inflammation of the gums of teething children, which any patient observer must often have noticed, and which has been described as Odontitis. No help is given towards fixing any rule about lancing the gums, though this is surely a matter on which, after forty years' practice, a man must have arrived at some conclusion.

There is no opinion given as to the advantage of the early operation for harelip (p. 152), though the subject has of late received much attention in France.

In his account of the diseases of the digestive organs, Billard, Rilliet, and Barthez have been drawn on very largely, but there is a popular physiology of his own very often introduced, which we do not at all like. See, for instance, the account of the anatomy and physiology of the digestive organs at pp. 165, 175. Whom is this meant to teach? It is certainly more adapted to the capacity of the monthly nurse than the medical practitioner, or even the most juvenile student.

The chapter on croup is, on the whole, a favorable specimen of the book; but what is there in it? It teaches nothing new, nor gives any weight to what was known before. In the chapter on pneumonia (p. 281) we have a description of all sorts of absurd and even impossible symptoms; the expectoration described in children who cannot spit, and delirium talked of in infants who have not yet attained the use of reason. The morbid appearances are entirely from Rilliet and Barthez, as is the case also in the article on phthisis. The important and extensive subject of diseases of the brain is compressed into the narrowest limits, and is as unsatisfactory as possible.

The general style of the work is slovenly, sometimes ungrammatical. Misprints abound, while there is a great attempt at a display of learning both in the text and notes. If, however, the reader will look at the wondrous authorities (German) quoted in the notes of pages 151, 250, 253, 264, &c., he will assuredly feel much doubt as to whether the author has ever seen the books he quotes, or could read them if he had.

Before concluding, we must say one word on the title-page of this book. We should be glad to be informed on what authority the author appends the M.D. to his name. We happen to know that he is not a graduate in medicine in any university or other institution qualified to confer the degree and title of "MEDICINÆ DOCTOR;" and we have always been taught to believe that a person who has not so graduated, is no more

entitled to place after his name the two letters M.D., than he is to place the three letters KNT., or the four letters BART. The author of this volume is a LICENTIATE, or, as he writes it, a MEMBER of the London College of Physicians; and he is unquestionably legally qualified to practise as a physician; but the College neither has, nor pretends to have the power or authority, to grant to any one the degree of *Doctor in Medicine*, and therefore we regard the assumption of this title and dignity by a mere licentiate as altogether unwarrantable. As the licence of the College qualifies the holder, as we have said, to *practise* as a physician, it would not perhaps be overstraining the privilege, if such holder took *the name* as well as the office of physician. At least, we should ourselves not only readily accord this title, but we would even, out of courtesy, give the name of *Doctor* also, just as we give this name to Bachelors of Medicine. But just as no one ever saw a Bachelor of Medicine, however long he may have been *named* Dr. in common parlance, place the talismanic letters M.D. after his name, so, *a fortiori*, no one ought ever to see an ungraduated licentiate of the College of Physicians to do so. Dr. Coley—as we are willing to style him by courtesy, though we believe his proper title to be Mr. Coley, Physician,—can certainly plead, in extenuation of his fault, that he is kept in countenance by many other of his fellow-licentiates, both *intra et extra urbem*; but it is principally on this very account that we notice the subject here, and we say—*peccatur intra muros et extra*. We know Dr. Coley to be a gentleman of the highest respectability; and though he has written a bad book, we believe him to be an experienced and skilful physician. No Doctor of Medicine, at whatsoever university he may have graduated, could hesitate for a moment to meet him in consultation; none, we should hope, would refuse to address him by the ordinary title; but it is well that the members of the profession generally, and the Fellows of the College of Physicians especially, should be made aware that there are to be found among the ungraduated licentiates of the college, men of a very different stamp, who are a disgrace to the title of *Doctor of Medicine*, which they so improperly assume—men who are not only utterly undisciplined in all that literary culture which a *degree* and its concomitant *title* are usually understood to imply, but who are even destitute of that amount of special knowledge which ought to be possessed by every member of the medical profession, however humble. Can such things be—and overcome us like a summer cloud, without our special wonder? But if any of our wondering readers should be curious also, and desire to know *how* such things be, we refer them for particulars to that famous statute which established the ELECTS of the College of Physicians.

## PART SECOND.

## Bibliographical Notices.

ART. I.—*Notes and Recollections of a Professional Life.* By the late WILLIAM FERGUSSON, Esq., M. D., Inspector-General of Military Hospitals. Edited by his Son, JAMES FERGUSSON.—*London*, 1846. 8vo, pp. 248.

AFTER having served with distinction for twenty-three years, during the stirring and eventful period between 1794 and 1817, and having attained the rank of Inspector-General of Army Hospitals, Dr. Fergusson retired into civil life and established himself, first at Edinburgh, and, four years afterwards, at Windsor, where he enjoyed an extensive and lucrative practice, till attacked with paralysis two years previous to his death. He possessed ample opportunities during his military career of studying the diseases of soldiers, both on the Continent and in the West Indies, in the field and in garrison, in the time of peace and during war.

"I embarked," he remarks, "nine times from the shores of Britain with armaments on foreign expeditions, and, during twenty-four years' actual service, I spent seventeen years, or parts of them, in other climates, passing through every grade of medical rank in every variety of service, even to that of the sister service of the navy; and it thus was my fortune to have sailed in every ship of war, from the first rate of the line down to the smallest craft that carries a pennant."

Dr. Fergusson contributed many able papers to the medical journals, on subjects connected with the public health, and more particularly on those relating to military hygiene. The latter half of the volume before us consists of a reprint of some of the more important of these contributions, while the first part contains a series of papers, commenced shortly before his illness, and which was intended to form "a digested abstract of opinions and observations that have been subjected to the ordeal of long and deliberate consideration." Unfortunately he was not spared to carry out his views on this point. Of the papers he had completed, some refer to the state of the army as it was when our author served in it, and exposes abuses which happily no longer exist, while others are perhaps too purely military to demand lengthened notice from us. We would recommend the perusal of the work to every army medical officer who feels really interested in the soldier, and takes a pride in the service to which he belongs, as the production of an able and intelligent officer, and containing many excellent observations on subjects connected with the health of troops; it has also the advantage of being written in an amusing and very readable style. We shall content ourselves with one quotation from a paper "On Fever as an Army Disease," which we believe contains the true secret of its successful treatment, and may suggest useful reflections to those who

are instant in season and out of season with their endeavours to cut short and to cure it by heroic remedies.

"It is the *gastro-enterite*, cry the disciples of Broussais, and there can be no cure but the leech. It is cerebral inflammation, respond the followers of Clutterbuck, and the only remedy is venesection. Purgatives are the true treatment, proclaims the Edinburgh school. They are irritating and dangerous, replies that of London. In fact, all are equally right and all are equally wrong if they fail to note times and seasons, the nature of the epidemic, and the characteristic tendencies of the patient's constitution, his powers to bear the operation of medicine, and his ability to resist the tendency to death. There can be no treatment of fever by physic but in studying the *juvantia* and *lædientia* of the case—cultivating the first, eschewing the last, and never forgetting that there is a mighty power always operating in your favour—the *vis medicatrix nature*. Do not thwart her beyond the mark, and she will get you through difficulties with which, without her aid, you could not cope; but the physician who believes that he possesses beyond these, medicines of specific power in fever, really should have his own licence suspended, and himself put under cure until the monomania subsides. . . . . The battle is to be fought by the nurse, whether in the shape of physician or other attendant it matters not. Only let that attendant be sagacious and diligent, and the patient is saved—the contrary, and he dies."

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ART. II.—*The Microscopic Anatomy of the Human Body in Health and Disease, illustrated with numerous Drawings in Colour*. By ARTHUR HILL HASSALL, F. L. S., M. R. C. S., &c.—Parts I and II. 8vo, pp. 76. With six Plates.

THE design of this work is very commendable. The present rapid advance of microscopic inquiry, together with the increasing importance of its results, fairly demand that a special treatise should be devoted to it; and a volume so moderate in size and price as this promises to be, would deserve every encouragement if its performance were equally satisfactory. This, however, we regret to say, is not in our opinion the case, in regard to any one of the points on which the author claims a favorable consideration. After stating the intended number of parts to be twelve, he proceeds—"It is intended to embrace a systematic and copious *illustrated* description of the various fluids and solids of the human frame, no structure or organ of the body being omitted." And he afterwards adds, "The design of the work, if not altogether original, is almost unique; the only work extant which embraces the entire range of microscopic anatomy, and this *not exclusively human*, is that of Mandl." Why, Mr. Hassall's work is not a whit more exclusively human than that of Mandl. Of the *three* plates in the first number, only *one*—and of the *three* in the second number, only *half a one*—represents objects of human anatomy. We should not think the worse of the book on this account, if it were not that if the other subjects are treated upon the same scale with the blood and circulation (which occupy the two parts already published), the book must extend to fifty parts instead of twelve; and that the fact is plainly inconsistent with the author's preliminary announcement. Further, Mr. Hassall says, "One great feature of the book, it is hoped, will be the *fidelity of the drawings*; a principal fault in most of the published works on minute and microscopic anatomy being that the figures do not exhibit



the characters of the text." Here, again, we feel obliged to state that Mr. Hassall's figures are *very* far from bearing a resemblance to nature, either in their drawing or colouring. The drawing is in coarse lithography, which is not at all calculated to give an idea of the real aspects of the objects. And the colouring will strike every practised microscopist, we venture to affirm, as egregiously incorrect, and calculated to mislead. In all the figures of red corpuscles of the blood, for example, these objects are represented as of a full red tint, even when lying in a single layer; when every tyro knows that the hue is scarcely distinguishable in a single disc, and that the red hue becomes apparent only when two or three overlap. The differences between the red and colourless corpuscles of the human blood, in the very first figure, are thus grossly exaggerated; as is manifest from the fact, that no one could overlook them in the plate, whilst in nature they have only been lately detected by a careful scrutiny. We recommend to Mr. Hassall's attention the admirable figures of Donné, which are engravings of high finish and great beauty, faithfully copied from daguerreotype images of the objects themselves; as showing what blood-corpuscles really resemble under the microscope.

We greatly regret that we are obliged to speak thus disparagingly of Mr. Hassall's very laudable attempt. But our duty to the medical public requires, that as the subject is one on which, from the attention we have given to it from the very first, we feel well qualified to form an opinion, this opinion should be expressed as unreservedly as if it were more favorable. Should future numbers present such an improvement in their character, as to warrant our commendations instead of our condemnation, we shall most gladly bestow them.

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ART. III.—*Elements of Physics*. By C. F. PRESCHSEL, Principal of the Royal Military College at Dresden, &c. Translated from the German, with Notes, by E. WEST. Illustrated with 430 Wood-engravings.—London, 1845–6. Three Volumes, 12mo, pp. 894.

"THE study of the physical sciences," says the translator, "has become of late almost universal; partly because it has been found well suited to cultivate habits of observation and reasoning, which cannot fail to be of use in the business of life, and partly because the practical application of scientific principles to the details of almost every department of manufacturing and agricultural industry has rendered some acquaintance with natural philosophy and chemistry indispensable." We need not, we trust, now dwell upon the importance of these studies to the youth who is preparing himself for the medical profession; since our opinion of their value has been frequently and strongly expressed. Continued observation, indeed, has only served to convince us yet more decidedly that the *direct* benefits of these pursuits to the medical student, great as these are, are the least of their advantages; and that the philosophic training which they are calculated to afford is, if due pains be taken by the teacher to apply it, the most important result of attention to them. But this result cannot be attained by the acquirement of a mere smattering of acquaintance with physical science, such as may be very useful as regards its

practical applications ; it requires for its development a closer and more patient study, such as few youths in this country, save those who go through a regular university course, devote to the subject. We quite agree with the translator that, notwithstanding the multiplicity of elementary treatises on physics, there was yet room for another, which, without aiming at being a *popular* introduction, should be sufficiently simple for an intelligent beginner ; but which should employ the acquaintance with elementary mathematics, which most schoolboys now gain, as a means of introducing a more scientific method of treatment, such as is at present confined to works of the highest class. And though we cannot doubt, from the excellent manner in which he has translated and edited the work of Peschel, and from the character of the additions which he has made to it, that he was fully competent to produce such a treatise for himself, yet he has not done unwisely, perhaps, in availing himself of one which so admirably fills up the deficiency alluded to.

The first volume, published rather more than a year ago, and containing the physics of ponderable bodies, has been most favorably received by those best qualified to form a judgment of it ; and has, we believe, been introduced as a text-book into several of our leading academies. It would, therefore, be superfluous in us to offer any opinion of our own upon its merits ; but we may state that a careful examination of the other two volumes, which have recently appeared, has satisfied us that their merit is equal, if not superior, to that of the first ; and that the whole book may be most safely recommended to those who desire to attain something more than an elementary knowledge of physical science. It will not supersede the excellent Manual of Dr. Golding Bird, which is more concise in its character and limited in its range, and which is better adapted, therefore, for those who can spare but a small modicum of time for such pursuits, or who are, unfortunately, not sufficiently well grounded in mathematics to avail themselves of the higher advantages of the treatise before us. But we trust the day is not far distant, when every medical student, who aims at taking a respectable stand in his profession, shall have made himself fully master of Peschel's '*Elements of Physics*,' even if he proceed no further.

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ART. IV.—*The Vegetable Kingdom ; or the Structure, Classification, and Uses of Plants, illustrated upon the Natural System.* By JOHN LINDLEY, PH.D., F.R.S., F.L.S., Professor of Botany in the University of London, and in the Royal Institution of Great Britain. With upwards of Five Hundred Illustrations.—*London*, 1846. 8vo, pp. 908.

THIS work, which we may really call a splendid one, may be considered as a third edition of Dr. Lindley's *Introduction to the Natural System of Botany*, of which the first was published in 1830. At that period, the system of De Candolle was in general estimation amongst those botanists who had the wisdom to discern the absurdity of adhering to the Linnæan or artificial system, long after its author would have disowned it. The progress of science, however, showed many imperfections and inconsistencies in De Candolle's arrangement, particularly as regarded the want of

some intermediate subdivisions between the few primary groups of Phanerogamia and the hundreds of natural orders which were even then recognized. And in his second edition, published in 1836, Dr. Lindley attempted to carry out a plan, which had been proposed by some continental writers, of grouping these orders into alliances. The ten years which have elapsed since that period, have served to confirm the general truth of the views which he then propounded, whilst they have rendered necessary many modifications as to details. The new materials continually presenting themselves to the botanical architect, for which a place *must* be found in his edifice, require continual shiftings and alterations in his minor arrangements, although the principles of construction may remain the same; and consequently it is as absurd to criticise him for departures from his former plans, as it would be to find fault with a chemist or a physiologist for making discoveries which upset or modify some previous system. "In fact," as Dr. Lindley justly remarks, "there is no such thing as stability in these matters. Consistency is but another name for obstinacy. All things are undergoing incessant change. Every science is in a state of progression, and of all others the sciences of observation the most so." And with most commendable humility he proceeds: "The author's object, and he thinks he may say that of every one else who has turned his attention to the question of late, has not been to establish a system of his own, which shall be immutable, but to contribute to the extent of his ability towards that end. . . . All that we can do is to throw our pebbles upon the heap, which shall hereafter, when they have sufficiently accumulated, become the landmark of systematic botany."

The following outline of the contents of this volume will indicate the vast amount of matter contained in it, and will give some idea of the labour which must have been bestowed upon its composition:

"Its object is to give a concise view of the state of systematic botany at the present day, to show the real or supposed relation of one group of plants to another, to explain their geographical distribution, and to point out the various uses to which the species are applied in different countries. The names of all known genera, with their synonyms, are given under each natural order, the numbers of genera and species are in every case computed from what seems the best authority, and complete indices of the multitude of names embodied in the work are added, so as to enable a botanist to know immediately under what natural order a given genus is stationed, or what are the uses to which any species has been applied. Finally, the work is copiously illustrated by wood and glyphographic cuts; and, for the convenience of students, an artificial analysis of the system is placed at the end." (Preface, p. xii.)

It would be quite beyond our province to attempt any detailed criticism on such a masterly compendium of botanical science as the one now before us. And we shall content ourselves with recording our conviction, that no work of any thing like equal value exists either in our own language or in any other; that it is consequently indispensable to every student who desires to make himself acquainted with the present aspect of botanical science, besides serving as a most valuable work of reference to those who desire only occasional information as to the characters, properties, and uses of the various species of plants (to the acquirement of which the copious indices give every facility); and that Dr. Lindley well

deserves the gratitude of all botanists for the zeal and perseverance which could alone have carried him through a work of such immense labour.

We cannot put aside the book, however, without remarking that, in our opinion, Dr. Lindley's attempt to Anglicize scientific names may be very well spared, and is in fact injurious. In a *popular* treatise, it is very right to endeavour to ingraft science upon facts of familiar knowledge, and to show that the many-syllabled Euphorbiaceæ and Ranunculaceæ are nothing but Spurges and Crowfoots, or plants allied to them. But in a scientific work, intended for foreigners as well as natives, the common language of science ought, we think, to be rigorously maintained; and we cannot see the advantage, either in euphony or appearance, of Hydrocharads over Hydrocharidaceæ, Bromelworts over Bromeliaceæ, Pontederads over Pontederaceæ, or Kadsurads over Schizandraceæ.

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**ART. V.**—*The Anatomy of the Arteries of the Human Body, and its Applications to Pathology and Operative Surgery; with a series of Lithographic Drawings the Size of Nature.* By RICHARD QUAIN, F.R.S., The Drawings by JOSEPH MACLISE, Esq., Surgeon. Eighty-seven Plates, imperial folio. The Letter-press 8vo, pp. 550. London, 1844.

ALTHOUGH we have already given a full and elaborate analysis of this incomparable work (No. XXXVIII, April, 1845), we are induced to notice it once more, from the circumstance of its now coming before us, virtually, though not actually, as a New Edition, and because we feel it a duty to our readers, to make them acquainted with the splendid prize now within their reach. The publishers, in their new Prospectus, inform us "that as the large size of the stones on which the drawings are made, renders it difficult to retain them uninjured for use from time to time, whenever they may be wanted, they propose to throw off at once 500 copies, and then efface the drawings from the stones, that the plates may not be produced in a less perfect manner." When subscribers are received for these copies (500), the work will be delivered at the price of SIX GUINEAS, in place of the original price of £10 12s. This, we will venture to say, from our own knowledge, is one of the cheapest publications ever offered to the profession; and we earnestly advise *every surgeon who operates or expects to operate*, not to lose the present opportunity of possessing a work which may be really said to be almost indispensable. The *possibility* of obtaining it can only exist for a short time, as there cannot be a doubt but the small subscription list of five hundred will soon be filled. We would suggest to the publishers to offer the work even at a less price, say *five guineas*, provided the subscribers (as seems likely) greatly exceed the stipulated number of five hundred.

## PART THIRD.

CONTRIBUTIONS TOWARDS THE ADVANCEMENT  
OF THE  
Natural History and Treatment of Diseases.

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## I. ON EXCESSIVE TRUST IN NATURE IN THE CURE OF DISEASES

BY J. A. SYMONDS, M.D.,

Consulting Physician to the General Hospital, Bristol.

(In a Letter to Dr. Forbes.)

Bristol, August 20, 1846.

MY DEAR SIR,—I was much gratified by your remarks in the Journal for April, obviously intended to counteract some of the impressions which had been produced by your article in the January Number. I knew you to be a vigorous eclectic in practice, but I feared that the vehemence of your protest against the manner in which the *medicina perturbatrix* is sometimes exercised, and that the pains which you took to hint or assert the adequacy of Nature alone to the cure of many and very important diseases, might be prejudicial in two ways. In the first place, I apprehended that your frank and plenary confessions as to the inutility or injuriousness of some of the ordinary methods of cure might be caught up, not only by ignorant and pestilent empirics, but also by the fanatical apostles and disciples of the homœopathic and hydropathic heresies; and that the confidence of the laity in legitimate medicine might be thereby shaken more than it has been already. This *læsa fides* is no small addition to the difficulties and perplexities which, under the most favorable circumstances, must often beset the honest and scientific practitioner. On the other hand, I was fearful that your recommendation of a trial of the undisturbed play of the *vis medicatrix* might induce in some minds a general indolence of practice, and in others might encourage that liability to paralysis of judgment, which, by the bedside, has often produced absolutely fatal results, both to the patient and the practitioner. I know that you maintain that there are many cases in which, with our present knowledge, it is our bounden duty to exercise great vigour in our interference with disease. We must act up to our lights. Be they strong or be they feeble, we must still act up to them. The *laissez-faire* system is good, in so far as it gives free scope to healthful energies; but, if it presumes to tolerate the powers of darkness, the sooner it goes to Tophet, the better for poor man, who has hard work enough as it is in fencing off disease and the devil.

I repeat, then, that it gave me great satisfaction to find that you had so distinctly expressed your belief in the necessity of an active interference with disease, and in the efficacy of many of the received remedies, and that you wished to be understood as having only entered a strong caveat against the blind heroics and unreasonable polypharmacy still too prevalent, and as having suggested a more thorough and searching inquiry into the natural history of morbid phenomena, and into the principles upon which therapeutics should be founded.

Your essay elicited some very interesting correspondence—Dr. Laycock's paper must have been read with the same satisfaction as the many other contributions which have been bestowed by that enlightened physician on the

philosophy of medicine. The letter to which the honoured name of Dr. Andrew Combe is attached, and the anonymous epistles in your last Number, appear to me to set rather too much in the direction of the expectant method. You will therefore, perhaps, excuse me, if, at the risk of uttering things trite and tedious, I venture to make a few remarks, the drift of which will be opposed to the non-coercive system of treating diseases.

X The question before us is, whether it is safer to leave diseases to their own course, or to interpose what we call remedies? And really this is a very frightful question to be asking at the present date of the world's history, when medicine has been proposed as an art two thousand years and more! What! is it a probability admitting of debate, that, of the countless swarms of ghosts, that have passed "the melancholy flood," medicine has introduced to the "grim ferryman" those whom kind Nature would have held back? But we need not frighten the imagination with horrible surmises of forthcoming corollaries from a possible conclusion, which, if it were true, would be one of the mournfullest truths in the universe.

I confess that Dr. Fleischmann's table somewhat startled me—for a moment it seemed to be the most awful statement in numbers that I had ever met with. I say awful, because, if received as it stands, it would require us to revolutionize from the foundations the whole constitution of medical art. To think of 100 recoveries out of 105 cases of peritonitis!—with no treatment but what was dietetical, for I entirely agree with you in computing the globules as = 0. What sort of disease peritonitis may be in Austria, I have no knowledge from experience; but, if this statement be correct, I have no hesitation in expressing my belief, that it must be a different malady from that which is associated in my mind with the name. But, on further examination, I noticed anomalies which soon removed the discomfort which the first view of the table occasioned. There appear to have been only six cases of enteritis. This may perhaps be accounted for by a restriction of the name of inflammation of the muscular coat of the intestine. There is no mention of enteritis as seated in the mucous membrane, and I therefore infer that it is included under diarrhea. Yet, strange to say, this last-named category numbers only 114 cases—that is, only 9 more than peritonitis. Again, while 300 are assigned to pneumonia, bronchitis has only 15! At first it seemed probable that the milder cases of disease are not admitted into the hospital; but, were this the case, how are we to explain 300 affixed to tonsillitis? *Gout* appears to be common among the paupers of Vienna;—possibly the paternal government makes them luxurious. There are no less than 102 cases—more than half the number ranged under the head of rheumatism (183). These *peculiarities* of Dr. Fleischmann's table are, I think, sufficient to justify us in excluding it from the present inquiry.

Whether a true art of medicine does or does not exist, it must be allowed that there *ought* to be one. Man has succeeded in conquering or repressing so many of the adverse powers in Nature, and has subjugated so many others to his purposes, that we might by analogy expect him to acquire some dominion over the *vis vitatrix* and the *vis necatrix*. The existence of such forces in Nature is apt to be forgotten by physicians, who, from the influence of the old Hippocratic doctrines, got into the habit of representing Nature, *φύσις*, as all-beneficent—as if she had no poisons, generated no diseases, and allowed nobody to die if she could help it. Now, it would be contrary to all other experience under the sun, if it should be discovered that men of genius and patient observation have for two thousand years been watching a certain set of phenomena with a view to altering them and making them serve their purposes, and have tried a number of experiments to that effect, and yet have accomplished nothing, or next to nothing! Let it not be said that, though men have been so successful with the mechanical and the fine arts, like results were not to be expected when they had to deal with *vital* phenomena. Has



nothing been done in agriculture, horticulture, and the breeding of animals, &c. &c. ? Art, after all, is but Nature in a new form—a fresh arrangement of the forces of Nature, compelling them to work under new conditions.

Much of the practice in the ordinary system of medicine is a strict imitation of salutary processes in Nature, and its object is to make such processes supersede the *baneful practice of Nature*, who, though she knits up wounds with her adhesive inflammation, by the very same method glues the intestines into fatal entanglements, shackles the heart, and chokes up the windpipe. In *this* man she soothes “the grief of a wound” by pouring out serum; in *that* she makes the same effusion effectually close the rima glottidis. She spirts blood from the hemorrhoidal vessels of Paul, who blesses her for saving him from apoplexy; of which very disease poor Peter dies, because she has poured the same fluid into his lateral ventricles. And so on, for man’s body is a microcosm in which one sees the play of Zoroaster’s antagonist principles. Nature is ever the same. Blessings are mixed with curses—the poisonous berry and the nutritious root are found in the same plant. There are balmy dews and pestilent fogs—fertilizing streams and destroying deluges—and the lair of the murderous lightning is in the cloud that floats across the blessed sunshine. But man is sent into the world to wage eternal war with the evil one—and woe betide him, if he lets the demons of disease get an advantage over the angels of reparation, for want of his friendly and judicious interference.

I think, then, that there *ought*, in conformity with man’s other achievements, to be an art of medicine. And if it can be proved in any case that human skill has successfully interposed where Nature would assuredly have failed, we have good reason for caution against a general trust in the capabilities of Nature, and a general distrust of those of art. Let us call to mind two or three instances of Art versus Nature. Surgery suggests, among a host of other triumphs, the operation for strangulated hernia, ligatures on arteries in aneurisms and hemorrhages. A man’s life is ebbing: he cannot tell why. He looks bloodless—he knows of no hemorrhage—there is no apparent disease. The physician insists on his using a commode for one day instead of a dark water-closet. Florid arterial blood is observed in the evacuations, and, on examination of the rectum, a painless bleeding tubercle is found just within the verge of the anus. The aid of a surgeon is requested for the application of caustic or a ligature, and the patient takes out a new lease of existence. In obstetrics what wonders are wrought by active interference with Nature, e g. in placenta prævia, arm presentations, retained placenta,—nay, craniotomy.

Look at medical treatment in the eye. To say nothing of extracting cataract and making artificial pupils, who has not seen cases in which Nature had been doing nothing, or worse than nothing for months, and yet, on a happy application of some positive disturbing agent, no doubtful invisible medication, but such a thing as lunar caustic or blue-stone, the disease is gone. Or take the skin. A lady suffers from an obstinate lepra—she has long given up medicines—she has been content to mitigate the disease by diet, and baths, and regimen—she has had the disease for years. She is moderately bled—an issue is inserted in the arm, and she takes that triple poison, the hydriodate of arsenic and mercury: in six weeks she is cured. I have chosen for examples, diseases the evidence of which is objective. Neuralgia and other subjective phenomena are open to more doubt.

But to come to the cases in which it is especially needful to have our minds made up as to the right course of practice—I mean the acute visceral inflammations—when I become satisfied that these diseases may be left to pursue their own way, my belief will have suffered a more remarkable *bouleversement* than it has ever yet undergone. We know well enough what these diseases are capable of doing in the way of disorganization, whether meddled with or not. We have met them in the latter predicament in the neglected cases pre-

sented to us in dispensary and hospital patients; also in cases of latent inflammation, revealed only by the scalpel—cases in which *Nature* had been stealthily prosecuting her deadly work. But it might, perhaps, be said that there is no sufficient evidence to prove that such cases would have had a better issue had they been placed under orthodox treatment. Some years ago I was called to a patient who had been ill four days. He had been seized suddenly—the symptoms were those of peritonitis. For reasons which it is needless to state, not a single active measure had been employed. He had been confined to bed, taken diluents, and very trifling medicines. When I saw him the belly was tympanitic, with complete constipation and sickness. The pain had lessened, but there was tenderness; the voice had become husky; there was full evidence that the usual effusions had occurred in the peritoneum. I thought the case all but hopeless, but I practised some moderate depletion. Some alleviation ensued, but nothing more. He was not laid prostrate by the remedies, and, as a proof of it, he lived to the seventh day from the attack. He was a strong healthy man of 50. The necropsy showed the extensive adhesions, &c., of sthenic plastic peritonitis. Do you doubt that if, at the onset, the patient had been bled, leeches, and taken opium freely (with or without calomel), he would have had a far better chance of recovery? I have said, “we must act up to our lights.” Now, one has repeatedly seen cases begin in a way closely similar, and after, or because of certain remedies, yielding, as to the most essential points, in two or three days—nay, in a day;—the inflammation stopping before fibrin has been effused. I don’t believe there can be a greater sceptic in these matters than I am; yet even my Pyrrhonism is overcome when I see the marvellous change induced at the *onset* of an inflammation by a bleeding, a poultice, and an opiate. Even if it be maintained that the *symptoms* only are abated, e. g. the pain and tenderness, and the vomiting and constipation, and the rapid pulse, and hot skin, and scanty secretion, but that still the essential disease, the pathological lesion, the *causa causans*, the *vitium vitians*, goes on a certain course, and stops only when Nature thinks proper—still I say, thank God for the lancet and opium! My patient is lapped in balmy slumber, and I am quite at ease as to the issue of the malady.

Take another instance. I was called up in the night to a lady, who I found had been ill several days. She had been treated with very mild remedies for what appeared to be hepatic congestion. She was very feverish, and breathed in a way that gave me the impression, when I first set eyes on her, that she was suffering from pneumonia; but I listened to the chest, and failed to find any sign of inflammation within the thorax. The pulse had an irritable character, and there was slight delirium; and, in the absence of proof of local disease, it was decided to try an expectant method, on the probability of the case turning out to be one of fever. The next evening things were rather worse. Again I explored the chest, and this time listened where I *ought* to have listened before, but where I had been deterred by a blister, which had been placed over the region of the liver. There I found crepitation and bronchial respiration. The case was clear. It had been occult pneumonia, which had possibly begun in a part more removed from the surface. Then came a terrible antiphlogistic visitation; all the great guns were fired: the patient recovered perfectly. Relief and abatement of disease ensued at once; but a check was not at once given to the hepatization, which involved two thirds of the lung. The most decided improvement in the signs, both physical and rational, was coincident with mercurial stomatitis. I do not deny that I have known hepatization consequent on neglected pneumonia get well without active treatment; but it has been in a prodigiously longer time than when antiphlogistic remedies had been employed.

Bear with me, if I narrate one more case. A young lady had been ill for many months. For several weeks she had been under a homœopathic practitioner,

who was, of course, treating her symptomatically. She got worse, and ordinary medicine was again appealed to. We found an enormous collection of fluid in the left pleura, pushing the heart far over to the right side. We practised very moderate depletion, chiefly with a view to favour absorption; blistered largely; and exhibited diuretics. The case is now under treatment; and, as I have repeatedly seen in like cases, the fluid has been traced receding in proportion as the excretory organs have acted freely, and the discharge has continued from the blistered surface. The heart has resumed its proper habitat, and the symptoms (especially the dyspnoea) are greatly mitigated; but the case will probably end in phthisis.

If one were at a loss for instances in which art interfered with advantage, when Nature was doing nothing, or rather doing mischief, I doubt if any instance could be more striking than the cure and relief of many of the diseases of children by lancing the gums. On the expectant method a child may, after many days of suffering, and after incurring perils in various organs, at last recover when the tooth has pushed its way into the light; but all the pain and jeopardy might have been spared by one or two free incisions.

The efficacy of medical treatment is, I think, evidenced by the short time requisite for the cure of diseases, which, cured by Nature, occupy a far longer period. Within the last six months I have watched two cases of subacute pleuritis, both well marked by the auscultatory signs. In both there were reasons, from the peculiar constitutions of the patients, for avoiding general blood-letting and the stronger medicaments. They both recovered; but I have no doubt, from my experience in similar cases, that a more active treatment, not violent, would have reduced the disease in at least half the time. Whether it would have been ultimately a *gain* might be questioned; but such instances show the power of art over morbid processes.

Time does not allow me to touch on the *reasonableness* of the antiphlogistic treatment—as, for examples, the influence of venesection on the action of the heart, arteries, and capillaries, to say nothing of the blood; the operation of opium on the neurotic element of inflammation—of mercury and antimony on the excretory actions; and the close correspondence between the effects of such agents and the processes which appear to be instrumental in resolving diseases which are cured spontaneously.

But what shall we say of fevers and the exanthemata? Only what nineteenth-century eclectics would affirm, viz. that the less heroically they are treated the better. And why? Because neither theoretical nor empirical therapeutics have suggested remedies that appear to tell on the essential part of the disease, that is, the poison in the blood. At least, I know of none, unless it be chlorine in scarlatina and typhus, when accompanied with putrid symptoms. Yet we cannot but hope that, when the pathology of the blood has reached a development approaching to that of the tissues, we shall have remedies equally direct and appropriate.

I am not fond of arguments from final causes; but can it be doubted that the various medicines we possess were, as such, a part of the plan of the universe, designed to have relation to morbid states of living organisms, as much as esculent matters to healthy conditions? What! were diseases planned? It would seem so; and that it was no less needful that men should depart from the stage of life than they should enter upon it. Disease and death are as *natural* as health and life. And lest it should be objected to this view, that if diseases were intended to sweep men from the earth, antagonists would not be provided for them in the form of medicines, I may remark that this is only one of a multitude of illustrations of the fact that it has pleased Divine Providence to order the world upon a system of checks and counter-checks. Few things show this more strongly than the predatory instincts of one set of animals, and the provisions in a weaker set for eluding and guarding against the attacks of the former.

In your pamphlet, page 31, some excellent reasons are adduced for believing that "Nature can cure diseases without the assistance of art;" but they appear to me to admit of some limitation. Thus it is quite true that diseases are cured "among uncivilized nations, of ancient and modern times, under the sole influence of magic, charms, or other practices equally ineffective;" but, on the other hand, in proof of the inefficiency not only of these methods, but also of Nature herself, one might appeal to the eagerness with which savage tribes invite the medical aid of travellers, and to the extravagant confidence they repose in it, as compared with their anticipations of benefit from their own remedies.

With regard to the expectant system of medicine, though it became very prevalent, we must not forget that its failures generated that disposition to violent heroics which characterised the practice of the first quarter of the nineteenth century. Should the present overweening trust in Nature continue, we may apprehend a similar reaction.

The apparent success of quack medicines, inferred from the large consumption of them, does, I agree with you in thinking, in some measure prove that a vast number of diseases get well spontaneously. Yet we may err in presuming that the popularity of such remedies depends mainly on their seeming utility. Much of the estimation in which they are held may, I suspect, be assigned to the circumstance that a large number of them are, more or less, derived from the class of purgatives; the action of which is always in high favour with the laity. Many a physician has heard the reproaches of patients for the inertness of his prescriptions, simply because they had not produced catharsis—a process associated in their minds with any thing like activity of treatment.

As for hydropathy, the subjects of its milder processes may, perhaps, fairly be said to owe their recovery to the *vis medicatrix*; but in its full administration the system is a glaring instance of *medicina perturbatrix*. It is drugging, and very dangerously, with aqua pura.

That practitioners as they advance in life, become more and more inclined to confide in the resources of Nature, would imply that experience has not taught them to attach as much value to artificial methods of cure as at earlier periods of their career. But we must not lose sight of the fact that age brings not only wisdom, but also an indisposition to enterprise in our own as in other professions. There is also another consideration to be borne in mind. Physicians, whose practice is chiefly consulting, meet with a large proportion of chronic cases; and, as Dr. Alison has judiciously remarked in his essay on Inflammation, in the 'Library of Medicine,' with reference to the value of blood-letting, they are often called in, either when the time has gone by for the remedy to be either safe or advantageous, or when it has already been practised as far as the patient's strength would allow. They are, therefore, less likely to be impressed with its efficacy than those who are present at the onset of the disease, and see it quelled at once by the prompt administration of this measure.

I have not adverted to hygienic methods, which are said to be capable of curing diseases without the aid of other remedies. Allowing this to be true, we must, nevertheless, perceive that they are interpositions of art. If a patient is confined to bed, when but for orders he would be sitting up or walking about; if he is restricted to bread and water, or milk, when otherwise he would be living as usual; if a particular temperature of his room is maintained, &c. &c., and he gets well—his case, surely, ought not to be adduced as an example of the curative powers of Nature. There has been a decided interference of Art.

I should deeply regret were these cursory remarks to give you the impression that I underrate the reparative powers of Nature, or that I advocate any thing approaching to *meddlesome* practice. My intention was to hint, first,

that *a priori* we might expect there should be an art of medicine—an art analogous to all other arts; for they all consist in the subjugation of the materials and forces in Nature to the wishes of man; and, secondly, that it is an error to speak of salutary changes, as those of Nature *par excellence*, for she is destructive as well as conservative; exciting processes which tend at one time to disorganization and death, at another to reparation and life; and that it is needful to watch her actions with rigorous attention, nay, with suspicious scrutiny, and to do our best towards encouraging the *vis medicatrix*. He who has learned neither to wait too long, nor to interfere too soon, has made an attainment scarcely inferior to that of knowing *what* remedy should be employed.

I have carefully read your cogitanda; but though there is scarcely a suggestion with which I do not willingly accord, I am of opinion that it is not so much a revolution or reformation of medicine that is wanted, as a more thorough and extensive education of practitioners in the art and science of medicine as it at present stands. The most intelligent and accomplished must sigh for more knowledge in a thousand directions, but the interests of medicine very loudly demand that its professors should *know* what is already *known*. Were pathology pursued and applied, and the operations of remedies observed and made use of, after the methods and rules laid down and illustrated by Dr. Williams, in his 'Principles of Medicine,' and by Dr. Watson, in his 'Lectures on Practice' (I instance these works as *typical* in their respective departments), both the profession and the laity would have practical proofs of improvement in our art, sufficient, I think, to satisfy them that knowledge has been sought in the right tracks. "Our venerable mother," *me judice*, is neither sick nor sorry; nor does she, by reason of age or decrepitude, require a Medea's cauldron in order to come out as "Young Physic." In fact, she is not old enough; she is undergoing development, advancing *towards* maturity, though slowly, and a long way off the goal.

The desiderata are many and great. Not the least is a knowledge of the *natural history* of diseases left to themselves. But who shall furnish this? No one can conscientiously seek it in cases which experience has taught him are capable of arrest or abbreviation by art. Neither you nor I could make up our minds to stand composedly by the bedside of a patient suffering an acute inflammation of the pleura or peritoneum, and in the characters of scientific umpires to say to the sanative and morbid forces, "Now, you antagonist *vires*, we are here to see fair play, a clear stage, and no favour. Our part is not to interfere in your deadly struggle, but to note your relative prowess—*laissez-aller!* Philosophic eyes are on you; and God defend the right!" But as we are not prepared for this position, you ingeniously suggest that we should turn to account the records of homœopathic medicines, as containing reports of maladies that have pursued their course unaltered by remedies. But I fear it is impossible that a physician, under the influence of Hahnemannian doctrines, should observe as *we* should observe; for even minds the most exempt from the bias of preconceived views cannot help, in their observations, seeing more or less the reflex of themselves. An observation is a compound result of the thing observed and the observing mind, the latter being frequently the predominating element. A homœopathic mind could hardly give the same copy of a pathological phenomenon as one that had never succeeded in reconciling itself to the ineffable absurdity involved in the doctrine of infinitesimal doses. The conclusive *reductio ad absurdum*, effected in your article, and in the essay of Dr. Alexander Wood, compels one to suppose that those who have received the doctrine in question have done so in accordance with that strange paradox of Tertullian, "Credo quia impossibile est;" while to us it is a great trial of catholic feeling to forbear exclaiming, when such insults are offered to our reason, "Incredulus odi!" Were other arguments against this fantastical system, with its fractional truth of "*similia similibus curantur*," and its delectable *psoric* figment, wanted, I think a sufficient one might be found



in the improbability that a therapeutical system has a sure foundation, which might be practised quite as well by a person whose knowledge of disease is purely symptomatological, as by one who is profoundly versed in modern pathology. The knowledge which is made up of the coarse or spider-web speculations of other men's minds (and a great deal of medical learning, we may concede, belongs to such a category) may come to nought; but that which is derived from a careful and enlightened observation of natural processes, in health and disease, must sooner or later be susceptible of practical application. But homœopathy has no place for the results of the scalpel, the microscope, and the test-tube.

It has been urged by one very estimable writer, that homœopathy ought to have a fair experimental trial, and that it is unjust to reject the system on *a priori* evidence. But to this I would answer, that life is not long enough for making experiments, unless they are suggested by some antecedent probability. Because a dreamer or dupe tells us that he has a method for extracting sunbeams from cucumbers, are we, in order to show our freedom from prejudice, to waste precious time in putting his nonsensical process to the proof? And because we do not choose to steal the time due to more promising pursuits, are we to be everlastingly reminded of the infamy attached to those who opposed the discoveries of Harvey and Jenner? If, on every occasion that these immortal names have been invoked, a real discovery had been announced to the world, of only one hundredth part of the value which belongs to the doctrine of the circulation and to vaccination, few mortals would become weary of life, and "age, ache, penury, and imprisonment," would become unknown evils. But stars of such magnitude are not born every night. Long ages of darkness must overshadow the world between such portentous births. And yet, if an enthusiast takes it into his head to walk about with a farthing candle, and fancy it "light from Heaven," we are not allowed to smile at his delusion till we have climbed up an observatory designed for watching the very stars, and from so exalted a position taken note of so ridiculous a phenomenon!

Dr. Andrew Combe tells us that we must carefully distinguish the principle of "*similia similibus*" from that of the "infinitesimal doses;" and he suggests that we should test the former separately. But to do this, we must begin an entirely new line of observations. And what is to induce us to administer a scruple of jalap for a looseness, and a grain or two of opium for a lethargy? I suppose Dr. Combe would say, the testimony of homœopathic practitioners. But the observations of these gentlemen on the "like-by-like" doctrine are inextricably mixed up with the alleged effects of infinitesimal doses, and are therefore quite useless to the proposed investigation. Doctor Minimus may aver that he has cured gastritis by decillionths of arsenic, but is Doctor Medius warranted by this testimony in trying to do the same with sixteenths or twentieths? Certainly not. The records of homœopathic medicine give no help to us in solving the question of the sufficiency of the "*similia similibus*" theory separately from the minute doses. Then what is to show us the duty of departing from orthodox practice? Is it the statement that Hahnemann persuaded himself that he had seen ague brought on by taking bark? *Quousque tandem?*

O, prophet-sage of Cos! well might you mourn over the brevity of life and the slow progress of art, for you doubtless foresaw that, after more than two thousand years, physicians would fritter away the one and impede the other, in proving or refuting the notions of so whimsical a brain as Samuel Hahnemann's!

But I am myself encroaching on this short life with this long letter; and I fear that after all I have been emphasizing truisms.

Yours, very faithfully,

J. A. SYMONDS.



## II. ON THE PRESENT STATE OF HOMŒOPATHY IN GERMANY.

BY DR. A. MÜHRY, OF HANOVER.

*(Extract of a Letter to Dr. Forbes.)*

HOMŒOPATHY has undoubtedly taught us, that however high an estimate medical men may have before formed of the healing powers of Nature, these must be rated much higher still. This fact may indeed render us more modest, but need not depress us into spiritless unbelievers. If, on the one hand, we ought to feel ashamed that the curative powers of Nature, aided—by nothing at all (*ein reelles nichts*)—should sometimes achieve as much as we are able to do ourselves, the history of homœopathy has, on the other hand, shown this system to be inadequate to the wants of human society; and has brought into stronger light, not merely the nullities (*nichtigkeiten*) of medicine, but also as a secondary result, its positive utility.

I think I am right in affirming that the history of homœopathy can be better traced in Germany, where the system originated (fifty years ago), and where its development took place, than in England, where it has not been practised more than from ten to fifteen years. It appears to me, likewise, that the changes wrought in homœopathy have not been sufficiently taken into account in your essay; and hence it may here be of some use to point out some of the principal epochs of the system.

Samuel Hahnemann published his 'Organon' of Medicine in the year 1810, but had begun to make his system known before that time. I think with you that he was a sincere believer in the truth of his doctrine. It is but just, however, to remark, that, at least once previously, he had deceived the world, by selling at a high price, under the name of *pnœum*, a nostrum which consisted of nothing but *borax*. This is a fact undenied even by his adherents. He had before this published a pharmacological dictionary ('Apotheker Lexicon'), and his system was altogether the offspring rather of Pharmacy than of Medicine, properly so called.

Among others, the following retractions and retrogressions have been made in the original doctrines of homœopathy, by various adherents. At first it was deemed requisite to administer medicines only once a week or once a fortnight. At present scarcely a single homœopathist adheres to this; but, on the contrary, now frequently orders them several times daily. A contradiction was also recognized in the assumption of a force-creating power (*potenzirung*) of remedies by attenuation—a greater efficacy in less attenuated remedies. At the present day there is often administered a drop of unattenuated and unpotentiated tincture (termed elementary, or mother tincture [*Urtinctur*]). Originally, the abstraction of blood, emetics, aperients, mineral waters and baths, were all proscribed: now they are often acknowledged as useful. Hahnemann's later doctrines, as promulgated in his 'Chronic Diseases,' met with few adherents: he had become inconsistent, and became more so still when he recommended camphor in very large, unattenuated doses against cholera. His successors became more and more divided, modifying the system in various ways, until its original meaning was almost entirely lost. This became apparent at a congress of homœopathic physicians, which took place at Magdeburg, either in 1836 or 1838. Their confession of faith attests how much reform and reaction the doctrine of numerous homœopathists had suffered. A fraction of the school has even laid aside the name of homœopathy, substituting for it that of "Specific Medicine." This sect is numerous

in South Germany, in the Grand Duchy of Baden. These still hold fast to the principle "*similia similibus*," as explanatory of the curative process, but they no longer give the remedies after the original method. They busy themselves in experimenting with medicines upon healthy persons, but in large doses, and from this they look forward to grand results, which are, however, not perceptible as yet. They likewise attend to the physiological relations of the diseased body, and they have amongst their body some most able and respectable men. For at least ten years past this "*Specific Medicine*" has supported a journal.

We may regard the homœopathists as sincere; but it is true that their number has augmented less by adoption of the original doctrine than of its modifications. They bear the original name wrongly, being no longer true homœopathists. The majority have even resumed a great portion of the allopathic medicines.

I do not hesitate to say that, although men of worth are to be found amongst them, there prevails, generally speaking, both within and without the profession, a low opinion of the standard of their intelligence. In Germany no man of undoubted eminence has ever become a convert to the system. Only once has an instance like that of Professor Henderson occurred: Dr. Kopp, of Hanau, unexpectedly published a volume of '*Practical Observations*,' descriptive of a series of homœopathic cases and cures of his own witnessing. He thus stamped himself a homœopathist. A few years afterwards another volume of the '*Observations*' appeared, communicating a further series of cases and cures,—but of an allopathic nature. The author thus declared himself reconverted, and he has remained so ever since.

With us in Germany there are in almost every largish town one or more homœopathists, who are perhaps consulted in chronic cases, after various methods had been previously tried, and where it is thought that homœopathy, at any rate, can do no *positive* harm. True it is, that cures are sometimes thus effected, and these create a sensation; whilst the cures accomplished by ordinary medicine pass unnoticed. A large proportion of homœopathists have recently adopted the hydropathic method of treatment. I am not cognizant of any town where homœopathy is exclusively practised.\*

If homœopathy has taught us that our curative control over the course of disease is far below what we rated it at, it has *à fortiori* though unintentionally, taught us that our "*apparatus medicaminum*" possesses less merit than we imagined, and that it need no longer continue as gross and rude as the instrument of an "*amentarium chirurgicum*" of old. It has, however, at the same time enabled us to mark with greater distinctness those diseases in which our remedies really effect the cure. Such are, for example, intermittent fever, scabies, syphilis. These it was that first showed the insufficiency of homœopathy; and you will find that in the statistic bulletin of Dr. Fleischmann these diseases are omitted.

One thing certain is that, through means of homœopathy, Medicine has undergone, is undergoing, and—as in your treatise you predict—will, in the future, yet further undergo a rigorous scrutiny. It appears to me, however, that rational scientific medicine has, in some degree already passed this ordeal; and if it can never claim the exactness of chemistry and mechanics, it may yet hope, in point of accuracy, to reach the standard attained by modern Physiology.

Yours, most sincerely,

A. MÜHRY, M. D.

June, 22d, 1846.

\* I may here mention that, besides the Homœopathic Hospital at Vienna, there is also one at Leipsic, which can, however, with difficulty maintain itself.

III. REPORT ON THE HOMŒOPATHIC TREATMENT OF ACUTE DISEASES IN DR. FLEISCHMANN'S HOSPITAL, VIENNA, DURING THE MONTHS OF MAY, JUNE, AND JULY, 1846.

BY GEORGE W. BALFOUR, M.D. EDIN.

(*In a Letter to Dr. Forbes.*)

Vienna, August 14th, 1846.

MY DEAR SIR,—I shall now proceed to lay before you the results of my inquiry into the practice of homœopathy, prefacing them with a short account of its present state in Germany, where it is now become quite fashionable, and nowhere more so than in Austria. Even travelling physician are now chiefly chosen from among its followers, who are, consequently, far from being insignificant in numbers. No young physician settling in Austria, excluding government officers, can hope to make his bread, unless at least prepared to treat homœopathically, if requested; and many, after attempting to do so, return to Vienna to make themselves acquainted with this new method. Many older men also attempt, by thus conforming to the foible of the day, to recruit a failing practice. Thus homœopathy is studied, not for any beauty or truth to be found in its doctrines, but from necessity, for a livelihood. Many continue to practise both methods, not eclectically, but according to the wish of the patient, believing in neither, leaving inquiry to others, and stumbling blindly on. Others, confident in homœopathy, merely use ordinary medicine, or, as it is termed, allopathy, in so far as occasionally to give a laxative, or, where the relatives urge it, to bleed—all by way of *placebo*; and I believe there are few, except the older and better established practitioners, who would not give such a *placebo* if requested. Nay, the inclosed recipe will show that even they are not altogether free from blame in this respect, or else that they have found their guiding principle in many cases false. The gentleman who gave me it (an allopathic physician) told me he might have procured many such.

While thus, from force of circumstances, everywhere increasing there domains, homœopaths are far from sitting idly down, content in following the footsteps of their first great master. Imbued with the progressive spirit of the age, they also strive after improvement, and while professing to retain "*similia similibus*" as their fundamental principle, are endeavouring to advance their method, and give it a more permanent and dogmatic character. Seeing, as it would seem, that the above-mentioned principle does not suffice for every case, they change the name of homœopathy to *Specific Medicine*. These men are dissatisfied with Hahnemann's work on the *Materia Medica*, on account of the imperfect nature of the observations, and look upon it as an enormous and almost unreadable catalogue of symptoms, more fitted for the memory than the intellect, and thus not only rendering the practice of homœopathy more difficult for his own followers, but throwing an almost insurmountable obstacle in the way of physicians who think otherwise, and often preventing those very intellects best fitted to become leaders in the reformed practice from ever studying it. (*Österreichische Zeitschrift für Homöopathie*, 1 Band, 1 hft. s. 4-5). Accordingly they have commenced a careful re-proving of all the medicines, with the view of obtaining, not a mere catalogue of symptoms, but a collection of medicinal diseases. A journal (the above-quoted) is also published in Vienna, with the view of giving publicity to these provings. This shows them to be in earnest in their endeavours to simplify their method and render it more practicable, as any one will be more inclined to confess, after having looked into Jahr's Codex, the best of the day. Who can wonder

at the difficulty experienced by homœopathists in choosing their remedies, when we find the same symptoms repeated, it may be, in a somewhat different order, under the head of almost each separate medicine? Nay, Isensee (*Geschichte der Medecin*, vol. vi, p. 1569) goes so far as to say, that in no case are the peculiar and characteristic operations of a medicine to be found, except in such cases as Hahnemann has, from want of original observation, borrowed from the allopaths; and that his own symptoms may all be referred to sobriety, fasting, ill-humour, and sleepiness, caused by continued attention to—*nothing*, mixed with those innumerable sensations which crowd every hour of our life!

Among those who are guided by the same principle in their choice of a remedy, the dose in which this ought to be given has formed, and is forming, a fruitful source of internal contention. Many, amongst whom may be reckoned Fleischmann and most of the Vienna homœopathists, employ the modern formula of 10:90 in making their dilutions, and seldom go higher than the 3d or 4th perhaps, but more rarely the 6th. Others, contending that those dilutions are too low and apt to produce disagreeable consequences, employ from the 14th to the 30th; while yet others, outstripping Hahnemann himself in their idea of the effect of friction and shaking in developing the latent powers of remedies, contend that these are at first produced too powerfully and irrestrainably, and, when employed in those low dilutions, only render the disease worse, without in the least tending to cure; but that by being further diluted (*potenziren* is the technical term) the agent at length arrives at that pitch at which, according to their ideas, the whole remedial power is developed, but so mild and tractable, that it at once cures without producing any exacerbation (*verschlimmerung*), and is therefore—and then alone—entitled to our confidence and the name of remedy. The champion of this sect is Dr. Grosse, a practising physician, at Jütterbock, whose usual dilutions are 2, 4, 8, and 900; and he often contents himself with allowing the patient to smell the remedy—whether one or more globules at one time I am not aware—waiting patiently for four weeks or so, for the completion of the cure, not even permitting a second smell: so mild, yet certain is the remedial action! This method, however, does not pass current with the homœopaths generally; it is rather too finely drawn for even their sensitive imaginations. A paper by Dr. Grosse, urging the importance of this mode, and accompanied with cases to prove its advantages, was published in the 21st volume of the '*Homœopathic Archives*,' and has been criticised in a late number of the '*Austrian Homœopathic Journal*,' by Dr. Böhm, a practising homœopathic physician in Vienna, and his cases in proof set aside on the grounds—1st, that in those cases in which a cure seems best made out, lower powers produce equally good results—hence an immense saving of time to the physician; 2d, that many cases recorded as cures seem to be merely a longer than usual intermission between paroxysms, which may, nay, often does occur, without any previous treatment; 3d, that many others were merely the natural result of the lapse of time—e. g. a severe pain in the foot which took its departure four weeks after smelling phosphor. 200; 4th, in others the diagnosis seems to have been incorrect, and the credit due to Nature given to the remedy. Dr. Fleischmann, also, in a note says, with respect to this mode, "Are the results favorable? I grieve that truth appears so decked in folly's garb, as to drive many from her in disgust. Are they imaginary? I grieve again, that spectres arise before which the soberest must retire in fright."\*

This Dr. Grosse seems but the pioneer for newer and bolder discoveries in this vast and unknown field; for another writer speaks of arsenic 2000.

\* Might not all this be with equal truth applied to homœopathy generally? It is to be wished that common homœopathists would as carefully criticise their own cases, and cease to imagine that "*quia post, non propter*" refers only to allopathic and high-power cases.—G. W. B.

What would Dr. William Wood say to this, if 30 proved too much for his weak faith and still feebler imagination? At this rate the dilutions of one remedy alone would, with the requisite utensils, form no inconsiderable laboratory, and these preparations consume no trifling portion of valuable time, not to speak of the innumerable trials necessary to be made, in order to find out in which dilution, from 1 to 2000, the remedial powers of the agent are most fully and usefully developed; verily, *ars longa, vita brevis*.

There is still another sect, or at least an individual, in Vienna, Dr. Georg Schmid, who has within the last few months given to the world a volume, as the result of many years' experience in homœopathic practice, with respect to the preparation of medicines and the amount of the dose (*Ueber die Arzneibereitung und Gabengrösse*, Wien, 1846). The latter portion of the work possesses peculiar interest, inasmuch as the author contends, from personal experience, that the present small doses are worse than useless, and that the mother tincture, 1 drop for a dose, or larger doses than usual of the 1st or 2d triturations (*verreibungen*) ought to be employed. These triturations he employs not because he considers that remedial power or new chemical properties—as solubility—are thereby imparted, but because the medicine being thus divided into its finest atoms, and each particle thus brought into immediate contact with the organism, is better enabled to exert its remedial agency. He adds (p. 60), that by using this means of division, we have not an absolute increase of power, but rather, with the diminished mass, a diminution of it. So long as the smallest trace of the remedy is perceptible, so long can we understand that such a dilution may be efficacious; but when the medicine is neither to be recognised by chemical nor physical means, and probably never will be, how is its efficacy to be explained? His remark upon homœopathic exacerbation deserves to be carefully read by every homœopath. He states that Hahnemann, in his preface to his work on Chronic Diseases, says that nothing worse could happen than that the small doses did not *help*, for *hurt* they could not. "Is it not," asks Schmid, "the duty of a physician to be of positive use to his patient, that is, to help? To do this he must use the proper remedies in the proper doses. But he can also injure; therefore medicine is no child's play, but the life-task of the physician; and he who feels not the power and the courage to discharge this duty, ought to choose some other profession. As there is no means of distinguishing the medicinal disease from the real, so the physician may ascribe an exacerbation which arose after taking his physic, to this cause, though it may be totally unconnected with it; and by then diminishing the dose, may allow this exacerbation to proceed unchecked. How often has not this already happened! Indeed it would not be difficult to discover amongst the many published cases of so-called medicinal exacerbation, merely the progressive increase of the disease, over which the medicine had not the slightest influence, being either of the wrong substance or deficient in quantity; yet the physician has reduced the dose, giving over his patient to nature, it may be to death." (pp. 227, &c.)

Dr. Schmid even turns Hahnemann against himself, showing that of the only four of his cases which have been published, two having been treated with the mother tincture, and two with various dilutions, the first two alone are entitled to the name of cures. Again, Hahnemann, after quoting allopathic cases in favour of his principle, adds, "large doses, though dangerous, often cure without peculiar disadvantages." (*Organon* 4 Aufl. s. 54-104.) Finally, Rau—though his work be no longer new—may be quoted as constituting one step towards the formation of an eclectic school; and doubtless, his remarks have afforded much comfort to those homœopaths who have been led astray by interest. He acknowledges that there is much good in the old school, nay, that "an anti-pathical practice may indeed be thought of." (*Organon der Specificischen*



Heilkunst, s. 45, 46.) He goes on to say that venesection, derivative and repulsive remedies, may at all times be useful; and quotes cases from his own practice, illustrating the fact that symptoms not to be relieved by homœopathic remedies will often yield at once to purges or emetics.

The Homœopathic Hospital of Vienna is a private one, in the convent of the Sisters of Charity: it was erected by them, in the first instance, with the view of thereby obtaining practical instruction in what constitutes their chief object—the cure of the sick. They have also another smaller hospital in a sister convent in the suburb Leopoldstadt. The homœopathic hospital is situated southwest of the city, in the suburb Gumpendorf, near the lines, much farther from the town than the General Hospital, and upon a loftier and airier situation; the latter being situated on the banks of the brook Alser, which has, within the last year or two, been covered over. The homœopathic hospital is also less than the other, containing only 50 beds—25 for males, and 25 for females, divided amongst four well-lighted and well-aired rooms. A few cribs for children are also occasionally occupied.

From its opening in July 1832, till July 1833, Dr. Mayerhoffer was its medical attendant. During the early part of this period the cholera was ravaging Vienna; and during the prevalence of this disease Dr. Mayerhoffer established his confidence in homœopathy. After him came the above-mentioned Dr. Schmid, who treated the patients, indeed, according to the homœopathic principle, but not with the wonted small doses. He continued till January 1835; at which time Dr. Fleischmann commenced those duties which he still continues to discharge, and whose name, in connexion with homœopathy and this hospital, is now known over both the old and the new worlds; and from whom the practice of homœopathy has received a greater impulse than from any other since the days of Hahnemann. During the first appearance of cholera here, the practice of homœopathy was first introduced; and cholera, when it came again, renewed the favorable impulse previously given,—as it was through Dr. Fleischmann's successful treatment of this disease that the restrictive laws were removed, and homœopaths obtained leave to practise and dispense medicines in Austria. Since that time their number has increased more than threefold in Vienna and its provinces.

The medicines employed in this hospital are all prepared in it by lady apothecaries (apothekerinnen), who, for this purpose, receive a special education, and undergo an examination. None of the drugs are obtained from allopathic apothecaries; or, if so, are carefully tested and purified. The tinctures are either made in the hospital, or obtained from those homœopathic physicians who reside where the plants are indigenous. The text-book for the preparation of the tinctures and dilutions is Gruner's '*Homœopathic Pharmacopœia*.' Gruner employs the proportions of 10 to 90 in making the latter, or 5 to 95 in the case of such salts, oils, &c., as are not soluble in the above proportion. This dilution is not marked 1, but fractionally, and a double portion 20 : 80, taken to form the second dilution, so as to bring it again into the proper relative proportion. Hahnemann's empirical rules as to rubbings, scrapings, and shakings are discarded. The time employed for the first two is regulated by the relative hardness and adhesiveness of the material; and in making the dilutions the shakings are continued only till the whole is clearly dissolved; when the dilutions are commenced by trituration with sugar of milk, 3 such are made, and one part of the third added to 9 of distilled water to make the fourth. The fifth dilution is made by the addition of one part of the fourth to nine of watered spirit; i. e. spirit containing 70 per cent. alcohol, mixed with an equal quantity of water; and the further dilutions are continued with this spirit. The temperature recommended is 12° to 15° R. The medicines are administered to the patients either in powder or solution, according to



the frequency with which they are to be given. For the first method, a drop of the solution, or a grain of the trituration directed, is mixed with a small quantity of sugar of milk for each dose; for the latter, a small bottle-ful of water is taken, and the trituration dissolved, or the solution dropped into it in the proportion of 2 grains or 2 drops to each ounce, half an ounce being the dose. Those for whom no medicine is considered requisite, get a powder of sugar of milk, or something of that kind. When two medicines are ordered, they are given alternately at intervals of two or three hours. The diet is light and simple; no coffee, tea, or wine is allowed; the latter, however, is sometimes given in cases of old people, or in convalescence from severe disease, if thought requisite. In acute diseases a light soup is given three times a day, exchanged, on commencing recovery, for a more nourishing one (*eingekochte suppe*), and the diet so progressively improved. No food is allowed to be given during treatment in which acid is predominant.

In taking into consideration the adjuvants to treatment, the religious character of the establishment must not be forgotten. The greater part of the patients are Roman Catholics. These find themselves surrounded by all the consolations of religion—by every thing which, in their opinion, tends to ensure, in the event of death, a speedy passage of the soul to the realms of bliss. Their minds, thus set at ease with respect to futurity, are less gloomy and desponding, and consequently react less unfavourably upon the body than in the opposite circumstances. Nay, looking upon their nurses as self-devoted in the service of heaven and of suffering humanity, they cannot but believe that the blessing of the Almighty will rest upon their labours; and being the object of these labours, they naturally enough appropriate a portion of this blessing to themselves, and imagine that their recovery can hardly fail to be promoted by their being the inmates of such an institution. In support of this opinion, I may state that although most of the patients were young, and many dangerously ill, I never heard one expression indicative of a fear of death, nor one murmur, however slight, unless extorted by the extremest pain, and even then it was more an aspiration after relief than a grieving at suffering. The severer the disease, the more closely do they grasp their rosaries and crucifixes. So long as they are able to read, prayer-books are constantly in their hands, and even in the intervals of delirium, nay, often, in the cases of women especially, during delirium itself, the exercise of repeating prayers, or snatches of them, is their occupation. The superiority of attendance is also one great advantage in favour of this hospital, independently of the important fact just stated, that the nurses are spiritual as well as temporal comforters. It may not be inappropriate to state here also—more particularly as I shall have occasion to compare the results of the homœopathic treatment with those observed in an ordinary hospital—that in Dr. Fleischmann's institution the pneumonic patients are *not* ausculted and percussed, and unceremoniously lectured over several times a day, as is the case in the General-Hospital. This difference will not appear a matter of indifference to any one who has witnessed the proceedings in the two instances.

The comparative youth of the patients in this hospital must also be taken into consideration. This is at a glance evident to the visitor, and will be rendered so to you, when I state that of 320 patients I have seen treated there during three months, 31 were under 15 years, 90 under 20, 80 under 25, 44 under 30, 22 under 35, 13 under 40, and above 40 were 22, of whom 13 were under 50, 5 under 60, and the remaining 4 were aged as follows—1 of 70, 1 of 73, 1 of 76, and 1 of 95 (the latter died of old age); consequently, above one third of the whole number were under 20 years, and considerably more than one half under 25 years. This circumstance of comparative youth, under all kinds of treatment, has an immense influence upon the ultimate result.

Again, the patients are admitted and discharged by the physician, without any control, so that, to say the least, it requires a man to be very conscientious to decide impartially between temporary improvement and perfect cure, especially when he recollects that the fate of his creed and his institution may depend upon the nature of his returns to government. These returns are made monthly, with a yearly *resumé*.

Some of the following cases will be found to have been discharged too early to enable us to be positive as to the ultimate result. Again, these cases, or others discharged apparently cured, may apply for readmission, and be under some pretext or other, refused; while, to disarm suspicion, a few whose relapses seem more manageable may be readmitted. Such may not be the case in point of fact, still it is very possible. I have seen at least one patient refused admittance, and that, too, the very day after his discharge, without any good obvious reason. It was a boy, with effusion into the right pleura following scarlatina, which he had gone through at home. There was also a general anasarca state of the body, which speedily disappeared, but the chief complaint remained obstinate, and after 33 days' treatment with bryonia, the 2d dilution, four times daily, he was dismissed but slightly improved. This boy was denied admission when he applied the following day, on account of return of pain in the chest, not certainly for want of room, as his bed was empty for days afterwards. This is not the only effusion into the chest which has been dismissed unimproved during the period of my observations; yet this scarcely agrees with Dr. Fleischmann's returns, as out of 12 with exudation in the pleura, occurring during 10 years, he has, he says, cured all but 3, who died. Add a physician of the General Hospital has assured me that many such cases dismissed by Dr. Fleischmann, and subsequently refused admission, have applied to him for relief, and which relief they have obtained by the use of purgatives and baths. Then again, there are, I may say, hundreds of trifling cases admitted here, which would not have been admitted into any hospital in England. Many of the patients get no medicine; a few a single dose; and even of comparatively trifling cases many remain for weeks, nay months, in the hospital; while more acute or more interesting cases are hurried out too often with the cure incomplete.

Dr. Fleischmann's usual number of drugs is not very extensive, one drug serving for a great many diseases, but chiefly because the diseases principally consist of a few standard ones constantly repeated. Gastricismus, typhus, and pneumonia are the chief; and in treating these he employs almost always the same remedies, only varying when some one unusual symptom is very predominant. This uniformity is a cause of complaint from his fellow-practitioners, who say that "by seeing his practice you merely get a glimpse of what homœopathy can do; as Dr. Fleischmann, satisfied that his returns are superior to those of any allopathic hospital, gives himself no trouble in trying to suit the remedy to the disease, but is content if occasionally the disease suits the remedy—when it produces those miraculous effects which are the boast of homœopathy."

This may be so: yet it strikes me as being something extraordinary that those cases in which such miraculous effects have been produced, have always been cases of some standing, either ill-treated or not previously treated at all, and the homœopath having come in with his dilutions at the lucky moment when Nature was going to relieve herself. This always puts me in mind of the Irish proverb, "The hour that is darkest is the hour before day." Things could not be worse; they might, and luckily do improve; and the homœopath gets the credit of it. The case is then published as one of the triumphs of homœopathy, whilst the many similar cases where even homœopathic treatment has proved unavailing, are silently passed over, or recorded as instances of

the imperfection of the human intellect: the wrong remedy must have been chosen!

Dr. Fleischmann also uses cold applications to the head in delirium, sometimes in headache, and cold washing of the body in fevers; in arthritis, cloths dipped in cold water, and surrounded by oil silk, are wrapped over the affected joints, and allowed to remain so long as they are damp, and are then reapplied. He also uses for costiveness clysters of plain warm water, or mixed with a little salt. In diarrhea, rice clysters are employed. He told me that neither he nor any rational homœopath ever employed emetics or purgatives, however simple; yet I heard his assistant once order a woman to get a spoonful of oil.

The number of students allowed to attend the hospital is limited by the spiritual powers, on account of the nuns. They attend generally in the morning; but during a part of my visit several attended in the afternoon, the number in the morning being full.

The whole process of the admission and discharge of patients is mysterious. Still so much is certain, that most of those admitted have been previously visited at their own houses by the assistant. Many cases not improving, or apparently not likely to improve, are got rid of very summarily. During most of the time, I visited in the morning along with Dr. Fleischmann, and latterly, for some weeks, in the afternoon along with his assistant, it not being then permitted to visit in the morning. I was told the cause of this restriction was that the students might have an opportunity of taking a course from the assistant.

I feel convinced that the secret of Dr. Fleischmann's great seeming success lies in the fact of the admissions and dismissions being entirely uncontrolled, and there being no check on the diagnosis. Rarely other than well-marked cases have their diagnosis written on the board at their bed-head, the others being left blank, and entered in his book, of course, as he pleases.

I have visited the hospital from the 21st of March till the 5th of August daily, except for a short time at first, when I was not in very good health; but I only give you the details of the three months, May, June, and July; as at first I was not sure how I could best conform to your wishes. Three months' regular observations are better than desultory observations extending over a longer period. I have not been able to do all that might have been desired, still I think enough has been done to enable you to judge fairly of the state of things. The patients are in general so stupid that it is not easy even for a native to obtain the requisite particulars from them; of course, it is much more difficult for a foreigner. And as to previous history, they are totally unaccustomed to recollect or relate any thing of the kind. If they have been previously ill, they can seldom tell you how or by whom they have been treated. The restraint belonging to the nature of the institution prevented even Dr. Fleischmann from making that examination which, in general or allopathic practice, at least, would have been thought requisite. Of course, I could do no more; I have, however, done my best to discharge my task faithfully and impartially; and in laying the following statements before you, I may say with Stork—"*Non hypotheses condu, non opiniones vendo, quod vidi scripsi.*"

#### TYPHUS FEVER.

During the months of May, June, and July, there were treated 32 typhus patients. Four of these still remain under treatment; and, excluding these, six died, giving a mortality of 21·4 per cent. The average age was 21·5.

Under Skoda, in the General Hospital, during the same period, there were 76 typhus patients treated, of whom 15 still remain; 19 of those died, giving a mortality of 31 per cent. The average age was 26·4. Of these patients,

however, there were, as I know from actual observation, fewer trivial cases. Skoda's treatment consists in giving acids (mineral), in doses of ʒj or 3ss. daily; extract graminis, or any other equally innocuous extract, by way of *placebo* for the patient; and where the diarrhea is violent, he employs tincture of opium to moderate it, generally in much the same dose as the acids. The following notes, carefully taken at the time, will give you some idea of the homœopathic cases and their treatment by Dr. Fleischmann:

CASE I.—J. A., a stout young woman, admitted Tuesday, April 27th. Next day at the visit, stated that for fourteen days past she had been suffering under daily attacks of cold followed by heat; resting, however, well by the night. (*Ipecac. 3d dilution, four times daily.*) On Friday last she was seized with vomiting, which lasted some time; bowels have not been opened for the last eight days; the pulse is accelerated; the tongue coated and dry; skin hot and dry; pain on pressing the abdomen. May 1st. The tongue cracked and bleeding. May 2d. One stool; was restless and wandering during the night; sleeping quietly at the time of the visit; cheeks flushed and eyes sparkling; tongue covered with dry brown scales. 4th. The pain in abdomen is to-day more violent and aggravated by slightest pressure. 5th. Bowels opened three times; skin cooler. 6th. Bowels opened eight times; thirst great; sleeps but little; wandering a little at night; the tongue is clearing and moist; the pulse less feverish. 7th. Bowels opened three times; slept better last night; coughs a little to-day, and expectorates a quantity of dull ill-coloured mucus streaked with blood; percussion-sound is good, but sonorous râles are audible over the chest. 8th. Bowels opened six times; abdomen slightly tympanitic. 9th. Her appearance is to-day more lively and sensible; the pain in abdomen better; the diarrhea ceased; tongue moist. 12th. The tongue is again dry and covered with brown scales; bowels not opened for three days; the pulse, however, remains quiet, the skin cool; expectoration continues. 15th. The pain in the abdomen is entirely gone; bowels open to-day. 17th. On account of difficult expectoration she got senega, the second dilution, to be given alternately with the former medicine, which she got on the 28th of April, viz ipecacuanha, the third dilution, four times daily. 19th. She is to-day bathed in perspiration; the pulse quiet; expectoration easy, and tongue again moist and clearing. 24th. Her cough is now much less troublesome. 26th. Her appetite has returned; the expectoration is much less copious, and more easily brought up. 28th. The ipecacuanha stopped, and the cough and expectoration having entirely ceased on the 18th of June, the senega was also stopped. On the 19th of June she was seized with a rheumatic attack in her ankles, which confined her to bed for a few days, after which she continued daily to improve; but was not discharged till the 26th of July.

CASE II.—M. M., a delicate-looking woman, aged 35, admitted on Thursday, April 30th. Complained at visit next day of rigors followed by heat, which together last from six to ten every evening, and have occurred daily for the last three days; they are followed by wakefulness, which lasts till the morning, when she has a little broken slumber; there is constant headache and bitter taste in the mouth; tongue coated; bowels open every third day; no appetite; slight pain in abdomen on pressure. (*Bryonia, 3d dilution, four times daily.*) The rigors left her; the fever increased somewhat, the tongue drying; otherwise her state was unchanged till May 9th, when she was covered with sudamina, having perspired a good deal during the night; the tongue moist and clearing; but on the 13th it was again dry, covered with brown scales; diarrhea set in, the bowels having been twice opened, and continued more or less for three or four days. On the 20th a slight cough came on, with mucous expectoration; this was gone by the 25th; the medicine stopped on the 26th, on which day she was up, still feeling very feeble, but otherwise free from complaint. On the 2d of June she was discharged.

CASE III.—T. B., a stout-looking woman, aged 26, admitted Sunday, May 3d. Next day, at visit, found her lying with flushed cheeks and wandering eyes, and a cold compress on her head; she would not speak above a whisper, saying that she was unable to do more; stated that for eight days past she had been suffering from alternate attacks of cold and heat; had no headache nor pain anywhere; was thirsty, and could not sleep; tongue coated and dry; pulse soft and accelerated. (*Belladonna*, 4th dilution, three times daily.) On the 7th she would neither speak nor take medicine, and continued lying in this stupid state till the 9th, when she seemed more sensible; her state otherwise unchanged. 12th. Has had a slight bleeding at the nose; complains of pain in the throat, but nothing can be detected on examination; she also swallows quite easily, sleeps well, speaks rationally; the pulse is quieter; the skin cool and moist; tongue clearing and moist; the feeling in the throat continued for a day or two longer, and then disappeared; she continued improving till the 31st, when she was allowed to rise, the medicine being discontinued; and on the 4th of June she was discharged, pain in the abdomen and diarrhea having been altogether absent.

CASE IV.—S. T., a stout woman, aged 26, admitted Wednesday, May 6th. Stated that she had been unwell for eight days. Her tongue is dry and cracked; pulse accelerated; skin cool and moist; no pain in the abdomen. (*Byronia*, 3d dilution, four times daily) 9th. Covered with perspiration; tongue with dry brown scales. 15th. Slight pain in the abdomen on pressure; it is also tympanitic; seemed, however, better; tongue moist; skin cool, and pulse quieter; bowels opened three times; diarrhea continued for four days. On the 21st her cheeks were flushed, pulse fuller and faster; delirium came on during the night. 22d. (*Byronia* omitted; *opium*, 3d dilution, three times daily.) Tongue dry; skin hot, with an increase of the fever during the night. In this state she continued till the 27th, on which night she died; no section.

CASE V.—M. E., a female, aged 31, admitted on Thursday, May 7th; next day at visit, stated that she had been ill for four days with headache and feeling of weakness; her tongue is dry and coated; pulse feverish; pain in abdomen on pressure. (*Belladonna*, 3d dilution, four times daily.) To this was added, in a few days, cough and expectoration of a dark-coloured mucus; and on May 11th, on account of difficulty of expectoration, the belladonna was alternated with senega, the 4th dilution; the percussion-sound was good; the auscultation gave mucous and sonorous râles. On the 17th of May the belladonna was omitted and arsenic given. On the 20th she expectorated a membranaceous mass about the size of a finger; the pulse is quick and weak; countenance rather sunk; face pale and lips blue; skin still hot; delirious at nights. 22d. The senega omitted, and opium, 3d dilution, four times daily, substituted. 23d. Pulse upwards of 130, feeble; thirst great; hands and arms tremble; breathing hurried. 24th. Bowels opened four times; pulse somewhat improved. 28th. Carbo substituted for arsenic; diarrhea continues; alvine discharges black; almost constantly sensible; pulse firmer; appearance somewhat improved. June 2d. The diarrhea has nearly ceased; the countenance again sunk; pulse quick and feeble; expectoration difficult; senega to be given instead of opium. 4th. Bowels still loose; looking better; slept a little. 8th. Sleeps well; cough gone; pulse quiet and stronger. 9th. Skin cool; feels altogether better. 10th. Complains of pain to day for first time; a slough is forming over right hip, which did not, however, separate till the 20th; it was as large as a man's fist. During this time she had considerably improved, but after this she once more relapsed, gradually sinking, and died on the 28th. No section.

CASE VI.—K. K., a girl, aged 18, admitted June 15th, stated that she had been ill for eight days, the illness commencing by attacks of cold and heat, accompanied by a continuous headache and pain in the abdomen; has been during



this period treated homœopathically; the skin is hot and dry; the tongue dry, brown, and cracked; pulse fast, but rather feeble. (*Arsenic, 4th dilution, three times daily.*) 16th. Has been restless and feverish during night; wandering occasionally in mind; tongue and teeth covered with black sordes; her face is pale; she continued in this low delirious state, with increase of fever at night, discharging her urine and feces involuntarily, diarrhea having set in on the 20th till the 25th, when her pulse improved, becoming quieter, and her tongue moister. On the 26th she was livelier, and answered questions more readily. 28th. She again relapsed; tongue dry; skin hot; pulse accelerated. June 1st. Pulse weak, scarcely perceptible; respiration hurried; face pale and death-like; four stools. 2d. Skin cool; respiration more regular; tongue moister; pulse quieter; her mind wanders still occasionally; the tongue now began to clear; she lost her wandering, and gradually became more lively in appearance; pulse became quiet, and gradually improved in strength—in short, from this time she steadily improved till the 26th, when she was discharged.

CASE VIII.—M. S., a female, aged 21, admitted July 20th. She was partially delirious, and unable to give any account of herself, except that she had been unwell for three weeks. The tongue is red, cracked, and bleeding; respiration difficult; pulse fast and weak; pain in abdomen on pressure; percussion-sound good; respiration vesicular, mixed with râle; sputa yellow, streaked with blood, and slightly adhering to the vessel. (*Phosphor. 3d dilution, every second hour.*) She gradually sunk, and died on the 26th. No section.

The general run of the cases here resemble the first two or three. Those like the latter seem only admitted occasionally—I suppose by way of attempting a bold stroke. Although I have at present got no more cases written out, yet I have all the severe ones noted, and the period of treatment, which I could furnish you with, if necessary. The average period of treatment was twenty-three days.

#### FEBRIS GASTRICA.

There were also 23 cases of febris gastrica treated. The average period of treatment was 10·8 days; the average age 27·8; they were all trifling except the following.

CASE.—S. D., a stout boy, aged 15, has been unwell for the last five days; complains of sickness and headache, with a feeling of oppression over the stomach; the tongue is coated but moist; the pulse full, and rather fast; he was admitted on the 17th of May, and got the 3d dilution of nux (vomica?) four times daily. 19th. He has been delirious during night, and this still being the case, on the 20th he got hyoscyamus instead of nux, in the same dose and manner. May 24th. He was asleep; great meteorismus; pulse full and frequent; tongue still moist; skin hot. On the 28th of May the delirium was gone; the pulse quiet, and the skin more natural. He continued after this to improve; the medicine was not, however, stopped till the 11th of June, and he remained in hospital till the 26th of July.

#### INTERMITTENT FEVER.

There were, during the three months, 41 cases of intermittent fever treated; the average age of the patients being 22·38. All but a very few received medicine at once on entering, without waiting to see whether they had a fever or not; a few had it once slightly, and it never returned. In 7 or 8 it never came on at all, and 30 had it repeatedly, the average number of attacks being 4·7. One had as many as 13; in one case the fever from a tertian became quotidian, and grew so much worse that the patient was discharged at his own request; the remedies employed were china, 2d and 3d dilutions, three or



four times daily; ipecacuanha, the 1st dilution, and nux, the 3d, alternately three or four times in the course of the day; and ipecacuanha and nux alone in the 2d dilution more rarely, arsenic, the 4th, or aconite, the 3d dilution. Chinin was only twice employed each time in the second trituration.

Skoda's cases of ague at the General Hospital are treated in the following manner. Each patient, on complaining, gets some extract, centaurii or taraxaci, or some such thing, and one attack is observed—if that come about the time specified, good—the further treatment is proceeded with, if not, a second attack is waited for, so that the time about which the fever may be expected may be known; then supposing it to come at nine o'clock, the patient gets two grains of sulphate of quinine at six, at seven, and at eight—at each hour two grains. The fever, if it be a regular one, may come slightly once after this, but never oftener; so that in regular cases, three times is the oftenest, generally only twice, and in rare and irregular cases four times—never oftener; but it also very frequently remains away altogether under the use of the bitter extracts, even where it has been of six weeks' duration, as I have myself seen; the powders are continued for three or four days after the last attack, in the same manner; a recurrence of the fever during the residence in the wards has not yet been seen, though this residence is sometimes, from the nature of the hospital regulations, protracted.

To return to the Homœopathic Hospital. In one case the fever returned after three weeks' absence, during the girl's residence in the wards; after two attacks, it again left; two of the patients I have seen in the cold fit of the fever, and can testify as to its severity; and two or three I have also seen in the hot fit. A boy who had one of the severest fevers, returned after six weeks' absence, on account of bronchitis; he was looking much better, but as he spoke nothing but Bohemian, I could not interrogate him as to his fever. The following case was also put down as an intermittent:

CASE.—W. M., a boy, aged 12, admitted on Sunday, June 28th. Next day, at visit, stated that he had had fever every day for the last four weeks. The attending sister stated that she had observed no cold fit the previous evening, though he had had a hot one. (*China, 2d dilution, four times daily.*) Upon a cross-examination, after Dr. Fleischmann had passed on, he said that for three weeks he had had a regular fever, since then every evening merely a hot fit. The percussion over the heart is dull from the third to between the seventh and eighth ribs. The first sound of the heart is somewhat impure; the pulse is irregular, every five or six beats, one failing, or rather in its place a faltering sort of beat is to be perceived; when lying on his back the heart's pulsations are not perceptible to the touch, though when he sits up, and leans slightly forward, they are then readily felt; this having been found and stated to Dr. Fleischmann, he declared that, on account of the absence of both pain and oppression in the breathing, he could not believe that there was any exudation present, he still believed him to have intermittent. On the 29th and 30th he was stated to have had regular fever, and said himself that he had a slight cold fit; after this he had no more. He was up on the 8th, and discharged on the 9th, the percussion-sound over the heart being unchanged, the first sound purer, and the pulse less distinctly intermittent.

#### RHEUMATISM.

There have been 27 cases of rheumatism; the average age of the patients was 25. The following deserve notice:

CASE I.—M. K., a young woman, aged 20, admitted on the 4th of May. The knuckles of both hands are considerably swollen and red; elbows slightly; complains of pains throughout the body and a pain in the left side; she states that she has suffered from rheumatic fever for fourteen days; there is a slight bruit with the first sound of the heart. (*Spigelia, 3d dilution four times*

*daily.*) The arms were after this enveloped in cloths dipped in cold water and covered with oilskin; these were not changed so long as they were damp; continued complaining grievously of pain till the 9th, when a diarrhea set in, after which the swelling and redness of the joints disappeared, and the pain speedily ceased. By the 16th the slight bruit had entirely gone; the joints were quite well; the diarrhea still continuing; the spigelia was stopped and ipecacuanha given; the diarrhea did not return, and on the 23d she was discharged cured.

CASE II.—J. P., aged 16, a young man, admitted Tuesday, May 12th. Said that eight days ago he had a rigor followed by a hot fit. Since then his ankles, knuckles, and wrists have been swollen, red, and painful, even on the slightest motion; he complains of pain in left chest, and has a slight cough. (*Acidum phosphoricum*, 3d dilution, four times daily.) Upon auscultation a bruit is heard with the diastole, over apex of heart. The rheumatism without further treatment was quite gone by 16th or 17th, leaving, however, a weakness in the ankles, which continued till his discharge; he is altogether a feeble-looking young man; the bruit also gradually diminished, and by the 27th was entirely gone. He was not discharged till the 10th of June.

CASE III.—Another articular affection of a similar nature, occurring in a girl aged 18, and admitted on the 10th of June, of five days' standing, was treated solely by the wet compresses, and in five days she was discharged cured.

CASE IV.—A. B., a delicate woman, aged 24, admitted on the 25th of June with a general rheumatic affection, of eight days' standing. She had for a year previously suffered under chronic gout and abdominal congestion, as she called it "leberkrankheit;" she continued without any medicine, and without expressing any peculiar feeling of pain till the 15th of July, when she got *Rhododendron*, 3d, dilution, three times daily. The pain has settled in the left knee, and almost entirely left the rest of the body; she complained of the most excruciating agony there, and continued to do so till the commencement of August, when I left. The last two or three nights she had not closed an eye, she was rapidly emaciating, and the attending sister said that her knee was much swollen, yet no examination was made.

CASE V.—K. B., a healthy-looking young woman, aged 24, admitted June 28th. Stated that a year ago she had had an attack of rheumatic fever, and since then had never been entirely free from occasional twinges; that eight days ago, on exposure to cold, the pains in her knuckles, wrists, and ankles had returned, accompanied by oppression in the chest, and occasional fits of difficult respiration; the affected parts are not the least swollen, indeed she can move them pretty freely; over the heart a bruit is heard at the apex along with the first sound; second sound is natural, but above it in the aorta is heard synchronous with the systole a loud rasping noise; her menstruation is quite correct; she has had a blister on the chest. (*Spigelia*, 3d dilution, four times daily.) The fits of dyspnea recurred for four or five days in the evening, and then left; the first sound of the heart became purer, the rasping sound in the aorta still continued; the pulse was generally quiet, and of tolerable strength, but she was much troubled with palpitation, generally recurring about noon. On the 9th of July her hands and ankles were swollen and painful, and continued so till the 13th, when the swelling again vanished; neither the appearance nor disappearance of this had any effect on the general symptoms. About the 29th of July rheumatic headaches made their appearance, and gradually increased in severity till she was discharged on the 3d of August. The sound in the aorta the same as before. The first sound still impure, the ankles weak and painful, the palpitations recurring daily.

The following case had also "Rheumatism" written as its diagnosis:

CASE VI.—J. P., a young man, aged 19, complains of pain along the region of the spine; the inferior extremities are nearly completely paralysed; when-

ever he attempts to move them he only produces slight convulsive twitches; an enlarged gland is situated in the right submaxillary region. (*Dulcamara*, 3d dilution, twice daily.) The urine flowed off spontaneously; the bladder on percussion is found to be quite distended, and in this state it remained during his residence. On the 28th of June the dulcamara was omitted, and *nux*, 1st dilution, four times daily given. He had two or three attacks of intermittent, and ipecacuanha was added on the 31st; this did not recur. On the 1st of July, having had no motion for eight days, and the abdomen being quite tympanitic, a clyster was given. After this nothing occurred; he gradually sunk and died on the 28th of July, sores having formed on his oedematous feet.

The other cases of rheumatism were pleurodynia, or slight muscular pains. The two following cases remained in the hospital when I left:

CASE VII.—A girl, aged 16, admitted on the 31st of July. She had been ill with rheumatic fever for three weeks. The right elbow and shoulder and left ankle were swollen, red, and painful; there was no heart affection. She got aconite, 2d dilution, every second hour.

CASE VIII.—A girl, A. F., aged 23, and admitted on the 27th of July, had been ill five days with a rheumatic attack in the muscles of the neck, and subject to daily exacerbation in the afternoon. However, on the afternoon of her admission this had not taken place, consequently could not be attributed to medicine, which she had not got. She improved steadily, and though well was not discharged when I left. She had aconite, 3d dilution, four times daily.

#### DIARRHEA.

There were eleven cases of simple diarrhea treated: average age 26.3. The medicines employed were acidum phosphoricum, 2d and 3d dilutions; ipecacuanha, 3d dilution, four times daily. In no case was the diarrhea checked or even moderated on giving the medicine, but in the course of a few days it gradually ceased, though frequently with previous exacerbations.

#### DYSENTERY.

Three cases of dysentery occurred.

CASE I.—A female, aged 31, admitted on Friday, May 8th. Has been unwell for eight days; yesterday the stools were, for the first time, accompanied by blood and straining; no thirst nor pain in the abdomen; tongue coated but moist; pulse very little accelerated. (*Sublimat.*, 3d dilution, four times daily.) The stools continued five or six times daily, mixed with blood and mucus till the 12th of May, when they became more natural; ceased altogether on the 13th. The medicine was stopped on the 15th, and she was discharged on the 18th.

CASE II.—A man, aged 28, who had been unwell for three or four days; his stools, on admission, the 29th of April, were chiefly blood and mucus, accompanied by straining and pain in abdomen on pressure; they amounted to 40 in the course of the day. (*Sublimat.*, the 3d dilution, four times daily.) His pulse is slightly accelerated; skin natural; tongue moist; the fever increased slightly for a day or two, the skin becoming hot, and the tongue dry, and again relaxed, both returning to their natural condition. On the 7th of May the stools had diminished to eighteen. (*Sublimat.* removed, and *rhus*, 3d dilution, three times daily, substituted; to which ipecacuanha, the 2d dilution, four times daily, was added on the 9th. By the 12th the stools were more natural, and by the 14th had entirely ceased. He remained in hospital, having been much reduced, till the 4th of June, when he was discharged.

CASE III.—A. S., a woman, aged 45, admitted July 14th. Stated that for eight days she had been afflicted with bowel complaint, having had thirty stools

in the course of each day. Each stool consisted of watery mucus, mixed with blood, and was accompanied by straining and griping. She has headache, feverishness, and sweats much, sleeping little; the pulse is full and accelerated; the tongue coated and moist. (*Sublimat.*, 3d dilution, every third hour.) 15th. Has had only one stool since admission, and that one without blood, free from pain, and more natural; slight pain in abdomen on pressure. Her bowels were not again opened till the day before dismissal, when she had a natural stool; the pain in abdomen had entirely disappeared. She was dismissed on the 20th of July.

#### CHOLERA.

Two cases resembling cholera sporadica were treated.

CASE I.—F. F., a man, aged 26, admitted Sunday, July 12th. Has always previously been healthy till Tuesday last, after eating a "saures rindfleisch," which is meat two or three days after its first cooking, rewarmed with a sour sauce, and which, I have been assured, very frequently produces the same symptoms, being a slight poison. Since then he has been almost constantly purging and vomiting; has vomited twice to-day greenish matter; stools to-day have been seven, watery, and forcibly expelled; he has headache, pain in the abdomen, cramp in the hands and feet; frequent eructations and great thirst; the tongue is red and dry; pulse fast and feeble; voice low and faint. (*Veratrum*, 2d dilution, every second hour.) 13th. Feels better; has not vomited; only twice had stools since yesterday; two attacks of cramp in the hands during night; his pulse is a little fuller, and the voice improved. 14th. Bowels again twice opened; no cramps; tongue moist, clean; pulse full and quiet; appetite returned. 15th. No pain anywhere, nor cramps; bowels opened twice. 17th. Medicine omitted, and on the 20th he was discharged.

The second case, with somewhat similar symptoms, was admitted on the 31st of July, and was left in treatment. The patient (a woman) was not, however, improving so rapidly.

#### COLICA.

There were three cases of colic.

CASE I.—This was a simple colic; ceased in a few days under the use of opium, the 6th dilution, three times daily.

CASE II.—This a colica menstrualis. The patient got *cocculus*, 6th dilution, twice daily; the third day became furiously delirious, for which she got *stramonium*, 3d dilution, four times daily, and was then removed into a private room, after which I never saw her.

CASE III.—H. B., a stout man, of 42, admitted Thursday, July 16th. He stated that he had been for years a painter; had previously had colic four or five times, for which he had been treated in the General Hospital here, and this was his first trial of homœopathy. He had intermitting pain in the abdomen, twisting about the umbilicus, and relieved by pressure; obstinate constipation; cramps in the calves of the legs and arms; loss of power in the hands; the edge of the gums where they join the teeth is of a blue colour; pulse quiet. (*Opium*, 1st dilution, four times daily.) 17th. During the night he got a clyster, which brought away some fecal matter, and he is now easier. 18th. The pain has returned somewhat; he again got a clyster, which again brought away some fecal matter, and afforded some relief. On the 20th the attack was again worse, but it had got better spontaneously by the 21st. He says himself that it is his worst attack, and that he has been more speedily relieved than at any former period. After this the pain and cramps remained entirely away, his hands were restored to their wonted condition; but his bowels were not opened since the 18th till the 27th, when they spontaneously and copiously relieved themselves. He was discharged on the 28th.

## SCARLATINA.

Two cases of scarlatina were treated.

**CASE I.**—J. K., aged 12, admitted July 9th. The next day at visit, he was lying with his eyes shut, in a half comatose state; was stated to have been very restless during the night, and was indeed bound to the bed. On being aroused, which he readily was, though he speedily relapsed, he stated that since Sunday he had been unwell with pain in the abdomen and throat. His whole skin is of a bright scarlet colour, which disappears on pressure, returning from the circumference; his tongue red and dry; throat is scarcely to be seen, on account of his unwillingness to open his mouth, and when it is forced open he complains of pain most grievously; it is internally much swollen and red; no swelling externally; he has had a few leeches on his throat; pulse very feverish; skin hot. (*Belladonna*, 1st dilution, every second hour.) 11th. An eruption like sudamina is scattered over the chest; he has been more restless than ever during the night; the comatose state still continued. The fever increasing, he died on the evening of the 12th. No section.

**CASE II.**—J. K., a boy, aged 7, admitted Sunday, July 26th. Next day, at visit, he complained of sore throat, but was unable to state how long he had been ill. Skin covered with a red rash, which disappears on pressure, returning from the circumference, and in points; he has on a thick cotton night-shirt, beneath that several cloths; his hands and feet enveloped in stockings, so that nothing but his face is left uncovered; he will not put out his tongue; his pulse is very slightly accelerated, for a boy of his age perhaps not at all. (*Belladonna*, 3d dilution, every third hour.) 29th. Tongue red and dryish; eruption beginning to fade. 30th. The cuticle beginning to scale off; his pulse is full and slow. 31st. Sitting up; a slight ulcer has formed on the tip of the tongue; medicine discontinued; his stockings, &c. are discarded; I left him running about the ward quite well.

## PNEUMONIA.

**CASE I.**—F. H., a stout-looking young man, aged 22, admitted on the 5th of May. Next day, at visit, stated that five days previously, after a sudden cooling, he had been attacked by alternating fits of cold and heat, followed by pain in the chest and cough; the two latter symptoms have since been constant, though the cough has not been very troublesome; he has, however, during the night expectorated a considerable quantity of a dark bloody-looking fluid, which still continues to be brought up in less quantity, mixed with mucus and saliva; his countenance is extremely anxious; his respiration hurried and imperfectly performed; his pulse full, but extremely rapid; on percussion the sound on the right side anteriorly is good as low down as the centre of the mammary space; on the left the same, with the exception of a space of two and a half inches from the left edge of the sternum and from the fourth rib; this space, and all beneath on both sides, are dull; this dullness posteriorly extends as high as the centre of the scapula and intrascapular spaces on both sides; the respiration on both sides, over the dull portions, is pure bronchial; over the superior portions of both lungs vesicular; though, from the above-mentioned state of the respiration, not always very distinct. (*The 3d dilution of phosphorus was ordered to be given every second hour.*) The patient died on the afternoon of the 6th.

**SECTION**, Friday, 8th, 10 A.M.—On opening the thorax both lungs were found firmly connected by cellular adhesions to the parietal pleura, particularly their inferior lobes; over the superior and external portion of the right lung was a small, soft, albuminous-looking exudation, of about half an inch thick; the inferior lobe of the left lung and the inferior and middle lobes of the right one were in a state of red hepatization, soft, and easily broken down; a considerable



quantity of reddish serosity gushed from their cut surfaces; the superior lobes of both lungs contained a quantity of frothy mucus; the cavities of the heart and large vessels contained a small soft clot; the intestines were gorged with blood, and the glands of the ileum slightly swollen. The other organs were not examined, except the spleen, which was soft, but scarcely enlarged.

CASE II.—F. H., a healthy-looking boy, of 16 years, admitted on the morning of Saturday, May 9th. At visit, same day, stated that two days ago he had a rigor followed by a hot fit, accompanied by perspiration, and followed by pain in the left breast and cough, which were, however, so slight that he came in only on account of the feverishness which still continued. He has always previously been healthy, and never subject to cough; his cheeks are flushed; tongue somewhat coated, but moist; pulse full and accelerated; sputa frothy mucus streaked with blood; percussion-sound normal; respiration vesicular, marked, however, on the posterior portion of the left side, from the lower third of the scapular space downwards by a fine crepitating râle. (*Aconite, the 4th dilution, every third hour.*) 10th. Where crepitation was yesterday audible, bronchial respiration is to day to be heard; the percussion-sound is also over the same space dull, anteriorly; from the lower portion of the mammary space downwards on the left side slightly tympanitic, otherwise normal; the sputa are now rusty, and slightly adhesive. 11th. The pulse is to-day much quieter; the physical signs remain the same. This state continued till the 13th, when over the superior portion of the dorsal space a crepitating râle was audible, accompanied by bronchial expiration; in the lower portion of the scapular space the respiration is still pure bronchial; dullness on percussion the same; right side normal in every respect. 14th. The dullness on percussion is to-day but slightly perceptible; the respiration is everywhere vesicular; over that portion of the left lung where bronchial respiration was formerly heard it is a little rougher than elsewhere, and accompanied by an occasional mucous râle. He has been a little restless and wandering in mind a little during the night; quite calm and collected now; has also had a slight bleeding from the nose; expectoration mucous; skin cool; pulse quiet and regular; tongue moist and clean. May 15th. Has again had a slight bleeding from the nose; percussion-sound everywhere normal; auscultation gave vesicular respiration, with a few mucous râles; the cough pains him slightly, and he has a general feeling of soreness over the chest; appetite has returned; his expectoration is mucous, and but slight; he continued improving, the râles not having been again heard; and on the 18th was allowed to get up, and discharged on the 21st.

CASE III.—F. D., a slightly-made boy of 16, with a clear skin and delicate complexion, admitted on Saturday, May 16th. Stated that he had been unwell and feverish since the Thursday previous, with slight cough and pain in the chest; he had formerly been subject to cough; percussion-sound over the right lung posteriorly was dull as far down as the centre of scapular space; anteriorly over the same lung tympanitic, as low as the commencement of the mammary region; over the rest of the chest normal; the respiration was found on auscultation everywhere vesicular, with the exception of the above-mentioned dull space, where it was bronchial; tongue moist and coated; pulse full and accelerated. (*The 4th dilution of phosphorus to be given every third hour.*) In this state he continued, the pulse remaining firm and full; the skin hot and dry, and the inflammation progressing daily till the 20th; when the whole of the right side posteriorly was found dull on percussion, and the upper portion of the left, as low down as the middle of the scapular space; anteriorly, over the entire right side, the sound is tympanitic; over the left side good, with the exception of the subclavicular space, where it was somewhat duller. On auscultation, pure bronchial respiration was heard over the entire right side posteriorly, and the superior portion of the left side poste-



riorly, elsewhere, the respiration was vesicular, and, though somewhat quickened, not laboured; the sputa were rusty and adhesive; the skin hot and dry; the tongue coated and dry; pulse full and bounding. 21st. On the left side, posteriorly, the dulness and bronchial respiration to-day extends as far down as the lower third of the scapular space; the other physical signs remain unchanged; the skin is cool; pulse quiet, and the tongue moist. 22d. The percussion remaining the same; the sounds, on auscultation, are so far altered, that over the acromial and upper third of the scapular space, on the right side, instead of pure bronchial, an undecided respiration is heard. May 23d. The physical signs the same as yesterday, with the addition of a slight crepitating râle occasionally audible over the right acromial and upper portion of the scapular region; the patient is looking livelier; his skin cool; pulse quiet; expectoration looser, and more catarrhal. 24th. Percussion as formerly; the respiration over the entire right side, posteriorly, was sharp, vesicular, puerile, mingled with occasional fine crepitating râles; anteriorly, on both sides, vesicular; posteriorly, on the left, undecided; marked bronchial as low as the top of the middle third of scapular space; the middle third bronchial; beneath vesicular; expectoration colourless mucus. 25th. Vesicular respiration every where to be heard, a little rougher in character over the middle third of scapular space of the left side; where the bronchial respiration was yesterday audible, no râle is to-day audible. 26th. Physical signs as yesterday; the dulness on the left side perhaps a little less distinct, and a few subcrepitant râles are heard over the right posterior dorsal space; cough is not so troublesome; expectoration easy, mucous. 27th. Left acromial and scapular regions decidedly clearer; no râle audible; respiration every where vesicular. 28th. Entire left side clear on percussion; otherwise no change. 29th. No change. 30th. Dulness over right side; posteriorly, less in intensity; anteriorly, normal; left side normal; numerous mucous râles are to-day mixed with the vesicular respiration; the expectoration and cough are somewhat increased. (*Medicine to be given only thrice a day.*) The patient continued daily to improve, and on the 4th of June there was only an occasional mucous râle audible over the right dorsal region; and on the 6th full, clear, vesicular respiration every where audible; the chest expanding well, and the cough gone; but still the percussion slightly dull, though improved, on the right side posteriorly; after this, the patient was always up and dressed during visit, and was discharged on the 11th of June.

CASE IV.—L. U., a stout healthy-looking young man, aged 18, admitted on Saturday, the 16th of May. Stated at visit, next day, that he had been on the previous Wednesday seized with pain in the chest and cough, which continue. Percussion-sound anteriorly over left side normal; over right side tympanitic, as low as commencement of mammary space; posteriorly, left side, normal; right side dull down to centre of scapular space; beneath normal; respiration every where vesicular, except over dull space on the right side posteriorly, where it was bronchial; sputa rusty, adhesive; pulse quick and full. (*The 3d dilution of phosphorus to be given four times daily.*) May 18th. Physical signs the same; skin covered with profuse perspiration; bowels opened six times. 19th. Bowels to-day not opened; on auscultation, where the bronchial respiration was audible, it is now indistinct and mixed with a fine crepitating râle; pulse quiet and regular; expectoration mucous. 20th. Respiration to-day every where vesicular; the crepitant râle still audible in the same situation as yesterday. 21st. The percussion-sound over affected part is to-day somewhat clearer; condition otherwise the same. 22d. Percussion-sound still improving, and on the 23d every where good; the vesicular respiration is every where audible, and free from admixture of râle; cough gone; after this the patient remained in hospital free from all complaint till the 28th, when he was discharged.

CASE V.—E. B., a stout-looking woman, aged 48, admitted Saturday, May 16th, afflicted with pain in chest and difficulty of breathing. She got the 3d dilution of *aconite*, four times daily; exchanged on the 20th for the 3d dilution of *phosphor.*, four times daily, and being on the 21st evidently much worse, mind excited and wandering; expectoration retained. *Tartarus emeticus* was given in the 2d dilution, three times daily. The patient died during the day.

SECTION, Saturday, May 23d, 10 A.M.—Extensive plastic exudation was found between the visceral and parietal portions of left pleura, forming at the lower portion a sac containing fluid; the entire inferior lobe of left lung in a state of gray hepatization; right lung and upper lobe of left, though not to the same extent, gorged with blood and serum; bronchi filled with frothy mucus; right lung connected to costal pleura by old cellular adhesions; a small amount of plastic exudation in the pericardium; the right heart contained a fibrous clot, the left one was empty; other viscera, so far as examined, normal.

CASE VI.—A. T., a stout-looking man, aged 46, admitted Thursday, May 21st. Same day, at visit, stated that he had caught cold, and had now great pain in breathing, accompanied by a slight cough; he has not expectorated since admission; percussion every where normal; respiration vesicular; pulse full, strong, and accelerated. (*Aconite*, the 3d dilution four times daily) May 22d. Expectoration rusty, blood-stained; percussion anteriorly on left side normal; on right side dull as high as centre of mammary space; posteriorly, on left side, normal; on right side dull as high as centre of scapular space; dulness likewise extends round over the lateral and infralateral spaces; elsewhere vesicular; pulse still full and bounding; *aconite* stopped, and *phosphor.* given, the 3d dilution, four times daily. 23d. Not examined, on account of his evidently dying condition; the breathing hurriedly and imperfectly performed; the countenance extremely anxious; pulse still more accelerated, but failing in firmness and strength. He died in the course of the day. No section was made.

CASE VII.—F. B., a young woman, aged 24, admitted Sunday, May 24th. Next day, at visit, she stated that, having been always previously healthy, she had five days ago caught cold, and since then been suffering from pains in the chest, accompanied by cough, most troublesome at night. Expectoration slight, rusty, and adherent, has no headache, and no appetite, but great thirst; percussion-sound good, except posteriorly over right lung, as far down as centre of scapular space; beneath this again good; on auscultation, mucous râles are heard pretty generally distributed over both lungs; over the dull space bronchial respiration is audible; pulse accelerated; tongue coated, inclined to be dry. (*Phosphor.* 2d dilution, three times daily.) 26th. Physical signs the same; the râles, however, are not audible to-day; the tongue is moister, pulse quieter. 27th. The expectoration more mucous, less adhesive, still blood-stained; respiration undecided over dull portion. 28th. Respiration every where vesicular; over the dull portion rougher; more puerile; expectoration mucous; the dulness is also a shade better. After this she continued improving; by the 31st her cough was entirely gone; the medicine was, however, not discontinued till the 2d of June, and she was discharged on the 4th with still a shade of dulness over the right side, posteriorly extending, however, no farther than the top of the scapular space.

CASE VIII.—A. S., a young man, aged 17, admitted Wednesday, May 27th. Next day, at visit, stated that the previous Sunday he had fallen suddenly to the ground without known cause; had always previously been healthy. Has since then been troubled with headache, sleeplessness, slight cough, and pain in the chest; he feels weak, and is tormented by thirst; his tongue is red, but moist; skin natural, and pulse but slightly accelerated; the percussion-sound is anteriorly normal, with the exception of over the right cla-

vicle, where it is slightly dull, and where a fine mucous râle is to be heard; posteriorly, on the left side, normal; on the right one also good, down to nearly the inferior border of the scapular space, beneath which it is dull; the respiration is vesicular, obscured posteriorly on the right side by loud, sonorous, and sibilant râles; expectoration, a jelly-like mucus. (*Phosphor.*, 4th dilution, every third hour.) 29th. Face to-day has a dingy appearance, with a dusky red spot on each cheek; pulse more accelerated; sputa adherent, of a deep orange colour; percussion as yesterday; pure bronchial respiration over the posterior inferior portion of the right chest, mentioned yesterday as dull; superiorly, still an occasional sonorous râle to be heard. 30th. The dulness on percussion extends now half across the inferior portion of the lateral and infralateral spaces; anteriorly, the right infra-mammary and inferior portion of the mammary are tympanitic, the rest normal; bronchial respiration is audible over the dull portions; sonorous râles are now more audible over the right anterior portion of the chest. 31st. Posteriorly, the percussion-sound remains the same; the portion anteriorly, which gave yesterday a tympanitic sound, gives to-day a slightly dull sound; where bronchial respiration was yesterday audible, the respiration is to-day indistinct, and accompanied by consonating râle. June 1st. The percussion remaining the same, a fine, loud, consonating râle is heard over the dull spaces, both anteriorly and posteriorly on the right side; the left side still gives only vesicular respiration; the skin is still dingy; the sputa less yellow and more copious; the pulse quieter. 2d. The percussion unaltered; the râles less frequent; the expectoration more copious, and now of gray mucus; the patient sleeps better, and the colour of his skin has decidedly improved. 3d. Percussion, posteriorly and laterally, as formerly; anteriorly normal, except over right clavicle, where dulness still continues; auscultation as formerly; expectoration grayish mucus; tongue moist and natural; skin cool, pulse quiet. 4th. A few consonating râles still to be heard, mixed with an undecided respiration. 6th. Percussion good, excepting, as before, over the right clavicle, and also the right infralateral and inferior portion of lateral region, where it is still a little dull; the respiration is also there undecided, elsewhere vesicular; somewhat puerile over the posterior inferior portion of right chest. On the 8th, the percussion and respiration being everywhere normal, except over right clavicle, where slight dulness still continues; the medicine was discontinued; the expectoration slight; cough almost gone. On the 10th he was up, and on the 15th discharged, both having entirely ceased.

CASE IX.—M. U., a young woman, aged 19, admitted on Thursday, May 28th. Stated next day, at visit, that on the Sunday previous she had been seized with shivering, followed by heat, pain in the chest, and cough. Has been previously subject to cough; expectoration tolerably copious, adherent, and blood-stained; percussion normal, except posteriorly, on the right side, where it is dull as low down as the centre of the scapular space; beneath that normal, over the dull portion bronchial respiration is heard; over the rest of the chest sonorous and sibilant râles. (*Phosphor.* the 2d dilution, to be given every second hour.) 30th. Expectoration more copious; cough more troublesome; physical signs unchanged. 31st. Breathing laboured and anxious; entire right side, posteriorly, dull; anteriorly also; percussion-sound less clear than on the left; posteriorly, over the right chest, bronchial respiration was heard, mixed with consonating râle; over the left side, sonorous and sibilant râles; anteriorly, over both sides, loud, sonorous, and sibilant râles to be heard; expectoration still copious; pulse full and accelerated. June 1st. Physical signs unchanged; breathing less oppressed, and her look less anxious. 2d. The râles are to-day not so frequent. 3d. They are to-day much louder, especially anteriorly, and quite perceptible to the touch, i. e. the thrill caused by them is so; and the respiration is again more laboured; the pulse is, how-

ever, not so feverish. 4th and 5th. Her condition continued to improve during these two days; the expectoration becoming more copious, and more free from blood-stain; and on the 6th, there were anteriorly no more râles to be heard; the percussion, anteriorly, is normal; posteriorly, the right side is still dull; the respiration there is, superiorly, undecided; inferiorly, disguised by sonorous râles; a few of these are also to be heard over the left chest posteriorly; the expectoration is copious, gray frothy mucus; the cough is troublesome. (*Medicine to be given only twice daily.*) 7th. Except an improvement in the percussion-sound of right chest, her state is unaltered. 8th. Percussion everywhere good; respiration vesicular; disguised, posteriorly, on the right side, by sonorous râles. 10th. Vesicular murmur to-day everywhere audible; cough greatly better; expectoration more easily brought up, less copious. On the 11th she was allowed to rise; and was discharged on the 13th, her cough having entirely ceased.

CASE X.—M. B., a stout healthy-looking man, aged 58, admitted Tuesday, June 9th. On the morning of the 10th he was going about the ward, and was not noticed till the end of the visit, when there was no time to examine him; no medicine was ordered. On the 11th he stated that he had come in the instant he felt himself unwell; he complained of pain in the chest and cough; the expectoration copious, adherent, of an orange tint; pulse full, but slightly accelerated; percussion, anteriorly, over the left side, tympanitic; over the right normal; posteriorly, the entire left side dull; the right one normal; respiration vesicular, except posteriorly, over the left side, where nothing is audible but a loud consonating râle. (*Phosphor. 2d dilution, every third hour.*) This state remained unchanged till the morning of the 15th, when the percussion-sound remaining the same; instead of the consonating râle formerly heard, a fine crepitating one is to-day audible over entire posterior surface of left chest; the expectoration is copious, mucous; the pain very much better. 16th. The respiration is to-day vesicular, a few crepitating râles being still audible over their yesterday's seat; the percussion-sound is also less manifestly dull; the expectoration less copious, more easily brought up, and cough less troublesome. He gradually improved till the 21st, when the percussion-sound was normal, and the respiration vesicular over the entire chest; a full inspiration still, however, produces a subcrepitating râle over the former seat of disease; and this continued to be the case till the 2d of July, the patient having been up and going about since the 22d of June: on the 5th of July he was discharged.

CASE XI.—W. B., a boy of 15 years of age, admitted Friday, June 12th. On the 13th, at visit, stated that for four days he had been troubled with a cough and severe pain in the left side. Expectoration rusty and adherent; pulse but little accelerated; the percussion-sound normal, except at the under portion of left chest, where it is dull anteriorly as high as the centre of the mammary region, and posteriorly as high as the centre of scapular region; the dulness extends entirely round, through the lateral and infralateral regions; the respiration is over the dull portion bronchial, elsewhere vesicular; the heart's tones are clear and unaltered. (*Phosphor. 3d dilution, every third hour.*) His condition remained unchanged, his nose having bled slightly on the 16th till the 17th, when, the percussion being the same, the respiration is over the dull portion undecided, mixed with mucous râle; bronchophony still continues; expectoration mucous. 18th. Again a bleeding from the nose during night. 19th. The percussion-sound is to-day somewhat clearer; mucous râle still audible, and, accompanying the respiratory movements, a fine rasping sound is to-day for the first time audible, about the lower edge of the scapular space. 20th. The rasping sound is to day almost inaudible; the respiration vesicular, with a few mucous râles; the percussion is everywhere normal: the expectoration trifling, and the cough almost gone; he is anxious to get up. 21st. Up

and dressed; chest expanding fully, and without pain. On the 25th the cough having entirely disappeared, he was discharged.

CASE XII.—A. S., a delicate-looking young man, aged 25 years, admitted Sunday, June 12th. Next day, at visit, stated that for the last day or two he had lost his appetite; had pain in the left side; feverish pulse; no cough; tongue coated, and a slight griping diarrhea, bowels having been opened three or four times daily; he had been treated in this hospital December previous for pneumonia; the percussion was normal; as also the respiration. (*Ipecacuanha*, 3d dilution, four times daily.) The same afternoon, pneumonia having manifested itself, he got from the assistant, *Phosphor*. 3d dilution, every third hour, the preceding medicine being discontinued. 14th and 15th. No visit, in consequence of religious feasts. 16th. Expectoration copious, partly rusty, partly of an orange tint; percussion every where normal, except posteriorly over the inferior portion of the left chest, where it is dull as high as the inferior border of the scapular region; over this dull portion the respiration bronchial, elsewhere vesicular; pulse feverish; skin hot and dry; respiration somewhat laboured. 17th. The percussion remaining the same; the respiration is to-day undecided; the pulse quieter; the expectoration more mucous, blood-streaked; the breathing more easily performed. 18th. The dulness to-day extends as high as the centre of the scapular region on the left posterior surface of the chest; elsewhere it is normal; over the superior portion of this dull space the respiration is undecided for a short distance down, perhaps the distance of two ribs with the intervening space; next to this comes a layer of pure bronchial respiration of equal extent, and beneath, the respiration becomes again undecided; over the other parts of the chest the respiration is vesicular; the sputa are mucous, and more easily brought up; cough less troublesome, and unattended with pain. 19th. The percussion the same; over the superior portion of left dorsal region a fine crepitating râle is audible; immediately above this a layer of undecided respiration, and above this, up to the centre of left scapular region, the respiration is bronchial; elsewhere vesicular. The crepitation gradually progressed over the diseased portion, till by the 23d it occupied it entirely; the sound, on percussion, had also gradually improved, and was now almost normal. On the 24th, he was up and dressed; cough infrequent; still feels a slight uneasiness in left chest, but is able to inspire fully without pain. On the 27th he was discharged.

CASE XIII.—A. L., a stout boy of 14 years, admitted on Sunday, June 28th. Next day, at visit, stated that he had caught cold on the previous Friday, after bathing, and on the Saturday felt himself unwell, with pain in the chest and cough, to which he had never been subject; the expectoration is rusty and adherent; the percussion, posteriorly, on the right side, is dull as high as the top of the lower third of the scapular region; anteriorly, the sound is on the same side tympanitic, up to about the centre of the mammary region, elsewhere normal; the respiration is vesicular, except over the dull portion, where a fine crepitating râle is heard, followed by bronchial expiration; the pulse is full and accelerated; skin hot and dry; tongue moist. (*Phosphor*. the 3d dilution, four times daily) 30th. The dulness on percussion extends posteriorly as high as the centre of the scapular region; laterally the infralateral and lower half of the lateral region are dull; anteriorly, the dulness extends nearly as high as the centre of the mammary space above; for the distance of rather more than one intercostal space the sound is tympanitic, gradually fading into the normal; elsewhere it is normal; the respiration over the dull portion posteriorly is bronchial; anteriorly and elsewhere vesicular; in the lateral region the one passes into the other by means of an intervening undecided respiration. July 1st. Posteriorly, from the centre of the scapular region to the top of it, the sound is to-day tympanitic, otherwise as yesterday; the bronchial respiration extends now as high as the top of the scapular region; anteriorly



and inferiorly over the dull portion there is no respiration audible; the expectoration is more copious, looking like a dark brown jelly; tongue moist; pulse quiet, and skin more cool. 2d. Physical signs as before, with the addition of a friction-sound at the lower border of the right scapular region; laterally it is inaudible, but anteriorly it is again met with about the centre of the mammary space, where the respiration on auscultating from above begins to vanish; the expectoration is still rusty and blood-streaked. 3d. Expectoration copious, more catarrhal, being simple mucous, with a few blood-streaks; posteriorly a mucous râle, by which the friction-sound is marked, anteriorly it is still audible; otherwise condition the same. 4th. Percussion-sound is posteriorly a little clearer; on auscultation there, a cooing sonorous râle is first heard, followed by a fine crepitating râle; anteriorly the friction-sound has disappeared; expectoration still copious, mucous. 5th. Percussion-sound posteriorly good, anteriorly beginning to clear; posteriorly the respiration is vesicular, with a few crepitating râles. He continued to improve till the 9th, when the vesicular respiration anteriorly began to be heard; the percussion-sound being much improved, but not entirely normal; a little crepitation is still to be heard posteriorly; cough and expectoration slight. 10th. The vesicular respiration is every where restored. 11th. The percussion is to day every where normal; the sole remnant of disease being a few crepitating râles still audible on the right, posteriorly and inferiorly. On the 12th he was discharged.

CASE XIV.—E. B., a delicate-looking woman, aged 20, of a clear complexion and nervous temperament, admitted on Sunday, June 21st. On the Monday following, at visit, she stated that she had been unwell for about four days, with feverishness and pain in the chest. She had for some weeks latterly been troubled with a cough, which had within a short time almost entirely ceased, but when it did come, pained her more than ever. The expectoration is rusty and scanty; skin natural, and pulse but slightly accelerated; percussion, anteriorly, dull over right subclavian space, where even the slightest pressure pains her, elsewhere normal; posteriorly, on the right side superiorly dull as far down as the centre of the scapular space, elsewhere normal; respiration is anteriorly vesicular; posteriorly, over the dull space bronchial; beneath it on the right side are numerous mucous râles, on the left vesicular. (*Phosphor. 3d dilution, four times daily.*) 23d. Percussion as yesterday; to-day, over the dull superior portion of the right scapular region a fine crepitation is heard on inspiration, followed by bronchial expiration; the mucous râle is to-day audible anteriorly as well as posteriorly; the expectoration is still rusty, adherent, and scanty. 24th. State the same. 25th. Pain much relieved; percussion unchanged; posteriorly, over dull portion crepitant râle followed by bronchial expiration; anteriorly, superiorly over dull portion consonating mucous râle; inferiorly anteriorly, and posteriorly on the right, mucous râle; expectoration more mucous, still scanty. 26th. Percussion anteriorly as before; posteriorly the dullness extends over rather more than two thirds of the right scapular and interscapular spaces, including also the acromial; on auscultation a consonating mucous râle is heard posteriorly over dull space; inferiorly on the right mucous râle, which is also heard anteriorly, and occasionally over the left side; expectoration more copious, frothy, mucous, mixed with rusty and yellow tougher pieces; tongue red, but moist; pulse more accelerated, rather feeble; skin hot and dry. 27th. The dullness and consonant râle extend to-day over entire right scapular and interscapular spaces; anteriorly the percussion over the superior portion of mammary space is slightly tympanitic, otherwise as before; the auscultation on the right as yesterday; on the left side the quantity of mucous râle is greatly increased, and the thrill caused by it quite perceptible to the touch; tongue posteriorly coated with a red point, inclined to be dry; pulse small and accelerated; expectoration as yesterday, but not so copious; troubled



with disagreeable dreams and startings from sleep by night. 28th. Physical signs much the same; expectoration more copious, thin, frothy, mucous; pulse not so fast, and scarcely so thready in character; the skin is cooler and moist; the tongue moist. 29th. Percussion as before; over the lower half of the right scapular and interscapular region a crepitating râle is audible; superiorly a consonating, and inferiorly a non-consonating mucous râle; a quantity of mucous râle audible anteriorly and over the entire left side; the expectoration copious and mucous. 30th. Still improving; skin cool. July 1st. The dullness is now clearing off; posteriorly, on the right side mucous râle without consonance; anteriorly and over the entire left side nothing audible but vesicular respiration; expectoration still copious. 2d. As yesterday, with exception of a fine friction-sound to be heard about the centre of the right scapular space. She continued improving in every respect till the 5th, when the dullness anteriorly was quite gone, posteriorly but slightly perceptible; a fine crepitating râle is heard over the top of the right scapular space; the friction-sound, is gone, having only been audible for two days; the respiration is everywhere vesicular. 7th. The chest sounds everywhere normally on percussion; the respiration is everywhere vesicular, accompanied over the superior portion of the right dorsal region by a few mucous râles, which, by the 9th, had, along with the cough and expectoration, entirely disappeared. The medicine was then stopped. She was allowed to sit up for half an hour daily, and continued gaining rapidly in strength till the 12th, when she was discharged.

CASE XV.—A stout-looking young man, of about 25, admitted on Friday, July 10th. At visit, the same day, stated that two days ago, without known cause, he had been seized with pain in the right side, cough and feverishness; he has always previously been healthy, and never subject to cough; he has, he says, within the last day or two expectorated a small quantity of blood; the expectoration is to-day scanty, frothy, mucous, with a streak or two of blood; the percussion anteriorly normal, posteriorly on the right side, dull over the acromial and upper half of the scapular regions; the axillary and upper portion of lateral region on same side are also dull to about their vertical centres; on the left side normal respiration; posteriorly over entire right side, and laterally over dull portion fine crepitating râle; elsewhere vesicular; pulse not much accelerated. (*Phosphor. 3d dilution, four times daily.*) 11th. Right side posteriorly dull over entire surface; otherwise percussion as yesterday, inferiorly and posteriorly on right side; a fine subcrepitant consonating râle over the superior portion of scapular and acromial regions on same side; sonorous râle with occasional bronchial respiration, which is likewise heard over the right axillary and superior portion of lateral regions; left side normal; sputa rusty and scanty. 12th. Percussion posteriorly, as before, laterally clearing off; posteriorly over dull side, mucous consonating râle; crepitation laterally; sputa rusty; pulse quieter; bathed in a copious perspiration. 13th. Skin cool, and still moist, but not perspiring so much as yesterday; physical signs unchanged; sputa mucous. 14th. Percussion-sound clear anteriorly and laterally, posteriorly clearing; sputa mucous and scanty; respiration everywhere vesicular, with an occasional râle where it was formerly dull. 15th. Percussion-sound everywhere normal; an occasional crepitating râle to be heard over the right side posteriorly; respiration everywhere vesicular. 16th. The crepitating râle is to-day only audible on very full inspiration; the cough and expectoration almost entirely gone. Discharged on the 17th.

CASE XVI.—J. H., a stout boy, of 10, admitted on the afternoon of Monday, July 13th. Stated next day, at visit, that he had been, previous to admission, only one day unwell. The sputa are few and rusty; cough not very troublesome; pulse full and accelerated; tongue coated, but moist; percussion pos-

teriorly on the left side dull, right normal; over the left lateral and axillary regions tympanitic; anteriorly, on the left, slightly dull, elsewhere normal; respiration, posteriorly on the left, bronchial, elsewhere vesicular. (*Phosphor. 3d dilution, every third hour.*) 15th. His condition was unchanged. 16th. Percussion is to-day dull over the right axillary and lateral regions, otherwise unchanged; over this dull portion bronchial respiration is heard; the auscultation is otherwise unchanged; the pulse is quieter. 17th. The expectoration is to-day more catarrhal; on the left posteriorly the bronchial respiration is not so decided. 18th. Percussion on the left posteriorly still dull; laterally and anteriorly tympanitic; respiration posteriorly on the left is vesicular, preceded by a sonorous râle, elsewhere on the left laterally; rough, vesicular, puerile on the right, and anteriorly on the left normal; expectoration slight. By the 21st the vesicular respiration was audible over the entire chest, and but a slightly perceptible dullness remained on percussing the left side posteriorly, which by the 23d had, along with the cough and expectoration, entirely disappeared. On the 25th his medicine was discontinued, and on the 1st of August he was discharged.

CASE XVII.—J. L., a stout boy of 16, admitted July 24th. Stated that he had been ill one day, with pain in the chest and slight cough; the expectoration is very scanty, yellow, and rusty, with a few streaks of blood, adherent; percussion anteriorly normal, posteriorly normal, except the inferior portion of right side, beneath the centre of the scapular space, where it is dull; and over this dull portion is heard on inspiration a subcrepitant râle, followed by bronchial expiration; elsewhere the respiration is normal; the pulse remains quiet; tongue moist, and skin natural. (*Phosphor. 3d dilution, every third hour.*) 25th. The râles are to-day more numerous, almost masking the bronchial expiration; the expectoration copious, of a brownish dirty-looking jelly. 27th. There is to-day a mucous râle over the left side also; the physical signs otherwise unchanged; the pulse has become somewhat accelerated. 28th. Anteriorly the percussion is dull on the right side, beneath and over the lower third of mammary region, also over the right infralateral and lower portion of the latter; a consonating râle is audible over dull space anteriorly and posteriorly; elsewhere vesicular respiration and mucous râle to be heard; skin hot; expectoration as before. 29th. Percussion-sound is anteriorly dull over entire mammary region, superiorly over right subclavicular, somewhat tympanitic; the respiration as formerly; the expectoration scarcely so copious; tongue red, inclined to be dry. 30th. The percussion improved to-day posteriorly; the râle thin, no longer consonates; the pulse is quieter; the skin moist and cool; tongue clean. 31st. Physical signs unchanged; the expectoration is scanty, colourless mucous; pulse quiet; cough not so troublesome. August 1st. Percussion posteriorly clear, anteriorly improved; respiration rough vesicular, puerile, over what was the dull posterior portion, and anterior also; expectoration mucous, still streaked with blood. 3d. The percussion is to-day still slightly dull anteriorly; vesicular respiration; cough almost gone, and expectoration scanty. 4th. Discharged.

To the foregoing must be added two other cases, of which I have not full notes, viz.:

CASE XVIII.—A. S., a male, aged 21, admitted May 2d. (*Phosphor. 4th dilution, four times daily.*) Discharged cured after 18 days' treatment.

CASE XIX.—F. O., a male, aged 23, admitted 9th May. (*Phosphor. 3d dilution, four times daily.*) Discharged cured after 12 days' treatment.

These cases of Pneumonia give an average age of 24, an average treatment of 12.6 days, and a mortality of 15 per cent., 3 out of 19 having died.

Skoda's cases of Pneumonia, during the same time, amount to 45, his deaths to 3, giving an average of 6.6 per cent.

During this period no bloodletting of any kind was used in Dr. Skoda's wards, his treatment being, extract. graminis  $\mathfrak{Hj}$ , or nitri puri, or sublimat.  $\frac{1}{2}$  gr., by way of attempting to reduce the plasticity of the blood. He also gave occasionally pulv. Dover, gr.  $\mathfrak{vj}$ , in the course of the day. I regret that, from the patients tiring of one medicine, in no case during these three months did I see a case treated solely with extract. graminis; but hundreds could be extracted from his books treated solely with nitri puri gr.  $\mathfrak{v}$ , ad gr.  $\mathfrak{xx}$ . This seems to be his favorite remedy, though at present he is trying the new chemical theories, and gives the patients sublimate. His average mortality for the last three years, during which no bloodletting—or most rarely—has been performed, and not one leech or cupping-glass applied, is 13·7 per cent., and the recovery of the patients has always been very speedy. He told me that, in the year 1840, he treated 64 females, affected with pneumonia, with large bleedings and large doses of tartar emetic, and only lost one; yet the deaths amongst the males the same year made the total deaths amount to 1 in 8. He considers that this is about the general proportion under all treatments. The great advantage of not bleeding, he considers to be the speedy recovery. The average age was 25·3.

#### OTHER DISEASES.

During the above period there were eleven cases of TUBERCULOSIS admitted. They were treated chiefly with *sulphur* 4. Two died, and two were left in the house; the others were discharged, some of them a little relieved.

Five cases of CATARRH and two of BRONCHITIS were also admitted; one of the latter following measles. They were treated chiefly with *sulphur*, 4th dilution, and, where the fever ran high, with *aconite*, 3d or 4th dilution.

Two cases of GENERAL SPASMS (at least complained of), were treated—the one with *chamomilla*, 6th dilution, twice daily, the other with *ignatia*, 6th dilution, once daily. They both recovered.

One case of CHOREA was treated with *ignatia*, 3d dilution, three times daily, and was discharged relieved.

Three cases of enlarged SCROFULOUS GLANDS were received and discharged, two in *statu quo*, one after suppuration.

One ASTHMATIC AFFECTION, without apparent cause, except hysteria, in a young girl, aged 18, was treated with *ammon. carb.*, 3d dilution, four times daily, and was cured.

A case of GOITRE was treated with *iodine*, 2d dilution, four times daily, and was discharged in five days; the breathing somewhat relieved.

One case of VARIOLA MODIFICATA ran its course regularly without any medicine at all.

One case of ICTERUS, of three weeks' standing, was discharged cured after twenty days; treatment with *china*, 2d dilution, three times daily.

Six cases of ANGINA TONSILLARIS, slight; treated with *belladonna*, 3d, 4th, and 6th dilutions, three times daily, and *hydrargyrum*, 2d and 3d dilutions, four times daily. Average treatment not three days.

One patient with *Catarrhal Ophthalmia* got on her entrance *sulphur*, the 3d trituration, four times daily, but the following day was not to be seen, so I suppose had been quietly discharged.

One case of old age, dead.

One case of *General Dropsy*, discharged after five days' treatment with *lactuca*, 3d dilution, four times daily. Improved slightly.

One case of *Exudation in the Peritoneum*, in a young boy, aged 12. The effusion was absorbed during the employment of *bryonia*, the 3d dilution, four times daily: it was followed by an inflammation and suppuration of the tunica reflexa, and the case was left under treatment.

Two cases of *Effusion into the Pleura* were discharged unimproved.

There were in all seventy-one cases in which the patients got no medicine,

including several surgical cases, as ulcers, injuries, &c., and one case of burn, which was treated with a turpentine lotion. The other cases were *Chlorosis*, *Headaches*, slight *Stomach Complaints*: *Expectoration of Blood*, in one case caused by a blow, in another, by hypertrophy of the heart.

But I must now conclude my narrative, incomplete and imperfect as it is. I hope you will excuse the hasty manner in which it has been drawn up, from consideration of the circumstances under which I write. I was anxious to send off my letter before leaving Vienna (which I do to-morrow, August 15th), and I thought you would rather have my account, even in a rough form, early, than in a more digested form at a later period. I possess a complete list of all the patients admitted into the hospital during the three months—a statement of the medicines administered—the length of the treatment, and period of residence in the hospital, &c., which I hope to transmit to you.

I think you will see by what I have stated, that the strength of the homœopaths lies not in the greater rationality or practical superiority of their treatment, but is founded on the weakness of allopathy; that they not only do not help their patients, but—if they are strict homœopaths—are for ever shut out from helping them;—that in their treatment of acute diseases—simpler, at least, if not better than that of their opponents—their success depends entirely on the hitherto unrecognized powers of Nature—all the magic influence of their infinitesimal doses of phosphorus, &c., being emulated if not excelled by the heroic virtues of *Extractum Graminis*.

I remain, my dear sir, yours faithfully,

GEORGE W. BALFOUR.

#### REMARKS BY THE EDITOR.

The preceding Report may be truly said to speak for itself, yet it may not be inexpedient to advert to a few of the more important particulars contained in it, especially those which bear most pointedly on the main questions to which this part of our Journal is for the present devoted.

1. In the first place, it seems clearly made out, from Dr. Balfour's account—confirmed and corroborated as it is in this point by Dr. Mühry's letter—that what is now called Homœopathy, is far from being, either in principle or practice, always or necessarily the system first promulgated by Hahnemann under this name. We think it evident, from Dr. Balfour's cases, that even Hahnemann's avowed follower, Fleischmann, sets little store by the fundamental principle, *similia similibus*, in the administration of his remedies.

2. Secondly, the facile and uniform attainment of the same triumphant results under all these varieties of the homœopathic system, points to the operation of one and the same curative agent in all—namely, our old friend, the *Vis Medicatrix*.

3. The great and manifold divisions already existing among the homœopaths, seem to lead irresistibly to the conclusion that the beginning of the end of homœopathy is already come. If we have the very best authority for believing that a house divided against itself cannot stand, we can hardly doubt that the house of Hahnemann is now tottering to its fall.

4. It is sufficiently evident from many passages in the Report, that whatever might have been the comparative general result of adopting the ordinary (allopathic) treatment in the cases related, its adoption would have been decidedly advantageous in particular instances, by affording aid to nature in relieving distressing symptoms, which were not relieved, nor attempted to be relieved, by the homœopathic treatment. Whether homœopathy be true or false, there can be no doubt that Dr. Fleischmann's treatment was often bad.

5. But whatever may be thought of homœopathy as a doctrine, or as a general system of practice, it will not be doubted, after this Report, that the

more common acute diseases may and can, during a certain period of time at least, run their course favorably enough under its administration. The general results of the treatment during the three months reported of, were we think, such as would have satisfied many practitioners of our ordinary medicine.

6. Neither will it any longer be denied that PNEUMONIA, even in a severe form, may, under homœopathic treatment, pass through all its stages to perfect recovery.

7. The great and important practical question is—Whether or not the homœopathic remedies administered in these cases contributed in any degree—or if in some degree, in what degree—towards the cure of the diseases, particularly the cases of pneumonia? This is a question which will be answered differently by different persons. No doubt, Dr. Fleischmann and homœopaths generally will regard these cases not only as highly favorable to the claims of homœopathy, but as unquestionable proofs of its great remedial powers. We, on the contrary, in common with our reporter, see no other powers operating in these cases but the natural powers of the living system, called into action under very favorable circumstances. The general aspect of the whole cases, favorable and unfavorable alike, and the minute details of each case, convey to our mind the most perfect conviction that, throughout, Nature, not art, was the worker; and it would seem easy to convey to others the grounds of this conviction, by a minute analysis of the individual cases through their whole progress, whether the patients were getting better or getting worse, whether taking or omitting the homœopathic medicaments.

8. The same doubts as to the efficacy of the medicines employed may exist in regard to the cases of Professor Skoda. But if the advocates of homœopathy insist on the value of the evidence in favour of the deillionths of phosphorus, aconite, or bryonia, they surely have no right to reject the evidence of a precisely similar kind, in favour of Dr. Skoda's grain doses of the extractum graminis or nitre. We may perhaps be forgiven even by the most zealous partisans of ordinary medicine, particularly the abettors of the heroic system, for setting down both systems as equally effective in the case in question, that is, as having no effect at all.

9. The results of the treatment of the cases of pneumonia, both by Fleischmann and Skoda, seem to prove that bloodletting is not so essential and indispensable a remedy, even in severe cases of pneumonia, as has usually been supposed; and that its omission, in a certain proportion of cases at least, does not even retard the period of convalescence or the completion of the cure.

10. The materials supplied in Dr. Balfour's Report, like those formerly supplied in Dr. Fleischmann's, do not in any degree authorize the general conclusion that homœopathic treatment is as good as that of ordinary medicine, much less that the latter like the former is valueless, Nature being all-sufficient in the cure of diseases; but both go powerfully to corroborate the following, among other important inferences formerly deduced by us from a review of the whole question, viz.—1. That Nature is more powerful in curing diseases, and has practically a much greater share in the ordinary cure of diseases, than is commonly believed. 2. That the prevailing doctrines respecting the actions and powers of many particular medicaments and other so-called remedial agents, require reinvestigation, with a view to ascertain the truth. 3. That while this investigation is in progress, we should be cautious, even timid, in the use of medicaments and other medical means capable of exerting a powerful influence on the animal system, and therefore capable (possibly) of doing evil as well as good. 4. That in the present state of our knowledge, the Hygienic—Eclectic—Hippocratic—Natural system of treating diseases, is the only one that can be justified or safely followed.

## BOOKS RECEIVED FOR REVIEW.

1. The Sanative Influence of Climate. By Sir James Clark, Bart. Fourth edition. London, 1846. 8vo, pp. 412. 10s. 6d.
2. Practical Surgery. By Robert Liston. Fourth edition, London, 1846. 8vo, pp. 582. 22s.
3. Observations on the Principle of Vital Affinity, as illustrated by Recent Discoveries in Chemistry. By W. P. Alison, M.D., F.R.S.E. (From the Transactions of the Royal Society of Edinburgh.) Edinburgh, 1846. 8vo, pp. 23.
4. The Open Surgery Question considered. By W. J. Preston. London, 1846. 8vo, pp. 25.
5. Meade's Manual for Students preparing for Examination at Apothecaries' Hall. Second edition. London, 1846. 8vo, pp. 386. 10s. 6d.
6. The Economy of the Animal Kingdom, considered Anatomically, Physically, and Physiologically. By Emanuel Swedenborg. Translated from the Latin by the Rev. A. Clesseld, M.D. Two vols. 8vo, pp. 574-496.
7. Verbrennung und Athmen, chemische Thätigkeit und organisches Leben. Von Fr. Naase. Bonn, 1846. 8vo, pp. 134.
8. Introductory Lecture to a Course of Military Surgery. By Sir George Ballingall, M.D. Edinburgh, 1846. 8vo, pp. 22.
9. Fragments of Medical Science and Art. An Address. By H. J. Bigelow, M.D. Boston, 1846. 8vo, pp. 34.
10. A Series of Essays on Inflammation and its Varieties. Essay I. The Natural History of the Disease. By Henry Chatterback, M.D. London, 1846. 8vo, pp. 67.
11. Three Reports of the Joint Deputation of the Society of Apothecaries and the National Association. London, 1846. 8vo, pp. 46. 1s.
12. Analysis of the Evidence in Favour of the Constant Supply System. By J. Wicksteed, Esq. London, 1846. 8vo, pp. 46.
13. Inaugural Dissertation on Yellow Fever. By Hugh Bosc, M.D. Bonn, 1846. 8vo, pp. 65.
14. Historical and Critical Remarks on the Operations for the Cure of Cataract. By A. Watson, M.D. Edinburgh, 1846. 8vo, pp. 35.
15. On the Treatment of Strictures of the Urethra. By James Briggs, Surgeon. London, 1846. 8vo, pp. 22. 2s. 6d.
16. On the Effects of Dew as influenced by Protective Clothing and Drainage. By W. A. Guy, M.D. London, 1846. 8vo, pp. 45. 1s.
17. On the Inflammation of the Uterus. By Forbes Winslow, M.D. London, 1846. 8vo, pp. 23.
18. On the Hygienic Effects of Mineral Springs on the System of the Human Body. By E. Prater, M.D. London, 1846. 8vo, pp. 46.
19. Lectures on the Uterus, and on the Pathology, Diagnosis, and Treatment of Uterine Diseases. By J. Rutledge, M.D. Dublin, 1846. 8vo, pp. 46. 2s. 6d.
20. Krimlogik. By K. A. Thom, M.D. Bonn, 1846. 8vo, pp. 35. 1s.
21. Homœopathy, Allopathy, and Physic. By John Forbes, M.D. Philadelphia, 1846. 8vo, pp. 120.
22. The Use of the Body in relation to the Mind. By G. Moore, M.D. London, 1846. 8vo, pp. 431. 9s.
23. The Surgical, Mechanical, and Medical Treatment of the Teeth. By J. R. E. London, 1846. 8vo, pp. 320. 10s.
24. Medical Report of the House of B. and Fever Hospital, Boyle street, Dublin. By G. A. Kennedy, M.D. Dublin, 1846. 8vo, pp. 195.
25. Experimental Researches on the Physiology of Animals, with Remarks on the Food of Man. By R. D. Thomson, M.D. London, 1846. 8vo, pp. 195.
26. Elements of Physics. By C. F. Gauss. Translated from the German by E. West. London, 1846. 8vo.
27. Practical Observations on Mineral Springs and Baths. By E. Lee. London, 1846. 8vo, pp. 134. 4s. 6d.
28. Dr. Hooper's Physician's Vade Mecum. By Dr. Guy. New edition. 1846. 8vo, pp. 164. 6d.
29. A Medical Topography of the County of Wells. By R. H. Powell, M.D. London, 1846. 8vo, pp. 174. 3s. 6d.
30. Clinical Collections and Observations on Surgery. By W. P. Ormerod, Surgeon. London, 1846. 8vo, pp. 312.
31. Fever Physiologically considered. By M. Reed. London, 1846. 8vo, pp. 24.
32. The Antidotal Treatment of Cholera. By R. Parkin, M.D. London, 1846. 8vo, pp. 24.
33. The Microscopical Anatomy of the Human Body. By A. H. Hassall, F.R.S. Parts I and II. 2s. 6d. each.
34. A Practical Treatise on the Diseases of Children. By J. M. Coley, M.D. London, 1846. 8vo, pp. 467. 14s.
35. Notes on the Epidemic Cholera. By A. H. Kennedy. London, 1846. 8vo, pp. 24.
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